Enabling Interdisciplinary Research: Perspectives from Agriculture, Forestry, and Home Economics

Martha Garrett Russell, editor
with
Richard J. Sauer and John M. Barnes,
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Preface

My own interest in interdisciplinary research dates back to my days as a faculty member at Michigan State University. At that institution I was fortunate to be in a department in which interdisciplinary research was rapidly becoming the preferred and, in fact, necessary way of solving significant problems in agricultural science. In the discipline of entomology we finally became painfully aware that any management action taken against an insect pest soon influences the well-being of not only other insect pests but beneficial insects, plant pathogens, and even weeds. Further, the pest management decisions are often made within such dynamic and complex agricultural production systems that economists and sociologists must be involved in the financial, human and environmental cost-benefit analyses.

Many interactions with my colleagues at Michigan State University, during the evolution of their strong programs in IPM (integrated pest management), tempered and sensitized me. It became apparent that the systems approach to the design of integrated agricultural technologies was becoming of increasing importance.

In 1974-75, I spent a year as Principal Entomologist for the Cooperative States Research Service (CSRS) of USDA. During that time, my interaction with CSRS scientists, my review of project and grant proposals, and my several visits to review research programs at state agricultural experiment stations further convinced me of a growing need -- a need to concentrate our research planning and implementation on an interdisciplinary approach involving a team of scientists rather than the traditional one investigator/one project approach.

Actually, we might conceive of three levels of research organization and execution:

1) Disciplinary (Intradisciplinary) - Research conducted by one or more scientists within a single discipline;

2) Multidisciplinary - Research in which scientists from several disciplines are involved but the research is planned, executed and evaluated by each person separately. Management of the various segments of the research is usually provided by each disciplinary unit (department) involved; and

3) Interdisciplinary - Research involving input from several disciplines and with the effort mutually planned, executed, evaluated, conclusions drawn and results disseminated.

Each of these levels of research has a place in a modern research program; however, I would maintain that many of the more complex problems facing us today must be attacked by either a multidisciplinary or an interdisciplinary approach. Further, I would argue that the interdisciplinary approach is sometimes most desirable in large, complex, mission-oriented research programs, such as state agricultural experiment stations. However, it is much more difficult to organize, fund and manage, because disciplinary boundaries often prevent adequate interchange among scientists and make it difficult to maintain resource flexibility.

It is with this orientation and perspective, first as a station scientist and extension specialist, then as a CSRS staff member, an associate director of an agricultural experiment station, an Entomology department head and now as a director of an agricultural experiment station, that has motivated me to explore the issues relative to enabling interdisciplinary research. It led to my bringing Ms. Martha G.

(Marty) Russell into our office for the past 1 1/2 years as an Administrative Fellow, the cooperative agreement the Minnesota Agricultural Experiment Station has had with CSRS, and the investment of considerable resources of the Minnesota Station to survey SAES directors on the need for and the administration of interdisciplinary research. These collective efforts have resulted in this publication.

Interdisciplinary approaches are increasingly being employed in the design of more efficient and effective pest management systems, of cropping systems designed to minimize soil and water loss, of controlled environment plant and animal growth systems, and, in fact, of total agricultural and forestry production and utilization systems. The scientific and technical skills are not available in single disciplinary departments. In fact, it is unlikely that the skills are completely available within our public institutions. We must develop and maintain institutional innovations capable of facilitating effective means of communication and collaboration between public- and private-sector research organizations. Of first priority, however, is the need to improve interdisciplinary communication and research collaboration within our own public institutions. It is hoped that this book will make some small contribution in that regard. It is also hoped that more of our interdisciplinary research will be facilitated and enabled by SAES directors, department heads and other research administrators rather than conducted in spite of us.

Finally, I must acknowledge the support and encouragement of Jack Barnes, and Walt Thomas of CSRS, and the tremendous effort of Marty Russell in carrying out the bulk of this study, analysis, compilation and editing. It would not have been possible without Marty's willingness and ability to do much of the work, as well as to provide a good deal of the creativity and initiative.

Richard J. Sauer
December 3, 1982
St. Paul, Minnesota

My interest in interdisciplinary research dates back to the period when I was still principally in research, with no administrative responsibilities. While at Pennsylvania State University, I was interested in a project concerning the genetic control of the uptake of chemicals. In order to do a complete study involving both basic and applied research, the only way it could be accomplished efficiently was to involve several people with different backgrounds. Accordingly, this project, which was successful in obtaining grant funds for nine and a half years, involved me as a geneticist-plant breeder and two other scientists, a soil chemist and a biochemist. Because no disciplinary boundaries were placed between the three of us, we were very successful in handling the funds, obtaining graduate assistantships, and making some acceptable accomplishments based on the publications we produced. Other inquires, which stemmed from our work, are still being extended by basic researchers today.

As we look at the broad spectrum of research in agriculture, it is apparent that interdisciplinary approaches are being more effectively used today to produce both basic information and also more efficient applied research and development.

As Administrator of CSRS, I appreciate the effort of some of my staff working with Dr. Richard J. Sauer and his staff in developing this study.

Walter I. Thomas, Administrator
Cooperative States Research Service
December 20, 1982
Washington, D.C.

Introduction and Overview

Interdisciplinary activities are not something new. In fact, they have been one of the continuing forces in both the history of knowledge and in the history of agriculture. Even the earliest communities of scholars recognized the importance of collaborative efforts among academicians in developing the philosophy of science. Their collaboration served as a basis for professional development and training. Throughout the ages, people involved in agricultural production have recognized the interrelatedness of natural phenomena. Their practical decisions have reflected their attempts to synthesize the collective impact of each of those phenomenon.

In the past century, the technological revolution has encouraged the acceleration and expansion of the knowledge industry. As state colleges and land grant universities have responded to the demand for scientific approaches to problems of the agricultural community, a cadre of academicians has emerged whose scientific and instructional interests have addressed agricultural issues. These early scientists organized themselves into departments in order to instruct students and train their successors and in order to allow people with similar interests to work together. Departmental consensus on what should be taught and what should be studied grew out of the disciplinary training of those academicians and, in turn, shaped the development of the sciences in agriculture, forestry and home economics. These disciplinary communities and their standards have been essential in the transfer of scientific knowledge into educational programs and into research endeavors.

Knowledge and technologies have also been transferred to social and industrial organizations. New technologies produced by research have changed many aspects of the communities which are concerned with agriculture, forestry and the home environment. New knowledge, also generated by research, has increased our understanding of the complex linkages between the agricultural enterprise and its socio-political, economic, biological and human contexts. This transfer of knowledge and technologies has occurred when scientists have successfully applied disciplined analysis to the important problems and issues identified by the scientific and lay constituencies of agriculture, forestry and home economics endeavors.

In most cases, the important problems do not have easy or perfect solutions. Solutions to critical issues involve trade-offs, and balancing those trade-offs requires both an understanding of the individual aspects of the problem, as well as an understanding of how each aspect relates to all the others. In this balance between the parts and the whole lie the importance of interdisciplinary research, the relevance of the systems approach, and the challenges to scientists and research administrators working within the current structures of the scientific community.

Interdisciplinary efforts in research have provided a mechanism whereby real problems have led the development and application of science. The potential of interdisciplinary research to advance the scientific enterprise is comprised of basically three opportunities. First, interdisciplinary research efforts provide a scientific arena in which the conceptual frameworks, the methodological approaches and the analytical tools of a discipline are challenged. In this confrontation lies a tremendous potential for the refinement and advancement of the disciplines.

A second related opportunity challenges a scientist to defend and expand his or her own repetoire of methodological approaches and analytical tools. In this opportunity lies the potential for the ongoing professional development of scientists.

Thirdly, interdisciplinary research efforts offer to students working with faculty scientists the opportunity to learn scientific approaches to the contradiction of simultaneous phenomena, to the analysis of complexity, and to the integration of multiple criteria. Interdisciplinary research efforts relate disciplined analytical approaches to the wider environment and develop tools to deal with complex, real problems.

These real problems for which interdisciplinary research is well suited are complex because of their interrelatedness. Most of the important problems are, at one level or another, connected. Yet the tradition of reductionism in science has lauded the practice of reducing problems to their simpler components, studying these components as separate entities and then summing those outcomes as explanation of the problem. This categorization of phenomena into smaller and smaller units has narrowed the scope of each field of study and has moved the study of phenomena further and further away from real problems.

The systems approach acknowledges that all objects and events are parts of a larger whole. At all levels of analysis, the systems approach seeks to understand phenomena in terms of the function of each phenomena in the larger system, in terms of how the phenomena relate to each other and in terms of how the system relates to its environment and to other systems in that environment.

The world does not present its problems to scientists in disciplinary form. Disciplinary analysis implies the imposition of boundaries on the understanding of problems. Interdisciplinary analysis builds bridges which cross those boundaries. Only such a holistic perspective, a systems approach, can lead to an understanding which is sufficiently adequate in scope to contribute to real problems.

Dealing with real problems increases the complexity of research problems. This complexity in research problems has heightened the need for skillful administrators to balance the sometimes conflicting interests of scientists and of the institutions in which they work. The demands of today's research enterprise challenge both scientists and administrators to work within the constraints of multiple sponsorship, shifting financial parameters, multi-layered administrative structures, and a changing cultural milieu.

This collection of readings presents strategies and experiences in enabling interdisciplinary research in agriculture, forestry and home economics. The papers, some reprinted here from other sources and some appearing here for the first time, represent an assembly of perspectives from scientists and administrators who work within the USDA research enterprise on the critical aspects of successful interdisciplinary research.

The intent of this book is to raise the administrative and intellectual issues of interdisciplinary research in agriculture, forestry and home economics and to present perspectives on how those issues can be addressed in order to harness the use of interdisciplinary research as a scientific tool. Some of these perspectives contradict each other. Some of the key factors in enabling interdisciplinary research will differ according to the size, sponsorship and focus of the interdisciplinary research team. Our goal in assembling these perspectives has been to identify the issues and, in doing so, to stimulate critical discussion of these issues among scientists, administrators and sponsors.

The identification and discussion of these issues is an important step in influencing the direction of policies which will enable interdisciplinary efforts to guide research planning, as well as in influencing the implementation of interdisciplinary activities. In this respect, this collection is intended both as an acknowledgement of the significant contributions which have already been made to interdisciplinary research management by members of the agricultural research community and also as a point of departure for those who seek ways to further enable interdisciplinary research.

Interdisciplinary research is, by its nature, elusive and continually changing. Yet understanding what it is and how it influences the relationships of disciplines to each other is essential in order to develop strategies for using this valuable tool. Several papers present conceptual frameworks which are helpful in understanding the organization of the sciences and the definition of research problems as they influence the conduct of interdisciplinary research.

Other papers describe the influence of the organizational setting on interdisciplinary activities, identify the variable components of such institutional structures, and suggest ways to deal with those structures in order to support and encourage collaborative research. In addition to institutional structures, the nature of the research task in terms of its complexity, its urgency, and its uncertainty - is explored as it relates to teamwork in research. And lastly, characteristics of interdisciplinary research teams are discussed with respect to the influence of leadership, communication, cooperation, interpersonal conflict, and team coordination on research activities. The approaches to these topics represent the views of scientists and of administrators at a variety of organizational levels. The scope of issues in this collection encompasses both the single researcher and the large-scale coordinated regional research effort, as well as several points in between.

The papers in this book are grouped into four chapters. The first chapter includes three papers which present "Theoretical Perspectives" on the nature of issues involved in interdisciplinary research. The second chapter, "Challenges," consists of papers which describe the various problems and constraints which confront scientists and administrators in planning and implementing interdisciplinary research efforts. "Leadership," the third chapter, is made up of papers which describe examples of successful interdisciplinary research programs which have been conducted in agriculture, forestry and home economics. The final chapter, "Potentials," concludes with papers which suggest future directions in which interdisciplinary research endeavors can serve a valuable role.

THEORETICAL PERSPECTIVES

A great deal of effort has gone into defining the differences between the intellectual and administrative units of science encompassed by agriculture, forestry and home economics in land grant universities and state colleges. The ways in which these distinctions have been made has, to a large extent, determined the orientation of disciplinary activites and the perspectives on interdisciplinary collaboration.

The first paper in this chapter, "Administrative Dimensions of Collaboration in Interdisciplinary Research," discusses how collaboration between scientists has been influenced by the sponsorship of research by agencies external to academic institutions and by the development of subdisciplinary specializations within the academic environment. A three-dimensional framework for identifying and understanding the nature of collaboration and interdisciplinary activity is presented. These dimensions include the personnel, the disciplinary and the administrative aspects of research activities. By addressing the dimension-specific issues of funding, equipment and facilities, leadership, teamwork, conflict, recognition, and promotion, Russell explores ways in which the services of research administrators can be effectively and efficiently targeted to their points of greatest impact.

Alternative ways of implementing collaboration in interdisciplinary research groups are presented in the next paper, "Working with Other Disciplines". This paper discusses variations of the research process by which scientists from different fields have worked together. Drawing specific examples from the involvement of agricultural economists with other scientists in social science fields, in natural science fields and in engineering, Swanson describes the organizational frameworks and leadership styles which have structured and directed such efforts.

The process of scientific inquiry takes many different forms. In the best of all worlds, each research activity is tailored to the objectives of the proposed inquiry and is implemented within an organizational format and with a style of leadership which encourages that process. In reality, substantial effort, together with some compromise, is required to achieve and maintain interdisciplinary collaboration in research projects. The theoretical insights presented in Swanson's paper offer insights into the ways in which interdisciplinary research collaboration can be encouraged by providing leadership and organizational structures which are consistent with the process and goals of research.

Another perspective on the research process is presented by Dillon in "The Economics of Systems Research". The systems approach to research encompasses aspects of interdisciplinary research which emphasize the interrelatedness of phenomena and the necessity of studying phenomena in their related contexts. The systems approach goes beyond this to include concepts of hierarchical levels at which organized phenomena can be studied and concepts of feedback in maintaining equilibrium between elements of the system. Systems research offers advantages for purposeful research management especially when choosing between research alternatives and in facilitating goal achievement. Dillon argues that professional training, research organization and research directions in agricultural sciences can be better targeted and implemented if they are based on an understanding of agricultural systems.

CHALLENGES

If interdisciplinary research were easily accomplished, it would probably be the prevalent mode of research. But given the organization of science, complexities of research funding, and the professionalization of scientists that do exist, interdisciplinary research is accomplished only with great commitment and much effort. The papers in the second section, "Challenges," identify some of the major constraints facing the conduct of interdisciplinary research.

One of the major limitations on the research which can be performed and on how it can be conducted is the limitation of resources. "The Challenges of Interdisciplinary Research" points out that the key to successful research with limited resources is the efficient use of those resources. Team research, the authors contend, offers some solutions to maximizing research resources but also presents management challenges. An overview of these management challenges in the context of the research programs of the state agricultural experiment stations is presented by Francis et al. Examples for interdisciplinary research opportunities in crop improvement and cropping systems are used to illustrate the needs for overcoming these barriers.

Another limitation is the manner in which problems for research are identified. Problem choice is a major determinant of the outcomes of research. Busch and Lacy report on a national study of the personal, professional and social factors which influence how agricultural scientists identify their research problems and conduct their research. The interaction of scientists with professionals and students from other fields, patterns of publishing scientific results, and the timing of research activities tend to be different for scientists from different fields. These differences pose significant challenges to administrators' abilities to help scientists from different disciplines collaborate in focusing their skills on a specific problem.

In "Problem Choice in Agricultural Research: Scientists' Initiatives", they report that agricultural scientists' disciplinary backgrounds are associated with several major differences in how they choose research problems. The authors conclude that disciplinary standards are, for many scientists, more important than organizational standards in guiding research. To the extent that scientists anticipate being rewarded for the research they conduct, the relative importance of disciplinary and organizational standards may influence the problems on which scientists choose to work. The ability and willingness of scientists to select research problems which reflect priorities in the nation's agricultural system are critical to the agricultural research enterprise's continued capability to contribute solutions to these concerns.

The importance of incentives and the balance between incentives and disincentives are key issues in how scientists allocate their time and resources. Waibel describes the pressures of converging demands and conflicting interests and how these influence the workload and professional development of the individual scientist. "The Impact of Interdisciplinary Research on the Research Scientist" explores the challenges of interdisciplinary research for the faculty member and offers suggestions for easing the time consuming technical aspects of research so that the efficiency and effectiveness of scientists' efforts in both disciplinary and interdisciplinary research may be assured.

The ability to develop and mobilize scientific expertise depends also upon "The Maintenance of Professional Integrity in Interdisciplinary Research", according to Mitchell. Professional integrity is developed within a departmental framework which provides security, resources, rewards and stimulus. Disciplines -- the patterns of inquiry around which departments are focused -- are organic in the sense that they are continually changing, evolving, and developing. This organic nature of a discipline means that it will be further enhanced by new experiences and exposures that invigorate and transform it. Mitchell argues that interdisciplinary activities are important in invigorating the discipline and addresses the critical importance of the disciplinary integrity of scientists who participate in interdisciplinary research endeavors.

The department head plays a central role in promoting and bridging these disciplinary distinctions. Sauer discusses the two types of administrative issues which confront department heads in administering the research conducted by faculty in their departments. The research challenges involve providing the intellectual leadership on which disciplinary excellence is based. The management challenges encompass motivating scientists to work on important problems and providing incentives and rewards to do so. "The Role of the Department Head in Interdisciplinary Research" offers suggestions for ways in which department heads can individually and collectively, through their state experiment stations, implement incentives for interdisciplinary collaboration.

The challenge of rewarding faculty for successful research efforts is addressed by McFarland in "Administration and Funding of Interdisciplinary Consumer Research". Departmental goals and faculty reward systems which are oriented toward departmental activities and which are influenced by departmental colleagues often impede interdisciplinary efforts in research. This paper argues that administrative leadership can provide positive reinforcement for participation in interdisciplinary activities and explores ways in which this leadership can be incorporated into departmental objectives.

One such challenge lies in securing funding for interdisciplinary research efforts. McFarland addresses several important issues in providing funding and stresses that searches for interdisciplinary research support require institutional, unit administrator and researcher cooperation. The importance of maintaining continuing contact with funding sources and of encouraging creative approaches to funding requests are emphasized.

Issues of funding interdisciplinary research are addressed from another perspective by Aldrich in "Budgeting for Interdisciplinary Research". The high costs of equipment, the extensive proportion of research budgets which are committed on a continuing basis to salaries, and the impacts of funding cutbacks have all influenced the budgetary context of interdisciplinary research. This paper describes how interdisciplinary research is administered in one experiment station and highlights the importance of the Director's office, of human resources, and of accountability in addressing the budgetary challenges of collaborative research efforts.

Although departmental and station influences on the administration and budgeting of interdisciplinary research are critical, these influences are strongly determined by an institution's posture and policies toward interdisciplinary endeavors. "Institutional Policy and Operational Issues Affecting Interdisciplinary Research" discusses the important role of institutional policies for personnel, resources and organizational flexibility in establishing an interdisciplinary milieu. Boger and Boyd describe several organizational approaches and operational assumptions which are essential to stimulating innovation in research activities. The congruence of policies which permit interdepartmental flexibility and the consistency of incentives for collaborative endeavors are among the essential components of an interdisciplinary milieu which encourages the cooperation and communication necessary for interdisciplinary research.

Communication is, indeed, one of the keys to developing a cohesive working team from a group of people with different professional perspectives. This is especially true in international research and is critical in international research which is interdisciplinary. "Communication in International Research Management" draws examples from a Cowpea CRSP program to discuss aspects of communication in international interdisciplinary research. Developing a common team language is the basis for collaborative communication and involves bridging cultural norms, personalities and disciplinary backgrounds. Lodwick, Axinn and Barnes-McConnell discuss the challenges of international research and describe the role of a Management Office in facilitating the bridge-building activities which are essential to communication and constructive collaboration.

All of these challenges confront the administration, the institutions, and the scientists which are currently contributing to interdisciplinary research on agriculture, forestry, and home economics. Another aspect of meeting research challenges which requires collaborative, interdisciplinary activities is that of preparing tomorrow's researchers. Barker's paper describes several aspects of the interdisciplinary training needed to prepare tomorrow's scientists for research in farming systems. In its initial stages, farming systems (as one type of interdisciplinary endeavor) has systems perspective on the farm and farm family. "Farming Systems Research: Interdisciplinary Responses to Problems" describes several approaches to training the next generation of scientists in the perspectives and skills necessary to participate in interdisciplinary endeavors. Essential to this training are the consideration of both farm and non-farm activities and also dialogues with the farmer or farm family on issues which reflect the contexts in which solutions are to be applied.

LEADERSHIP

To be successful, interdisciplinary research must be championed at all levels of an enterprise, including scientists, department heads, research administrations, institutions, and sponsors. This chapter presents examples of leadership in initiating, organizing and implementing interdisciplinary research at all of these levels. Just as collaborative research efforts require the commitment and participation of all collaborators, an overall research enterprise requires leadership and cooperation from all levels in order to achieve balance in the direction and scope of interdisciplinary research activities. The special circumstances which characterize interdisciplinary activities discourage across-the-board prescriptions in how such leadership should be channeled. However, successful endeavors, on a case study basis, do provide indicators of the attributes of successful programs and of the ways in which those components were brought together to enable successful interdisciplinary research efforts.

The leadership of experiment station directors is essential in maintaining steady funding for research programs, in cultivating communication about research needs and outcomes, in promoting the transfer of new technologies and solutions generated by research, and in maintaining interfacing mechanisms between cooperating sectors. "Creating Administrative Environments for Interdisciplinary Research" reports the perspectives of state experiment station directors in enabling interdisciplinary research in their stations. Initiatives for scientists, for departments, for stations, and for institutions can be influenced by directors. Russell and Sauer address the roles of experiment station directors in providing leadership, both directly through policies and initiatives and indirectly through influences on the administrative environment.

Leadership in problem definition and research implementation can also encompass the sponsors of the research programs. "A New Approach for Planning and Coordination of a Large Project" describes an approach used to plan a large-scale research and development program sponsored by USDA, the Adapted Convergence Technique for Agricultural Research. This technique was used to plan the Combined Forest Pest R&D program. Using a series of flows and arrays to structure several phases and subphases toward specified program objectives within a given time frame, this technique provided a tool for the planning and coordination of four Federal agencies in USDA, many state agricultural experiment stations, universities and colleges, and several state forestry organizations. Shea and Bayley describe how program funding and implementation, program management, progress reviews and adjustments, results dissemination, and public support were based on this planning technique.

In a similar vein, Oldenstadt et al. describe the specific research objectives and results of another large-scale interdisciplinary research program. STEEP (Solutions to Environmental and Economic Problems) is a multidisciplinary research effort to develop new techniques and strategies to control soil erosion on the croplands of Washington, Oregon and Idaho. A USDA grant to the experiment stations in those states, with supplementary funding from state and federal sources, brought together scientists from several states and several disciplines and illustrates one model for organizing and mobilizing scientific resources. "Solutions to Environmental and Economic Problems (STEEP)" suggests how this model might be applicable to other regions and to other problems. The contributions of this project to both identified problems and to contributing disciplines illustrates the multiple payoffs of collaborative research activities.

Another perspective on regional collaboration is presented in "Regional Coordination of Scientists' Initiatives in Interdisciplinary Research", using corn virus as an example. Scientists specializing in corn virology, genetics/breeding, agronomy and entomology collaborated in multidisciplinary and interdisciplinary stages of corn virus research, as the problems were identified, clarified, researched, further clarified and further researched. Barnes describes how communication and coordination between practitioners and scientists, between scientists and administrators, and among scientists made the accomplishments of this endeavor possible.

The benefits from interdisciplinary research extend beyond problem identification and the conduct of research to include also the evaluation of alternative practices to be implemented. "Interdisciplinary Research for Multiresource Forest Management" describes ECOSIM, an interdisciplinary integration of techniques using computer resources to predict multiresource outputs from forests under alternative forest management practices and strategies. The interdisciplinary team approach has been critical to forest management in research planning and selection of study areas, in developing multiresource inventory methodology, and in analyzing and interpreting research findings. Benefits of the program have included identifying and evaluating the complex tradeoffs involved in both the individual and the combined impacts of changes in forest management. Garrett describes the data management methods used by ECOSIM, as one example of the opportunities for collaborative research which can be developed using computerized data management systems.

POTENTIALS

The essential resource for research is the scientist. But some environments and some equipment technologies allow scientists to more fully utilize their skills and insights. Significant contributions have already been made, yet the full potential of the agricultural research system has not yet been realized. The last chapter, Potentials, includes several papers which describe how that potential can be developed.

One potential for the agricultural research enterprise lies in the training and preparation of tomorrow's scientists. The quality of the best researchers is most fully utilized, according to Smith, when it is directed toward performing quality research and toward preparing future researchers. New knowledge and new graduates who, through the process of education have mastered that knowledge, are the best manifestations of quality faculty. "Systems Research Provides New Knowledge about Agriculture" describes the potential of systems research for scientific inquiry and for training scientists. Systems research requires administrative sanctions which allow experienced disciplinary scientists to work as members of a team and also to maintain disciplinary identity and recognition. Both systems research and the computation capacity of computers which can be harnessed to study complex agricultural systems offer new horizons to agricultural research. The essential influence in permitting the fullest use of these opportunities lies in a change of attitudes among administrators and scientists, a change which acknowledges the importance of systems research.

Fitzhugh and Byington discuss several different types of models used in systems research, with particular reference to animal agriculture. "Systems Approach to Animal Agriculture" describes several advantages to using modeling approaches: that decisions produced from the systems approach are based on more rational grounds; that critics of the decision can pinpoint areas of concern; that new information can be more easily incorporated into new models; that the consequences of decisions can be quickly re-evaluated; and that decision makers become more accoutable for their actions. A system-based model serves as a frame of reference for participating scientists, helping to bridge communication barriers which can hinder interdisciplinary research activities and derail efforts to contribute to practical decision making.

Interdisciplinary research also holds great potential for guiding the planning of research programs and the ongoing refinement of disciplines. The rationales which underlie interdisciplinary efforts, the criteria for its effectiveness, and the involvement of multiple constituencies in its implementation serve both to maintain disciplinary rigor and to guide interdisciplinary relevance. The evaluation of collaborative research activities is difficult but essential. "Evaluating Interdisciplinary Research" argues that the evaluation of interdisciplinary research is maximized when both disciplinary and interdisciplinary affiliations and communication are strong for researchers, as well as for reviewers. Russell identifies the need for planning and evaluation to occur in tandem and describes the potential for the evaluation of interdisciplinary endeavors to both screen and strengthen research activities.

SELECTED BIBLIOGRAPHY

The papers presented in this collection were chosen because they provide an overview to issues important for interdisciplinary research efforts in agriculture, forestry and home economics. The land grant tradition has provided an organizational context in which opportunities for interdisciplinary collaboration have been encouraged by the mission orientation of the USDA and by some state experiment stations. The successes realized by these efforts have established a foundation of administrative and intellectual know-how on which future interdisciplinary efforts can be built. These successes provide lessons for the agricultural research community, as well as for the scientific community in its broadest sense.

In the same manner, the ways in which interdisciplinary research has been addressed by other fields of science, other agencies and other research enterprises offer insights into enabling interdisciplinary collaboration in agriculture, forestry and home economics. Efforts to define the issues and describe the approaches used by these other partners in science are presented in abbreviated form in the "Selected Bibliography". So that readers of this book might benefit from those insights, annotations on those readings have been included.

ACKNOWLEDGEMENTS

The compilation of this collection was itself an interdisciplinary effort, as a quick glance at the backgrounds of the contributing authors will reveal. The issues presented here are drawn from training and experience in fields of agricultural economics, agronomy, plant pathology, consumerism, entomology, family studies, animal sciences, sociology, forestry, home economics, agricultural engineering, international studies, plant genetics, and soil science.

The editors, too, represent an interdisciplinary team. As such we dealt with the issues of defining objectives, establishing a common language base, leadership and acknowledgement, funding, and criteria for evaluation. As an interdisciplinary effort, this book's maximum potential will be realized if this effort both invigorates the disciplinary components and encourages the interdisciplinary components of the agricultural research enterprise.

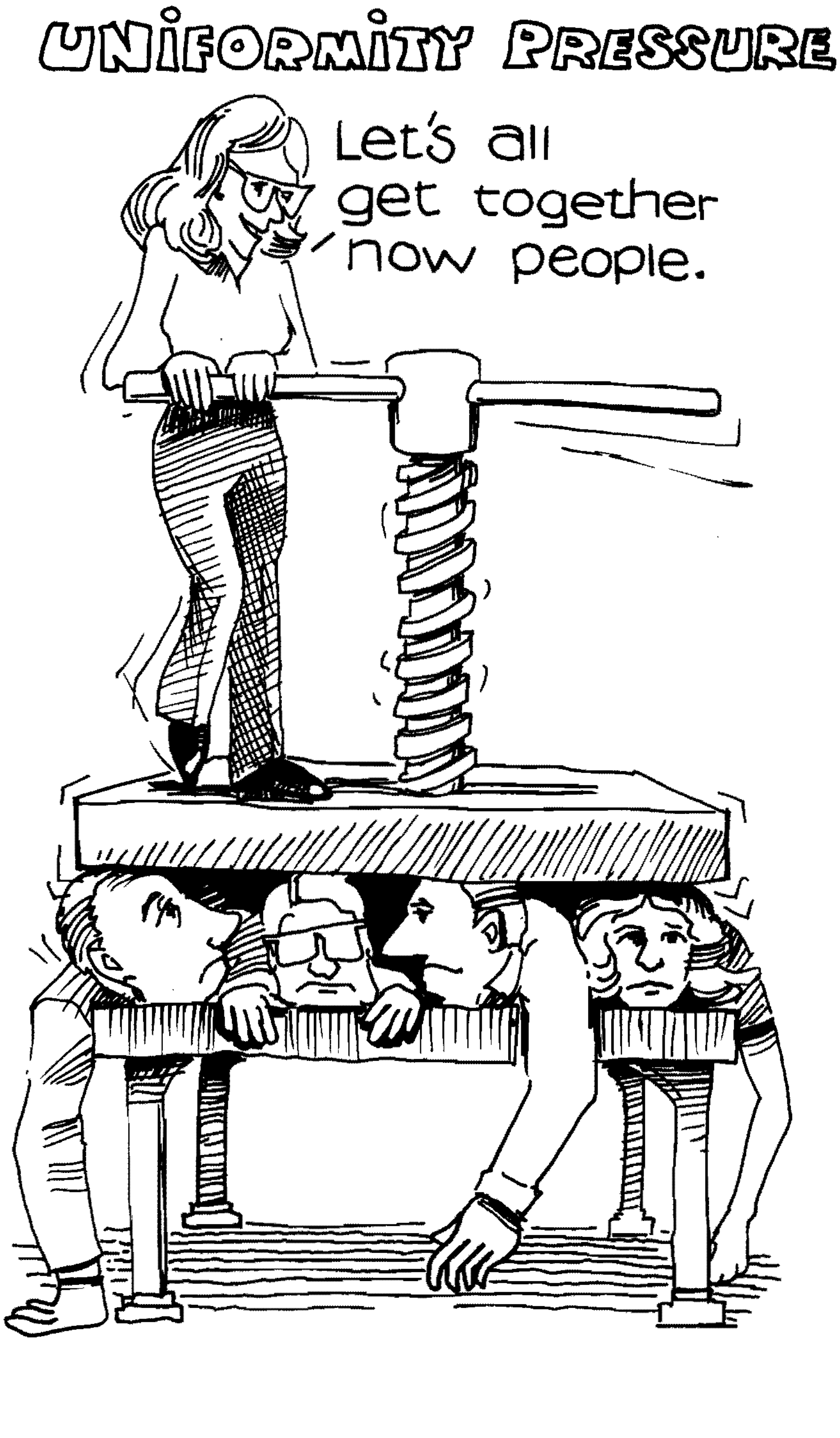
Many people contributed to this effort. The fiscal support provided by both the Cooperative States Research Service, USDA, and the University of Minnesota Agricultural Experiment Station was essential in providing resources, as well as in acknowledging the value of the effort. Susan Fugate, Technical Information Specialist, CSRS, provided significant assistance in locating bibliographic information in early stages of the book's development. John Cornwell, Research Assistant at the Minnesota Agricultural Experiment Station, contributed greatly to the preparation of the selected bibliography and to the coordination of communication with contributing authors. The enormous effort of Kathy Donahue, who typed the manuscript in its preliminary and final forms and who maintained the priority of completing the manuscript amidst other demands on the secretarial staff in the Minnesota Experiment Station office, is greatly appreciated.

Many thanks go to Paul Waibel, Professor of Animal Science and Administrative Intern, Minnesota Agricultural Experiment Station, who proofed the manuscript and offered helpful suggestions. Sam Brungardt, Editor, Communication Resources, Minnesota Agricultural Experiment Station contributed to the layout of the book; and Rick Bosak, Graphic Designer, provided artistic consultation and services. Thanks also to David Hills, Assistant Professor of Psychology, North Carolina State University, for the cartoons; these cartoons document a workshop on Creativity in Research Teams held at the Center for Creative Leadership, Greensboro, North Carolina, March 1982.

The collection would not have been possible without the concern, the commitment and the contributions by the authors of the papers included. The final acknowledgement, however, goes to those readers who will carry on the effort through their own activities, in cooperation with others.

Martha Garrett Russell
Minnesota Agricultural Experiment Station

Theoretical Perspectives



Administrative Dimensions of Collaboration in IDR*

Martha Garrett Russell

The personnel, disciplinary, and administrative aspects of research provide a three-dimensional framework for identifying and understanding the nature of collaboration and interdisciplinary activity. This paper discusses the issues of funding, recognition, promotion, equipment and facilities, leadership, teamwork, and conflict which are specific to each dimension. It also points out how the services of research administrators can be more effectively and efficiently targeted to their points of greatest impact by considering the dimension-specific concerns of these issues.

The application of research findings to agricultural production has influenced the experimental process. Prior to academia's embracing the experimental process, research activities were valued for their role in exercizing the reasoned intellect. Experimentation was supported by the scientist's own financial resources or entrepeneurial talents, and was encouraged by the mutual interests of other academicians (Brubacher and Rudy, 1968).

As state universities and land grant colleges implemented the mandates of the Morrill Act, a cadre of academicians emerged whose scientific and instructional interests addressed agricultural issues. These early scientists organized themselves into departments in order to instruct students and train their successors. Departmental consensus on what should be taught grew out of the disciplinary training of these academicians and, in turn, shaped the development of the agricultural sciences.

During the late 19th century, the government's legislated support of research in the national interests (Brubacher and Rudy, 1968), industry's utilization of technologies produced by research in academic settings (Ashby, 1974), and the labor market demand for trained scientists who could address the needs of an ever increasingly technological world (Weinberg, 1967) all encouraged the development of academic research. The agricultural sciences were no exception.

This expansion of agricultural research has been strongly influenced by two forces -- the sponsorship of research by agencies external to the academic institution and the development of subdisciplinary specialization. Both of these forces have affected how scientists have collaborated to address the complex issues of agriculture. This paper explores the administrative issues which have become critical in managing interdisciplinary collaboration in the state agricultural experiment stations (SAES's).

Russell cites the increasing subdisciplinary specialization of scientific communities as one factor which has raised the consciousness about interdisciplinary research in academic institutions (Russell, 1982). As the need for research expertise to address complex issues has grown, so also has the pool of knowledge on which such inquires are based. The ability of any one scientist to fulfill professional responsibilities, continue investigations, and maintain currency with professional associates is limited by human constraints.

In order to stay at the research front of an ever expanding pool of knowledge, a scientist's share of the total shoreline has decreased. In order to maintain the structure of scientific

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inquiry and its reliance on peer validation for accountability, the specialization required to stay at the research front has limited the concerns, the perspectives, the paradigms, the methods and the theoretical frameworks of the succeeding subdisciplines. This specialization has been accomplished through affiliations with subdisciplinary communities.

Collaborative efforts among such specialists have been strategic in the conduct of agricultural research, as well as in research in the larger scientific community. Scientific collaboration has been recognized as the philosophical underpinning of the earliest community of scholars (Gusdorf, 1977). In fact, historical documentation shows collaboration as the basis for the professional development of both disciplines and scientists (Beaver and Rosen, 1977).

In addition, collaboration among scientists has helped to provide dialogue and cooperation between scientists with different disciplinary training. Such interdisciplinary collaboration serves the strategic roles of both redefining the foci of disciplinary or departmental efforts and of forming the embryonic stages of subdisciplines and new disciplines (Cotterrell, 1979; Russell, 1981).

Russell also acknowledges the influence of external sponsorship for research in raising the stature of interdisciplinary research in the eyes of today's research administrators. Sponsoring agencies, faced with accountability for what their money buys, tend to make funding decisions based on expected payoffs to science and to the sponsor. To achieve their objectives, money is spent on research efforts which contribute to the solution of the sponsor's identified goals. Interdisciplinary research efforts are able to accommodate sponsors' objectives.

Trends in the external sponsorship of research efforts have influenced collaborative efforts at universities. In seeking to fulfill their many missions (Carnegie, 1973; Ford Foundation, 1978; Trivett, 1976), today's universities are responding to society's increasing appetite for technological applications to research conducted by their scientists. External support for research endeavors has become essential because of the sophistication and expense of technology and information developed through scientific advances (Kidd, 1959).

In agriculture, home economics and forestry research, these sponsorship trends have had an even longer history, reaching back to the Morrill Act, the McIntire-Stennis Act, the Adams Act, the Federal-Grant Programs in Marketing Research, and the Bankhead-Jones Act. The pressing concerns toward which these funds have been directed have been broader than the expertise of any one scientist, discipline or department. Collaboration -- between scientists,- between disciplines, and between administrative units -- has been and continues to be essential to addressing those concerns. This is especially true in the problems toward which agriculture, forestry and home economics research is directed.

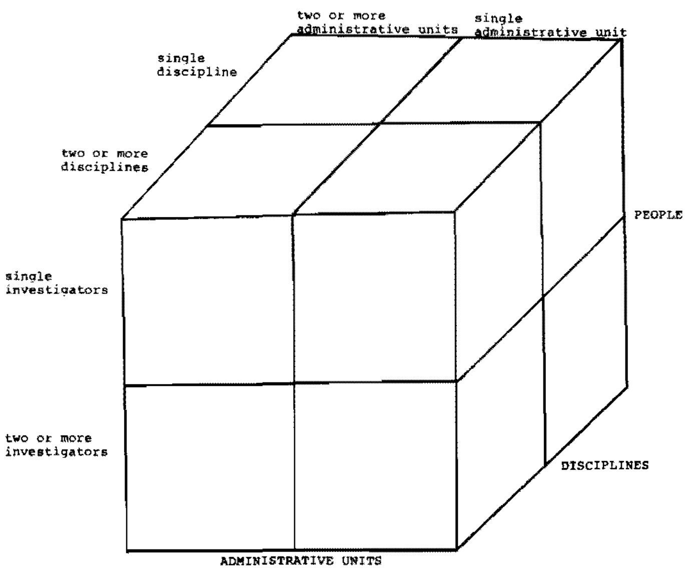
The role of interdisciplinary research in the solution of complex problems confronting society today has been widely acknowledged (Birnbaum, 1981a; Cotterrell, 1979; Gusdorf, 1977; Sinaceur, 1977). Interdisciplinary efforts have been recognized for their potential to deal with multi-faceted problems and to generate proposed solutions which satisfy multiple criteria. However, the challenges in bringing about scientific collaboration at universities have also been repeatedly lamented.

Both scientists and research administrators have been encouraged by successful collaborative ventures and frustrated by others which did not meet expectations. Studies have attempted to identify the characteristics of successful interdisciplinary research teams (Birnbaum, 1981-Academic; Rossini and Porter, 1981). And several studies have discussed the obstacles to such collaboration (Ikenberry and Freedman, 1972; Saxberg, Newell and Mar, 1977). But given the interest in interdisciplinary collaboration as a tool to solve problems through research, it is necessary to identify administrative strategies which facilitate the use of this tool. Critical to such recommendations is an understanding of both interdisciplinary research and of collaboration.

Inevitably when interdisciplinary efforts are discussed, the question is raised, what is interdisciplinarity? This question comes with its sequel. What is a discipline? Two definitions of "discipline" represent the range of perspectives. Thomas Kuhn defines a discipline in terms of its paradigms -- its parameters for recognizing, asking and answering questions (Kuhn, 1970). Rustum Roy, on the other hand, defines a discipline as a body of knowledge that is taught by 25 or more departments across the country (Roy, 1977). By definition, interdisciplinarity is between the disciplines, however they are defined.

Sometimes interdisciplinary activity is collaborative activity and involves the efforts of two or more scientists. Sometimes interdisciplinary activity involves the joint efforts of scientists from two or more departments. Sometimes the term refers to efforts which blend components of two or more administrative units. These dimensions, when integrated, form a three-dimensional framework for understanding collaboration and interdisciplinary activity -- the people, the disciplines and the administrative units.

Figure 1. Dimensions of Collaboration.



The People

The investigators are, of course, primary in importance. Discussions about interdisciplinary activity usually assume more than one investigator. This assumption stems from the tradition of disciplinary training for scientists. Students receive their scientific training and usually complete their research apprenticeships within a single discipline. New scientists earn their academic reputations as individuals with reference to a scientific community. Thus, it is assumed that one scientist represents one discipline.

By the same token, faculty are held accountable for the research they conduct and for their own development along disciplinary lines. In addition, they are responsible for educating students and certificating that education. These degrees and credentials are awarded through departments and programs comprised of disciplinary components. Criteria for judging merit and distributing awards to academicians center around the ability to make substantial academic contributions to a particular discipline (Lewis, 1979) -- both through expanding its knowledge base and also through attracting support for its research efforts (Orlans, 1968). This is the people dimension of collaboration in interdisciplinary activity. Collaborative efforts involve more than one scientist.

However, interdisciplinary efforts may be either collaborative or individual. Not all investigators restrict their scientific inquiry to one disciplinary approach. Some scientists, indeed it has been argued the better scientists (Ziman, 1980), enlarge their agenda of research questions, their research approaches, their visions of issues. Through interaction with scientists trained in other disciplines, through journal study and through self-study, they expand their disciplinary perspectives. They then incorporate these new scientific strategies (paradigms) into their research approaches.

Whether one scientist or more than one scientist is involved in a research effort has significant implications for the conduct and administration of that research. Cooperation, competition, motivation, leadership, power, teamwork, division of responsibility, and allocation of rewards are all important considerations which are influenced by the number of scientists involved in a research effort.

Disciplinary Dimension

Internal research funds are typically allocated to scientists on the basis of their abilities to make contributions to their respective disciplines. External support for research is distributed, on the other hand, according to scientists' abilities to make substantial contributions to the concerns of the sponsor. When these concerns reflect problems which are complex, the expertise from several disciplines is frequently required. This is the second dimension along which collaborative activities may be classified -- the number of disciplines involved. The critical issues in research which are influenced by the disciplinary dimension are those issues which involve the scientific community or subcommunities.

Styles of interdisciplinary collaboration have been defined using attributes of disciplinary paradigms, disciplinary literature bases, disciplinary methodologies and group identity as well as goals, methods and outcomes. (Birnbaum, 1981-Contingencies; Nagi and Corwin, 1972). In addition, Jantsch and others have developed definitional categories of disciplinary collaboration which are differentiated by the scientific methods which are used, the questions which are asked, the extent and characteristics of collaboration, and the agreement on project goals among the collaborators. These perspectives view collaboration along a continuum of disciplinary autonomy and integration, ranging from mono-disciplinary, cross-disciplinary, multi-disciplinary, interdisciplinary, and trans-disciplinary to pluridisciplinary.

Typically, scientists' research questions and avenues of inquiry follow the lines established by their training. Professional development and contact with colleagues refine and expand these established approaches. Most contributions to new knowledge demonstrate the usefulness of those refinements in addressing questions or in identifying new questions for which the approach may be instrumental. The exceptional contribution identifies new tools (concrete and/or abstract) with which questions can be either asked or answered (Price, 1982). All of these contributions are legitimized by disciplinary approval.

The recommendation by scientific peers or disciplinary representatives that a project be funded is another type of disciplinary influence. These peers are best prepared to give advice on the scientific quality of a proposed activity when that activity falls within the disciplinary jurisdiction of the scientific community (the discipline) which is represented by those peers. Identifying peers to review interdisciplinary activities depends either on the availability of individuals who have themselves established expertise in several disciplines or on the availability of a group of experts who are willing to fully acknowledge the legitimacy of each other's expertise (Russell, 1981).

Scientific peers also play important roles in encouraging or discouraging the identification of new research efforts, in acknowledging or ignoring contributions to new knowledge, and in accepting or denying new members. In these roles, the disciplinary group strongly influences what its members will study (Ziman, 1980). The criteria established for quality by the disciplines are "what make the disciplines disciplined" (Boulding, 1977). One can easily understand that such standards are maintained by adhering to them rather than by making exceptions.

Publication is the means by which research is evaluated. It signifies that the scientific community of peers has judged the work acceptable by their standards. These disciplinary standards are based on consensus regarding what questions, methods and interpretations are deserving of the attention of that scientific community. Again, the academic respectability of a discipline is based on maintaining those standards rather than on making exceptions to them.

The central issues for collaborative research efforts involving more than one disciplines, then, reside in legitimizing the questions, the methods and the interpretations which make up the activity - on the review by peers (Russell, 1981). Science is based on this type of critique. Recommendations for project funding and allocation of rewards, as well as decisions regarding professional recognition and quality control of scientific inquiry, are all influenced by the number of disciplinary perspectives involved in the activity.

Administrative Dimension

The third dimension of collaboration which is critical for both disciplinary and interdisciplinary efforts is the number of administrative units involved in the project. These administrative units can be construed as the number of departments, the number of institutions, or the number of countries, etc. Just as scientific communities are the professional and the ideological homes of scientists, administrative units are the financial and physical homes. These administrative units are accountable for the funds, both internal and external, allocated in support of a scientist's research efforts.

Two primary considerations in this dimension are: how many funding/administrative units are involved with the overall activity? And how many separate facilities/locations are a part of the total effort?

This is the dimension of administrative accountability. Firstly, administrative units must account for research funds. Administrative units are accountable for ownership of equipment purchased for the research activity and appropriate disbursement of funds for personnel and supplies. Secondly, they are also responsible for the products of research conducted under their funding jurisdiction, especially if these products include intellectual property and potentially patentable innovations. Thirdly, administrative units are the sources of institutional recognition, incentives and rewards.

These responsibilities held by administrative units impact directly on the collaboration of scientists and on the collaboration of disciplines. Policies regarding indirect costs, consultative arrangements, and reciprocity between units reflect the value of research activities and of collaboration to the administrative unit (Currie, 1978). Such policies determine the incentives or lack of incentives for collaboration (Ikenberry & Freedman, 1972).

Table 1. Eight Types of Research Activities.

	Personnel Dimension	Disciplinary Dimension	Administrative Dimension
1.	One scientist	One discipline	One administrative unit
2.	One scientist	One discipline	More than one administrative unit
3.	One scientist	More than one discipline	One administrative unit
4.	One scientist	More than one discipline	More than one administrative unit
5.	More than one scientist	One discipline	One administrative unit
6.	More than one scientist	One discipline	More than one administrative unit
7.	More than one scientist	More than one discipline	One administrative unit
8.	More than one scientist	More than one discipline	More than one administrative unit

They influence economic and noneconomic costs to department heads of permitting and encouraging their faculty members to participate in collaborative endeavors.

Administrative policies provide the basic framework of the research environment within which the research activities are conducted. Unit goals and objectives provide the benchmarks against which the importance and relevancy of scientists' efforts are evaluated. Collaboration between scientists from different administrative units is influenced not only by the policies of their individual units, but also by the policies of the administrative unit to which those individual units report. Policies which dissuade collaboration between scientists from different administrative units, at any level of the organization, constrain the effectiveness of incentives at other levels.

In summary, the classification of research according to personnel, discipline and administrative unit provides a three-dimensional framework for identifying and understanding the nature of collaboration and interdisciplinary activity (See Table 1), and the issues which characterize each dimension are specific to each dimension (See Table 2).

Examples

For example, a sociologist, a physician and a social worker who study family stress under the sponsorship of funds administered through the family health department in the medical school, the family studies department in the college of home economics, and the school of social work (McCubbin, 1981) will inevitably confront issues of project ownership. The administrative considerations for this project are those #8 (Table 1) - several scientists from several disciplines working through several administrative units. Some of these considerations include:

- How will change in research priorities of the various units influence the overall thrust of this research project?

- Through which unit will the various expenses be paid?

Table 2. Administrative Issues by Dimension of Collaboration and Interdisciplinary Activity.

Personnel Dimension	Disciplinary Dimension	Administrative Dimension
Rewards and incentives	Acknowledging new knowledge	Accountability for funds
Communication	Admitting new scientists	Ownership of facilities and equipment
Division of responsibility	Professional recognition	Appointment of scientists
Interpersonal conflict	Maintaining standards	Institutional recognition, rewards and incentives
Problem focus	Legitimizing the questions, methods, interpretations	Unit goals-planning and prioritizing
Teamwork	Peer approval	Unit prestige
Motivation		Research environment
Leadership		Accountability for intellectual property and patents

•How will contributions of clerical time and physical facilities be apportioned between the various units?

•When the initial research question develops into a large-scale community action program, which unit will claim and administer the full-blown effort?

Administrative policies regarding promotion and tenure inputs from multiple departments are very important to the investigators in this project. To facilitate this project, collaborative efforts must be championed at a level which encompasses all the collegiate units involved.

By the same token, all three dimensions of collaboration are illustrated by a multi-university project on integrated pest management (Ketcham and Shea, 1977). Investigators from fields of entomology, forestry and plant pathology drew upon disciplinary backgrounds in physiology, and biology, chemistry, entomology, ecology and social science to assemble a systemic analysis of three insects which were problemmatic in forested areas. In addition to specifying each individual's role vis-a-vis the identified problem, and mediating the decisions regarding the distribution and expenditure of funds in different administrative units, this project also added the layer of disciplinary issues.

Consider a different situation. Several chemists from the dairy science department and a chemist from the department of chemistry collaborated to identify and develop colloidal properties of bovine spermatazoa (Chandler et al., 1980). This project was collaborative but not interdisciplinary. It involved several scientists from the same discipline but from different administrative units. In addition to the considerations required to bring about cooperation between the departmental administrative units, working relationships between the scientists were also important. After consensus on the problem focus was achieved, the following considerations were relevant:

•How was the responsibility for the conduct of the project divided?
•How was the senior author determined?
•How were differences in interpretation mediated?
•Which department owned the equipment when the project was ready for termination or revision?

The availability of administrative support and encouragement at both the unit and the personnel interface was important.

Summary

By identifying the various dimensions of collaboration which describe a research activity, an understanding of the research management concerns of that project can be identified. The administrative issues important in enabling one scientist to study an issue from a single disciplinary perspective using funds from one administrative unit are surely different from those important in facilitating a group of scientists from several administrative units who are combining a variety of disciplinary perspectives in addressing an issue. Each dimension of collaboration adds another set of administrative and management concerns.

Collaborative research is not necessarily interdisciplinary. Interdisciplinary research is not necessarily collaborative. The services needed from a research administration office differ for a single investigator who uses a mixture of disciplinary approaches and receives funds for this research from multiple sources compared to, for example, a group of investigators from one multidisciplinary department, using departmental funds to combine their expertise on a research problem. Interdisciplinary research is sometimes interdepartmental. But interdepartmental research is not necessarily interdisciplinary.

The best research administration is that which enables scientists who ask good questions and who identify appropriate ways to look for answers to those questions to obtain the help they need in order to do so. The roles of research administration in enabling research are specific to each dimension, but not mutually exclusive. By understanding the issues specific to each dimension, the services of research administrators can be more effectively and efficiently targeted to their points of greatest impact. By understanding which dimension(s) are implicated in the management of a research project, the assistance can be delivered by the administrative unit closes to the issue.

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Working with Other Disciplines*

Earl R. Swanson

This paper discusses interdisciplinary research processes, especially those efforts in which agricultural economists work with researchers in social sciences, natural sciences and engineering. Substantial effort, together with some compromise, is required to achieve and maintain interdisciplinary collaboration in research projects. Alternative leadership styles and organizational structures for interdisciplinary research groups are described.

In this paper the term "discipline" refers to a specialized field of knowledge. Each discipline thus defined usually has a professional association and at least one journal. Equating a discipline to a profession is not completely satisfactory for all purposes, but it is consistent with common usage and convenient for the task at hand. Within a university context, a discipline corresponds approximately to an academic department, and disciplines develop when both faculty and administration come to recognize reasonably distinct areas of inquiry.

It is important to recognize that each discipline is usually composed of a set of narrower specializations and that the comprehensiveness of the discipline has at least three properties. First, there is a common conceptual model, shared to some extent by individual members of the discipline. Second, there is also a set of phenomena common to the various specializations; and thirdly, the breadth of the discipline is rarely embodied within any one scholar (Campbell, 1969). Cohesiveness of the discipline is achieved through the overlapping of the multiple narrow specialties. This overlap facilitates a greater degree of communication than is possible between disciplines.

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Agricultural economists have a long tradition of relationships with other disciplines. These relationships have taken a variety of forms. For many of us, our undergraduate education included a substantial component of the natural sciences, together with an occasional sampling of the social sciences. In graduate study the contact with other disciplines frequently focuses on the so-called tools that can be used by the agricultural economist. These skills and concepts are absorbed into our cluster of specialties and become a part of our collective professional competence.

Our record of joint research with the natural sciences and with engineering is well documented in Association surveys of agricultural economics literature. For at least two decades, a recurring theme of presidential addresses, invited papers, and Fellow's lectures at our annual meetings has been the admonition to borrow from our work with other disciplines, usually other social sciences, in a problem-solving mode.

NEED FOR INTERDISCIPLINARY APPROACH

A fundamental question has been raised in these admonitions: Who should integrate the results of research in the separate disciplines? Should it be the specialists working together in the research process, should it be the decision maker, or someone in between? Clearly, this question cannot be answered in the abstract, but the weight of professional advice has been to shift the emphasis to additional integration in the research process itself. Frequently the biggest contribution of our profession has been to help synthesize results from other disciplines (Nielson, 1974).

Although difficult to document, there has been some response to the call for interdisciplinary research articulated by Association speakers. The fact that reports from cooperative studies with other disciplines do not often appear in the American Journal of Agricultural Economics indicates in part that the research output from such projects does not lend itself easily to journal reporting, but also that such studies are not perceived to be in the mainstream of our profession. An informal survey of the Journal over the last twenty years indicates that virtually the only kind of joint research reported is that with natural scientists within colleges of agriculture.

This paper focuses on the process of research with other disciplines. An improved understanding of the process should assist us in allocating our research resources and, for that fraction devoted to working with other disciplines, to improve our effectiveness.

THE NATURE OF CROSS-DISCIPLINARY RESEARCH

In analyzing the nature of cross-disciplinary research, the classes of research activity can be distinguished along a continuum. These classes range from unidisciplinary to multidisciplinary to interdisciplinaty to transdisciplinary, depending on the degree of integration, a concept introduced by Rossini et al. (1978). Note that this classification refers to a particular research activity or project and not to other relationships among the disciplines.

Castle has provided a more comprehensive analysis of cross-disciplinary relationships dealing with implications for university organization (Castle, 1970). Implicit in the concept of integration is an anticipated tradeoff between comprehensiveness and depth of analysis. A narrow disciplinary mix may lead to analytical depth at the expense of comprehensiveness. This balance can only be judged with respect to the objective of the particular research effort. Research output in the form of a published report is the most easily available evidence for evaluating the degree of integration.

Editorial integration in research output is the most elementary level of integration. At minimum, it involves a report or a set of disciplinary reports on the same topic, edited without accounting for differences in terminology and concepts among the disciplines. While the report or reports may share an introduction and a conclusion, these sections may simply describe the history of the research project. A more integrated editorial treatment may involve consistent use of terminology throughout the study and the avoidance of isolated vocabularies. The reports that result from editorial integration may be multidisciplinary, in the sense of a patchwork quilt. On the other hand, interdisciplinary research ideally yields a seamless garment.

Interdisciplinary research is characterized by systemic integration. This implies a common view or representation which permeates and dominates the entire research effort. Evidence of strong integrative links among the various parts of the report is characteristic. Such integration may or may not be achieved by the use of a formal model.

Finally, at the upper limit of our continuum, transdisciplinary integration is theoretically a complete integration (Rossini et al., 1978). This classification implies that individual skills and disciplines are transcended and that scientists from several disciplines work together to create a new common cognitive map of the problem. This classification remains a theoretical ideal for any study of more than a very limited scope.

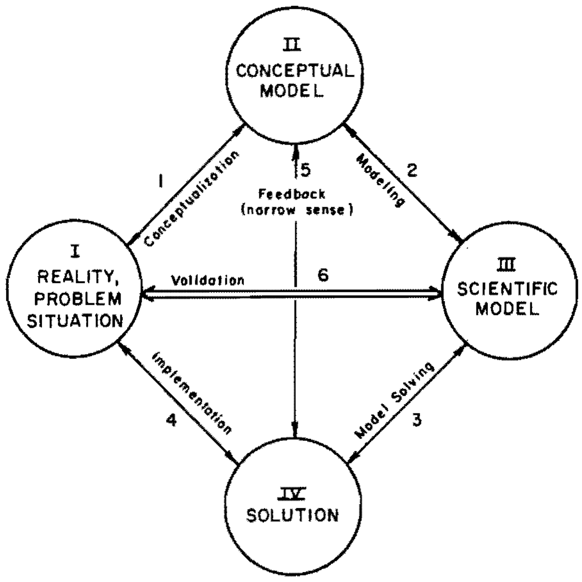
OBSTACLES TO CROSS-DISCIPLINARY RESEARCH

What obstacles are to be overcome in achieving an interdisciplinary or systemic level of integration? Two related considerations are involved. The first consists of the knowledge of epistemological problems and the second, of social elements (Petrie, 1976). The high level of interaction between these two elements implies that it is difficult to separate them even for exposition. This section discusses the knowledge aspects of cross-disciplinary research.

Admonitions to work with other disciplines emphasize problem solving, implying that the research output should be useful to public and/or private decision makers. Given problem-solving as the rationale for working with other disciplines, a systems perspective facilitates the following analysis of the knowledge bases in collaborative research (Figure 1). This version, described by Mitroff et al. (1979) and by Mitroff and Pondy (1974), is sometimes referred to as Singerian-Churchmanian model. Systems analysis in relation to agricultural and natural resource economics has been discussed in volume 2 of the Association of Agricultural Economics' literature survey (Johnson and Rausser, 1977).

A particular research activity may begin with any of the four elements and may end at any of the four elements: problem, conceptual model, scientific model, or solution. Each path between these four elements is bi-directional, implying a wide diversity in the sequence of problem solving processes. In fact, the simple system of Figure 1 has 3,555 total sub-systems

Figure 1. A Systems View of Problem Solving.



Source: Mitroff et al. p. 48.
Note: Figure 1 is reproduced with the kind permission of INTERFACES, published by the Institute of Management Sciences and the Operations Research Society of America.

that can be formed by considering all possible ways of combining two, three, and four elements.

If, for example, a cross-disciplinary inquiry starts with element I, Reality or the Problem Situation, the transition to element II requires formulation of the conceptual model. In some cases the conceptual model may be formal, a more likely occurrence in economics and the natural sciences than in the other social sciences. The conceptualization process (II) defines and bounds the problem in broad terms, the variables that are to be considered, and the level of aggregation.

Development of the scientific or formal model (III) is the next step. This modeling process includes the translation of the conceptual model into relationships which are then given greater empirical identity. This is followed by the solution (IV) and; finally, implementation brings us back to an impact on the initial problem situation (I). For completeness, note that paths also exist for model validation, between I and III, and for feedback interactions between II and IV. Certainly, the ideal problem-solving loop I-II-III-IV-I is not often found in individual projects. More often, the research process includes a subset of these elements.

Among the many possible subset loops, consider two: II-III-IV-II and I-II-IV-I. The linking of research with graduate education emphasizes loop II-III-IV-II in Figure 1 -- which concentrates on developing skills in conceptualization, modeling, and model solution, rather than on the implementation of results and policies (I-II-III-IV-I). Those agricultural economists who follow loop II-III-IV-II are concerned primarily with developing and solving models. Goals of research characterized by the II-III-IV-II loop usually include improved solutions to more complex models, with feedback in the narrow sense (Figure 1), and relatively little immediate interest in implementation. Many colleagues in the natural sciences operate within this loop and, in fact, acknowledge this loop as the only valid form of scientific activity.

Another loop used in agricultural economics is I-II-IV-I. This loop omits element III (Scientific Model). In research which follows this process, there is frequently confusion between conceptualization and modeling and an attempt to substitute a conceptual model for a scientific model. No matter how rich in detail the conceptual model might be, it can seldom substitute for a validated scientific model. Omission of the scientific model implies the loss of the opportunity to develop the logical structure of the conceptual model in a systematic way and to perform the important validation process. Thus the approach implied by the I-II-IV-I loop weakens the potential contribution to scientific knowledge.

To integrate approaches in the system (Figure 1), it is necessary for participants to communicate with others, ideally, in each of the four elements -- problem, conceptual model, scientific model, and solution. Perhaps the most important communication takes place with respect to the conceptual model. Each specialist can be expected to bring a specific paradigm (Kuhn, 1970; Maruyama, 1974; and Johnson and Rausser, 1977) into play as the problem is conceptualized. The degree to which core members of the research team share these paradigms is crucial in determining the level of integration in the final report.

As an example, consider collaboration between scientists using different conceptual models. Suppose that an agricultural economist is using a conceptual approach which combines the unidirectional-causal paradigm and the random-process paradigm, and that he or she is working with a biological scientist who subscribes to a mutual-causal paradigm.

The biological research worker looks for feedback loops in the system and for self-cancellation or self-reinforcement based on concepts of homeostasis. Although the economist may be familiar with related concepts from general equilibrium theory, the concept of the production function from the static theory of the firm does not extend very far toward achieving a common view with the biologist. Until they have at least moderate agreement on the paradigm, their prospects for successful interdisciplinary research are limited.

In this collaboration, the concepts and terms from statistics (Scientific Model, III) may form a useful communication device. Discussing the design of the experiment or the survey may lead back to an improved understanding of the differences and similarities in paradigms originating in the participants' disciplines. This awareness, communicated by both parties, may assist in achieving agreement on a conceptual model.

The difficulty of achieving mutual understanding of paradigms between disciplines should not be underestimated. Rossini et al. (1978) report that in a number of interdisciplinary studies, economics was the most difficult for participants from other disciplines to understand. The jargon, methodological preoccupations, and world view frequently proved difficult for noneconomist research collaborators.

Without at least a minimum of paradigm agreement among the specialists involved in joint research efforts, the frustrations from communication are likely to be high. In particular, if the communicating parties are unaware that they are using different paradigms and are aware only of differences in vocabulary or language, each party may view the communication difficulty as a result of the other specialist's deceptiveness, insincerity or lack of intelligence. This leads to the second aspect of research with other disciplines, the social process, which has two components -- organizational alternatives and approaches to interdisciplinary research.

ORGANIZATIONAL ALTERNATIVES

The institutional setting (Petrie, 1976) is an important organizational consideration for the research team. Research which is to be both problem-solving and interdisciplinary presents great challenges in an academic environment.

In order to encourage cooperative research among disciplines, a number of academic institutions have formed units outside the usual departmental pattern (Ellis, 1974; Capener and Young, 1975). For example, special institutes have been established to study the energy problem, and so forth. Less formal structures such as committees have also been employed, especially within agricultural experiment stations. The problems of a reward and incentive system that follows disciplinary lines and of the attendant risks for younger staff members who participate in such interdisciplinary undertakings have been spelled out (Johnson, 1971; and in Koopmans, 1979).

The most successful interdisciplinary research projects at academic institutions appear to be those that have been externally funded and that have a rather limited group, with no more than four or five disciplines represented. The most desirable institutional environment for such a research team must be one that permits a substantial amount of start-up time and that has flexible hiring arrangements. It is important that institutions contemplating the support of interdisciplinary work understand such special considerations.

It is unlikely that a research team deliberately identifies the intellectual and social components of the projects' organizational patterns in advance. It is more likely that the project's organization evolves into a stable pattern by trial and error. Nevertheless, it is useful to identify four types of approaches to interdisciplinary research and briefly to discuss their strengths and weaknesses (Rossini et al., 1978). These refer principally to teams containing from three to five core disciplines.

APPROACHES TO INTERDISCIPLINARY RESEARCH

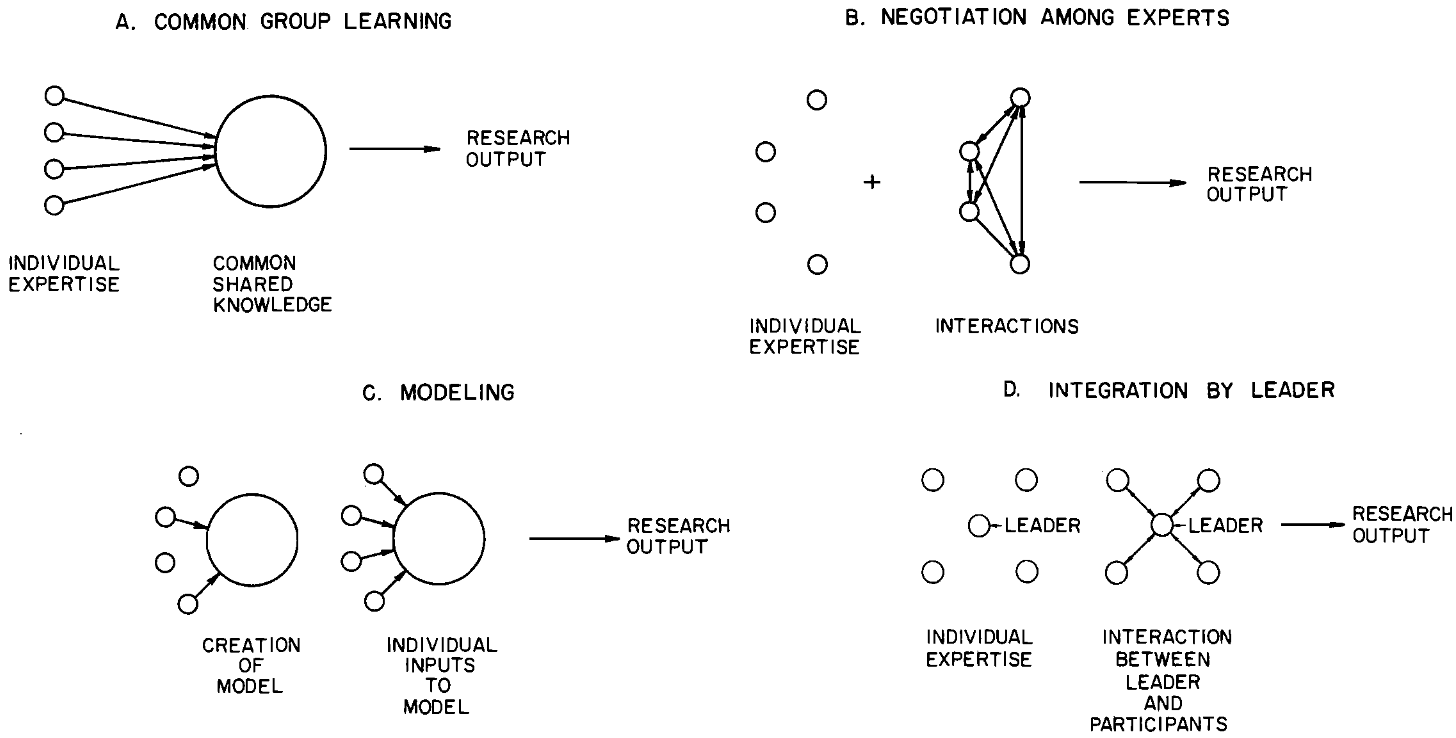
Common Group Learning

The research problem is defined and bounded by group decisions. Based on the interests and competencies of the team members, a division of effort is determined. Preliminary analyses are prepared, and each member reads the analysis of every other member. After discussion, reports are rewritten by members who are not specialists in the area (Figure 2A). The final report is the common intellectual property of the group. This approach clearly limits the disciplinary penetration. In a systems perspective (Figure 1), the interactions take place primarily between the problem (I) and the solution (IV) elements.

Negotiation among Experts

The beginning of this approach is similar to common group learning, with assignments of parts to disciplinary specialists. Attempts are made to bring the full power of the appropriate discipline to bear on its assigned part, and integration takes place by negotiation (Figure 2B). Negotiation focuses only on the overlaps and linkages among the separate draft reports.

Figure 2. Organizational Frameworks for Cross-disciplinary Research



The next iteration, done by the initial authors, takes into account the results of the negotiation. Nonexperts do not rewrite the separate sections of the final report. Committees of the National Academy of Sciences and task forces of the Council for Agricultural Science and Technology often tend to follow this pattern in the preparation of assessment and state-of-the-art reports. Discomfort with this approach tends to surface where competition arises between disciplines over the conceptual model.

Scientific Modeling

A formal model may serve a key integrative role for the team. All members need not participate in model construction (Figure 2C), but ideally all should contribute data. Models often tend to narrow the research focus, but they also encourage systematic collection and analysis of data. This approach works best when closely related disciplines are involved in solving an easily identified problem. For public policy-related studies, implementation usually requires a framework broader than the model alone. Finally, the model may have the advantage of depersonalizing the process of obtaining each participant's input.

Integration by Leader

The problem is divided and allocated by the leader on the basis of participants' expertise and interests. The only interaction occurs between the leader and the individual team members (Figure 2D). The communication pattern is that of hub and spokes, an efficient one for communicating simple tasks in small groups (Bavelas, 1976; Guetzow and Simon 1955). This is in contrast to communication in the other three approaches, common group learning, negotiation among experts, and modeling. In these approaches direct channels between most of the individual participants are needed.

Each of these four ideal models implies a somewhat different kind of leadership style. Consider the following five leadership styles (Robinson and Clifford, 1975): (a) activator, (b) controller, (c) martyr, (d) cavalier, and (e) abdicator.

The activator uses a democratic-facilitating type of leadership in which the style is group-centered. This leader is active and flexible in structuring group behavior and recognizes that everyone in the group has some skill or knowledge which exceeds his or her own. The controller's pattern is described as autocratic or authoritarian with rigid behavior that strengthens the position of the leader in the group. The martyr is passive and hopes that the participants will take pity and perform their assigned tasks. The cavalier is permissive; and, although this leader may entertain the participants, he or she cannot structure group behavior. The abdicator assumes little responsibility and behaves passively.

In the Rossini study (1978), it was not suprising that the activator style of leadership appeared to be the most effective in achieving integration in cross-disciplinary research. The second most effective was the autocratic or controller style, while the lowest level of integration was achieved with nondirective and permissive leadership styles (martyr, cavalier, and abdicator).

For both the common group learning and the negotiation among experts approaches, the activator leader was clearly the most effective. Activator leadership was also effective in the initial stages of a team operating within the scientific modeling approach. In the final stages, however, this type of approach may require a shift to controller leadership.

The success of the integration-by-leader approach depends very much on the leader. The routine demands of administering the project may be such that, even in the unlikely event that the leader is an expert in all the separate disciplines involved, the integration will be weak and the research output multidisciplinary rather than interdisciplinary.

FOUR EXAMPLES

The type of research team organization that has been used for a given project can seldom be determined by reading the report of the results. Participants in the research process are the only source of information. Consequently, in order to illustrate the use of the classifications and concepts presented, I now draw on some research efforts in which I have recently participated.

In the early 1970's, the College of Agriculture at the University of Illinois received a grant of approximately $600,000 from the Rockefeller Foundation for the project, "Nitrogen as an Environmental Quality Factor". The funds were allocated largely on a departmental or disciplinary basis, with five major groups participating: rural sociologists, agricultural economists, agricultural engineers, agronomists, and veterinary scientists. The common theme of nitrogen and the environment was the connecting link in the five separate efforts (Deeb and Sloan, 1975; Dickey and Lembke, 1978; Swanson, Taylor, and van Blokland, 1978; van Es and Sofranko, 1977; and Welch, 1979). A book on the project is being written by a single author.

This study corresponds to our multidisciplinary category because the separate disciplines approached a common problem, and the integration was at the editorial level, not at the conceptual level. Thus, the grant provided additional funds for departments to do research on the topic but did not necessarily provide the ingredients to integrate the results. The members of the team met to discuss one another's progress only occasionally.

A second project also had an environmental orientation. The project, "Soil Loss from Illinois Farms: Economic Analysis of Productivity Loss and Sediment Damage", was funded at a level of $122,000 by the Illinois Institute of Environmental Quality, a state organization. Six watersheds were studied (Guntermann, 1974). The organizational mode featured economics as the integrating discipline, and a formal model provided the scheme for integration.

This project's organization illustrates the modeling approach discussed above. Contributing specialists included an agronomist, an hydrologist, an agricultural economist, and a finance analyst. All persons were hired to work in the department of agricultural economics, an arrangement that clarified administrative allegiance. Early establishment of a satisfactory economic model permitted the contributors to identify their own objectives and contribution to the project.

The impact of the results on public policy can only be identified indirectly. A member of the Illinois Pollution Control Board mentioned that it would have been helpful if every important policy decision made by the Board could have had a comparable base of information. The usefulness of the report was, in part, a result of the interdisciplinary character of the project.

A third project, sponsored by National Science Foundation-Research Applied to National Needs, was an assessment of hail suppression technology (Changnon et al., 1977; Farhar et al., 1977). The eighteen-month project, carried out under a grant of $260,000, involved agricultural economists and other social scientists including sociologists, a political scientist, and a lawyer. Natural scientists included those from atmospheric science, one of whom served as a team leader, and an environmental scientist.

The organizational approach was a combination of negotiation among experts and scientific modeling. Although the central economic analysis used the usual economic concepts implicit in a national spatial-equilibrium model and the theory of the firm at the individual farm level, these concepts were modified in the course of the project. The modifications represented important inputs from sociologists and the lawyer, as well as others.

In the preparation of the conclusions and policy recommendations, the political scientist and the lawyer had important contributions to make. Among other things, the study recommended that funds for research and development of hail suppression either be substantially increased or eliminated. As a result of the study, National Science Foundation has now discontinued the $5 million line in their budget for hail suppression research. In addition, many of the recommendations have found their way into statements of national policy (report to U.S. Secretary of Commerce from the Weather Modification Advisory Board, 1978). In terms of policy impact the success was due, in a large measure, to the interdisciplinary nature of the study.

A final example is the National Defense University Long-Range Climate Project. This project proceeded in three phases. First, climatologists assessed global temperature and precipitation changes to the year 2000 for different major crop-producing areas of the world. Then, five climate scenarios were constructed. The second phase produced estimates of yield responses to weather variables made by agronomists. This permitted an assessment of the yield consequences of each climate scenario. The final phase was the input of this information into the U.S. Department of Agriculture's grain-oilseed-livestock (GOL) model to determine the impact of the five climate scenarios on location of crop production, international trade, and crop prices. Although the GOL model provided the integrating framework, the climatologists were unaware, or had only very hazy conceptions of how the GOL model operated when they made their assessments. Similarly, the agronomists were not aware of the various aspects of the GOL model when they made their crop response predictions. Nevertheless, this model served as an integrating device for the total project. Organizationally, this project represented a mix of scientific modeling and integration by leader.

CONCLUSION

To sum up, it is my judgment that the modeling approach or integration by leader approach is more likely to provide a satisfactory environment in which agricultural economists may contribute, especially when agricultural economists work with the social sciences, natural sciences and engineering. However, one should not presume that what seems to be a natural integrative role for agricultural economists will automatically be perceived as such by scientists from other fields. These disciplines also have macro models (ecosystems, energy accounting systems, etc.) with integrative potential, and some melding of conceptual models may be required. In many contexts there is an integrating task that is beyond economics. However, if the research has an implementation objective such as the drafting of legislation, it is important to select a conceptual framework which includes modeling and implementation.

On the other hand, an alternative approach which appeals to some agricultural economists involved in joint research involving social scientists is negotiation among experts. Given the diversity in preferences and skills of the participating experts and a limited time frame, this approach may provide a better oganizational structure for interdisciplinary research than one such as modeling which requires more complete synthesis.

This paper has attempted to describe the social processes of research which involves more than one discipline. Substantial effort, together with some compromise, is required to prevent research projects with interdisciplinary objectives from becoming multidisciplinary. Although the potential contribution from agricultural economists working with other disciplines remains high, allocation of large segments of our professional resources to such activity should be done with caution. The gains from specialization are too high to be sacrificed casually, and the opportunity costs of doing interdisciplinary research may easily be underestimated. It should be emphasized that the objective of the Association is to further the development of systematic knowledge in agricultural economics. A part of the development of that systematic knowledge requires working with other disciplines, and it is important that we do that part well.

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The Economics of Systems Research*

John L. Dillon

Professional training, research organization and research directions are three important opportunities for systems research in agriculture. This paper discusses the major tenets of systems research and argues that the systems approach to research is generally more efficient than the traditional disciplinary approach because it facilitates purposeful research management in terms of goal achievement and choice between research alternatives. These suggestions are offered in the context of the shift from the Machine Age to the Systems Age which is now occurring.

AGRICULTURAL RESEARCH IN THE SYSTEMS AGE

This section draws heavily on the contributions of Ackoff (1973), Ackoff and Emery (1972) and Weis (1972); to varying degrees, it also reflects the work of Dror (1971), Georgescu-Roegen (1971), Heady (1973), Heibroner (1972), Hightower (1973), Jordan (1973), Kuhn (1970), Lowe (1972), Weiss (1971) and Williams (1973).

As a prelude to looking at agricultural research per se, we need first to appreciate the broad scientific implications of the systems approach. In themselves, these implications are dramatic and far reaching. Before the advent of systems thinking in recent decades, scientists tended to derive their understanding of the functioning of the whole from the mechanical structure of its parts. The two concepts of reductionism and mechanism guided the scientific mode of thinking about and endeavouring to understand the world.

Reductionism implied reducing phenomena to their more basic (and hopefully independent) parts, analyzing these parts as independent entities to explain their behaviors, and then aggregating these explanations as an explanation of the phenomena under study. Any decent problem was tackled by reducing it to a set of simpler component problems whose solutions were seen, in the aggregate, as a solution to the whole. Ipso facto, reductionism abetted and fostered the proliferation of specialized deafness and tunnel vision in the form of independent scientific disciplines. The growth of science via reductionism resulted in the categorization of phenomena into smaller and smaller classes, each of which was associated with a specialist discipline -- and, as disciplines multiplied, each increased in depth while decreasing in breadth. At the same time, the narrowing and deepening of each discipline's interests moved it further and further away from the felt problems of the real world.

The idea of mechanism implied that phenomena could be explained in terms of mechanical or machine-like cause and effect relationships. Aided by Occam's Razor, explanations were pared to the simplest "adequate" causes. The influence of the environment was ignored by developing environment-free scientific laws or explanations in specially designed environments called laboratories (or research stations). Such closed-system thinking led to a view of the world as being both deterministic and mechanistic. Effects had only direct physical causes, and the world was just a machine to be analyzed as one might dismantle a car. There was no place for concepts of a teleological (i.e., purposeful) nature such as goals, choices and free will. The teleological view that effects may be due to the purpose they serve was considered to be outside scientific interest. As a concomitant to their deterministic and mechanistic view of the world, scientists had no concern for phenomena involving goal-seeking and goal-setting behavior.

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Increasingly since the 1950's, reductionism and mechanism have been seen as inadequate scientific bases for attempts to understand the world so as to enable purposeful manipulation and control. Expansionism, teleology and synthesis are now being recognized by science as necessary modes of thought for understanding the world. Stated another way, Science today increasingly tends to gain an understanding of the structure of the parts from an understanding of the functioning of the whole.

Expansionsim is the reverse of reductionism. By focusing attention on the system as a set of interrelated parts or elements, it denotes the view that all objects and events are parts of a larger whole rather than isolated elements. These parts may be of any kind -- concepts, physical phenomena, objects, people, etc. -- and as elements of a system have the following two characteristics:

(1) Each part affects the properties of the system as a whole.

(2) Each part depends on the properties of some other part(s) of the system for its own properties and for how it effects the system.

By virtue of these characteristics of its parts, a system is an indivisible whole and, in contrast to the reductionist view, is more than the mere sum of its parts in the sense that the system's behavior is not deducible from the behavior of its elements considered in isolation. Of course, as we all know, virtually every system we may care to delineate will be a part of some larger system and will itself have elements that can be viewed as systems.

Such a (systems) mode of thought leads naturally to synthesis, to the systems approach, whereby elements or phenomena to be explained (or understood or manipulated) are seen as part of a larger system or systems. Explanation then proceeds in terms of the role or function of the element in the larger system(s). System performance must therefore be judged not simply in terms of how each part works separately, but in terms of how the parts fit together and relate to each other, and in terms of how the system relates to its environment and to other systems in that environment.

The systems approach has obvious implications for the organization of science and research. While reductionism implies the specialized deafness of disciplines, expansionism and synthesis imply openness and "teamness" through interdisciplinary endeavor. Only such a holistically-oriented approach can lead to capturing an adequate understanding of a system for purposes of improving performance. Despite the traditional disciplinary organization of science, the world does not come to us in a disciplinary form.

There are also profound methodological implications of the systems approach. As soon as we recognize that physical systems are embedded in, or interact with, social systems, we recognize that science -- conceived in terms of understanding phenomena for purposes of manipulation -- can no longer be free from value judgements. Social systems involve not merely the interactions of physical forces but also contests of will arising from the purposive behavior of animate elements in the system. Even without contests of will, teleological considerations imply non-determinism and a capriciousness of behavior that can only be captured by subjective judgement and not by objective fact. Within the systems approach, therefore, value-free science will be diluted with some value-laden elements.

At the same time, if -- as has been hypothesized by Ackoff (1973) -- we are leaving the Machine Age and entering the Systems Age, the traditional hypothetico-deductive method of science, with its emphasis on cause and effect, will not have the field to itself. Of course, the traditional method (i.e., the old supposedly sequential routine of (1) observation, (2) hypothesis, (3) deductive prediction, and (4) performance testing based on the cult of arbitrary levels of statistical significance -- in the fashion of those great 17th century thinkers such as Sir Francis Bacon and the editors of many agricultural science journals) has never really been applied in strict sequential fashion (Medawar, 1967).

The teleological or means-ends approach must also be recognized as a valid scientific method. Because of the intrinsic purposiveness of social systems or systems with social effects or the purposiveness of a significant part of their environments, research on systems with social effects will increasingly require a means-ends approach. By this I mean the stipulation of a target and the assessment of alternative policies as to how the target might be achieved, given whatever initial conditions may be specified. Such a normative research orientation implies both an expansion of knowledge (how to get what we want) and problem-solving (how to be better off). To force it into the hypothetico-deductive frame is tortuous and artificial. Far better to recognize it for what it is -- a teleologically based and valid approach to research involving goal-seeking and goal-setting systems. By their nature, such systems are of a higher order than passive and reactive systems.

In the sense that lower-order systems are used as instruments by higher-order systems, systems may be classified (Ackoff and Emery, 1972; Jantsch, 1973) from lower to higher order as:

Passive: a system that is incapable of reacting to its environment (e.g., a clock);

Reactive: a system that can display different behaviors in different environments but only one kind in any one environment (e.g., a servomechanism);

Goal-seeking: a system that can respond in several different ways in any one environment, these responses each fulfilling an exogenously given goal by producing a particular outcome (e.g., an unmanned mooncraft); and

Goal-setting or purposeful: a system that can change its goal in constant environmental conditions or can pursue the same goal by changing its behavior in different environments -- it can select goals, as well as choose the means by which to pursue them (e.g., a manned mooncraft, a farm, the agricultural research system).

A further implication of all this is that physical scientists, by virtue of their concerns with lower-level (i.e., passive and reactive) subsystems, will increasingly need to respond to terms of reference set by social (including biological) scientists concerned with relevant, higher-level social systems of a purposive nature. In turn, social scientists will need to respond to terms of reference set by politicians employed by the overriding purposive system. Science will certainly have reduced opportunity to operate in a spirit of devine right -- although it probably never did quite operate this way.

Without doubt, I have used a broad brush in the above sketch to portray how I and some others see science presently progressing. Personally I believe a shift from the Machine Age to the Systems Age is fact, not conjecture. Only on this basis is it possible to rationalize the many straws in the wind within and about science -- straws that include, for example:

1) a shift away from inference to decision;

2) an increasing appreciation of the inescapability of subjective judgement;

3) the scientist's plaintive cry of "What are we here for? To solve problems or do research?";

4) a developing awareness that to use resources for one research project means something else cannot be done;

5) the growth of interdisciplines (such as cybernetics, information theory, operations research, computing science, etc.) concerned with purpose;

6) the growing use of task forces;

7) a concern with national science policy and the social function of science; and

8) an increasing recognition that Man does not live by bread alone.

What of agricultural research in the Systems Age? I believe the systems approach, viewed as a new and improved technology for our research, has substantive implications in three directions. These lie in the areas of professional training, research organization, and research directions. To me, these are the three major macro-economic aspects of the agricultural research system. Decisions made between and within these three areas will be the major controllable influences on how well agricultural research serves whatever purpose it is supposed to serve. Just what this purpose may be I leave until later.

Professional Training

The systems approach obviously implies the need for an appreciation of systems concepts and systemic procedures for research, particularly in relation to the agricultural system and its environment. As Klir (1973) emphasizes, purposive elements within and without the system need to be stressed because they will dictate the relationship of the professional to the system and the professional's role in the system. This has strong implications for the professional training of agricultural scientists. Training must also aim at versatility so that the future professional is not tied to a disciplinary straightjacket.

That people -- not cations or nodules or rumen flora or crop varieties or lifestock species or dollars -- consummate the system must be a basic text. Along with this must go an appreciation of lower-order systems, as instruments of higher-order systems. Teleological concepts and the means-ends approach to research should be introduced as a necessary corollary to the inadequacies of the old hypothetico-deductive method of research, as a means of assessing goal-seeking and goal-setting systems.

Disciplinary emphasis in training must be justified largely as a necessary prior background for interdisciplinary teamwork -- and this message should also be transmitted to those being trained. Professional training, moreover, should not be in terms of disciplinary majors but in terms of systems majors -- not biochemistry, soil science, agricultural economics and such like, but the crop-soil system, the plant-animal system, the production system, the communication system, the rural political system, etc., not forgetting the agricultural system per se.

The difficulty, of course, is that the systems approach is new and somewhat unfamiliar, while the disciplinary approach to training is old and familiar. Just like any other piece of new technology, albeit as yet in short supply, it will be adopted gradually, with the usual staging process from innovators to late adopters. Nor would I expect the rate of adoption of a new technology for professional training to be rapid. After all, academics are perhaps the most conservative purposive elements in the agricultural system.

In addition, the systems approach to training -- unlike much other new technology -- is not something with an easily quantified benefit-cost ratio. It lies outside the price system. With cost-return qualifications to be discussed later (Section 3), economic justification of the systems approach resides in its inherent logic as an approach to understanding and, *ipso facto*, in the inadequacies of reductionism and mechanism.

Despite all these difficulties to adoption, I see a need for a significant shift in the system of professional training. Instead of the present typical pattern of a broad (one or two-year) training base leading only to disciplinary specialization in later years, I expect the development of patterns involving an initial (one-year) introduction to the systems approach combined with basic disciplinary foundations, followed by a (two-year) period of disciplinary specialization, capped off with a (one or two-year) period of bringing together different disciplines in the context of some relevant agricultural system.

Research Organization

Little needs to be said on the need for shifts in the organization of research. What is required is a structure which will facilitate a synthesizing, integrative, team-oriented outlook rather than one that is analytical, compartimentalizing and disciplinary. As Heady (1973) puts it, in talking of Land Grant Colleges in the USA:

> "Over time the tendency has been for disciplines to dig deeper moats around themselves and to retreat further into their departmental bastions; while physically adjacent, their deepened discipline barriers prevent simultaneous attacks on the major facets of relevant problems. In fact, furtherance of the discipline typically is taken as more important than the solution of people's problems. This situation could be changed by administrative structures that give problem sets [systems] as much control over resources as the disciplines now have. Administrative control could be viewed in the context of a matrix where the rows are problem sets [systems] and the columns are disciplines." (p. 941)

With a systems focus, sole reliance on disciplinary units burrowing ever-deeper into virtual non-problems must inevitably go by the board. The systems approach immediately leads to recognition that the overall agricultural system is a purposive one, involving physical, biological and social parts, and that it operates within an environment having significant purposive components. Goal-setting elements within and without the system will dictate the need for multidisciplinary groups to work as systems teams on problems which the groups (not the individual scientists) judge to be significant. Moreover, unlike traditional disciplinary units, these interdisciplinary teams will not be permanent. They will need to be formed, reformed and regrouped as required for the solution of particular problems at particular times in the context of the overall agricultural system or of particular sub-systems. The membership of these interdisciplinary groups, too, will be more diverse than that typically represented by the disciplinary cubbyholes of agricultural research institutes.

The recognition of the key role of purposeful elements in the system or its social environment will lead both to enhanced recognition of the role of extension and to the participation of management and social scientists (particularly economists, sociologists and political scientists) as members of many research and/or extension teams. Far more so than at present, there will be a natural, vertical integration of research and extension activities. Further, the higher level of the system under study, the more the means-end approach will need to be complemented -- and eventually dominated -- by the hypothetico-deductive method of study.

As in the area of training, the transition of research organizations to a systems-oriented basis will not be easy. It will have its costs and, particularly while professionals trained in the systems framework are in short supply, its difficulties.

Research Directions

As soon as we look at a piece of agricultural research in the context of either the higher systems in which it is embedded or the lower systems which it influences, we can see implications which otherwise go unrecognized in the traditional disciplinary approach to research. This is so because optimization or improvement in merely a part of a system or sub-system cannot be presumed to lead to enhanced performance of the overall system. Indeed, it will generally result in overall sub-optimization. I can speak most easily of such effects in the socio-economic spheres of productivity, growth, security and equity, but similar implications must also exist in the physical and biological spheres. Given the dominant importance of people -- as both producers and customers -- in relation to the agricultural system, implications from the socio-economic sphere are most important. Let me therefore concentrate on the social sphere.

Traditionally, agricultural research has been production oriented. [I ignore the fact that some agricultural scientists have carried out pure research when they were being paid to do agricultural research. In the farmers' heaven, I expect their sin to be seen as grievous.] The goal and the result have been either the same output from fewer inputs or more output from the same inputs. To no significant degree has our research been oriented to encompass aims of social justice for farmers or domestic consumers. Benefits in the main have gone to well-to-do producers. The poor and disadvantaged, whether inside or outside of agriculture, have been neglected, if not positively disadvantaged. In the USA, for example, the development of cotton varieties specially adapted to the Southwest, along with the development of irrigation there, led to a much more efficient production of cotton.

As a result, cotton production shifted westward, and whole communities in the Southeast were left with unemployed farmers and workers, not to mention shopkeepers and parsons. Many of the displaced migrated to city ghettos in search of non-existent jobs for which they lacked training anyway. The net result was that a piece of agricultural research which was excellent from a disciplinary point of view generated great socio-economic inequities and added further to already overcrowded city slums. As some have argued, it is indeed a tenable view that the terrible riots of a few years ago in Watts, California, were basically caused by the "success" of production-oriented research which led to the displacement of agricultural labor in the USA -- for example, the mechanical cotton picker, hard tomatoes, etc. The results of such research gave too much to the wrong people and ignored significant (human) costs.

The first part of my argument is that the systems approach can provide an indication of where research is needed -- not just in the trivial sense of being able to say to a plant physiologist that we need an estimate of such and such a coefficient, but in the far more significant sense of recognizing disadvantaged regions or groups and identifying rural equity or security problems of farmers that lie outside what has been the traditional focus of research interests. We need to recognize -- and a systems approach which emphasizes purposive higher-level systems makes this explicit -- that there is more than a material product to agriculture. In the future, the questions to be answered may be "How much would it cost, how much are we willing to pay, and where should the money be spent in order to deliberately foster a less economically efficient system of agriculture whose social values add greatly to our entire national quality of life?" This question is already a live one in the USA. Its evaluation will certainly need a systems approach.

The second part of my argument is that the systems approach provides a logical and workable procedure for obtaining a guide to the likely ramifications of research. Williams (1973) suggests an example:

> "Of course we need new wheat varieties...but such plant breeding programs may be improved if we can see the problems of wheat growing as problems of farmers in particular environments trying to adapt to changes. To do this we should explore the potential for new varieties to meet particular specified needs, the role of new crops, the scope for changes in farm size, and the related problems of access to capital to do this, as well as the changes in the biological environment, and in educational systems and social structure, which these adjustments may involve." (p. 92)

Some of these effects will be good, some will be bad. Some will occur within the higher-order systems. Some will be significant, others not. But unless they are anticipated and appropriate steps taken either by redirecting research or by providing some form of cushioning to those who are adversely affected, agricultural research will fall into disfavor. Moreover, as economic progress takes place, the interdependencies of the socio-economic system will also grow, but at a faster rate. This will increase the likelihood that the gains and losses from new production technology will not be shared equally by society's members. So assessing the possible effects of production-oriented research is likely to become a more difficult task, and the need for a systems approach even more urgent.

AN EXAMPLE

Through an interactive, cyclical approach to examining needs and likely effects, it is possible to target required programmes (i.e., bundles of projects) to system goals. The political system influences overall goals both explicitly and implicitly, but here, too, a broad systems appraisal can be used to influence choice. Such an approach, under the name of Planning, Programming and Budgeting (PPB), led to the specification in 1967 of the four broad goals presently recognized by the U.S. Department of Agriculture (Bayley, 1971; Fedkiw and Hjort, 1967). These goals are stated as follows:

1) Income and Abundance -- to achieve a sustained and balanced agricultural abundance with fair income for our farmers;

2) Growing Nations/New Markets -- to provide new markets for our food, feed and fiber and to help growing nations win the war on hunger;

3) Dimensions for Living -- to expand the dimensions of American living and specifically to wipe out undernutrition in America; and

4) Communities for Tomorrow -- to build livable and healthy communities for tomorrow by revitalizing rural America and restoring the rural-urban balance.

These four basic missions or goals are divided into subcategories as follows:

1) Income and Abundance
   - Farm income
   - Agricultural marketing and distribution system

2) Growing Nations/New Markets
   - Food for freedom
   - Export market development
   - Agricultural development
   - International agricultural services

3) Dimensions for Living
   - Diets and nutrition
   - Health and safety
   - Education and training
   - Services for tomorrow

4) Communities for Tomorrow
   - Community development services
   - Housing
   - Public facility and business expansion
   - Resources protection and environmental improvement
   - Recreation, wildlife and natural beauty
   - Timber

These subcategories are further subdivided into program elements, and it is only at this level (and lower) that research categories of the traditional type (e.g., livestock improvement, grazing management) begin to appear. This is an interesting reflection of the fact that subsystems serve as instruments of higher-order systems and that, as I have already noted, the ends of purposive higher-level systems will set the terms of reference of scientists working on lower-level sub-systems.

THE SYSTEMS APPROACH TO PROJECT SELECTION

My thinking on this topic has been influenced by the contributions of Davidson (1973), Fishel (1971, 1973) and Lifson (1972).

My remarks will be limited to applied (i.e. real!) research on agriculture and to ex ante appraisal of such research. Some work has been done on ex post appraisal of agricultural research, generally indicating handsome first-order financial returns but neglecting social costs and second-order financial effects. For example, Schmitz and Seckler (1970) estimated a gross return of 929% on U.S. research and development expenditures that yielded hard tomatoes suited for mechanical harvesting. They also noted that such mechanical harvesting displaced 19 million manhours of labor annually, the economic and social costs of which need also to be taken into account. However, such account is of no assistance in ex ante choice between research alternatives, and the real problem is the ex ante choice. That it is a significant problem is indicated by the spate of recent conference proceedings (Davidson, 1973; Fishel, 1971; OECD, 1970) and other literature (see Anderson, 1972) concerned with the allocation of resources to agricultural research. The essence of the problem is limited funds in the face of virtually unlimited research possibilities. The basic question to be answered is: "Which projects and at what levels?"

Discussion on particular procedures, such as benefit-cost and cash flow analysis, aimed at answering these questions constitutes a significant slice of the recent management science literature. There are even two journals (R & D Management and Research Management) concerned with nothing else. Obviously I am not going to solve the problem of choice between alternative projects here. But, as evidenced by the work of Fishel (1973) and Pinstrup-Andersen (1974), I am convinced that the systems approach offers the opportunity of providing more complete and more reasoned answers than other procedures. While these other procedures [reviews and a listing of some relevant literature are given, for example, by Gilchrist (1973), Morgenthaler (1973), Pearson (1972) and Ritchie (1970); see also Parts IV and V of Fishel, 1971)] may be utilized within a systems approach, they are in themselves inadequate relative to publicly-funded research (although they may be satisfactory enough in a purely commercial context). Why? Because in not using a systems approach for appraisal, they inevitably give only partial assessments.

As a sub-system of the (purposive) national system, the agricultural research system should basically be a hierarchial one with decisions relative to national research goals being fed downwards. This is not to say, of course, that there should not be opportunity for upward feedback and suggestion so that goals can be conditioned by the perception of means. However, this is to say that at each hierarchical level the decision on "what to do" should come from above. This should then lead to a "how to do it" decision which is passed down to the next level as a "what to do" decision. To the extent that a research system is not organized in this management fashion, it can only be meeting national goals by accident!

The "how to do it" decision is a problem of risky choice. Any such problem involves a choice between alternatives whose payoffs are uncertain in that they depend on the state of the world which prevails after the choice is made. As judged by the decision-maker, these different possible states of the world will each have some probability of occurrence. For example, the "what to do" instruction may be to investigate the eradication of a certain insect pest. The "how to do it" alternatives to be investigated might be a chemical pesticide, a biological control system, a sterile-male technique, or some combination of these.

Each of these research alternatives will probably have quite different payoffs according to their costs and benefits and according to which state of the world eventuates. Thus, for each alternative there will be a subjective probability distribution of payoffs. The best alternative will be the one whose probability distribution of payoffs is most attractive to (i.e., most preferred by) the decision-maker. Note that in such a decision problem it is impossible to avoid judgement. To varying degrees, judgements have to be made regarding: (1) what alternatives are available or should be considered; (2) what states of the world may prevail; (3) what the chances are of these states prevailing; (4) what constitutes the costs and benefits contributing to the payoffs; and (5) which probability distribution of payoffs is most preferred in terms of the goal(s) that generated the decision problem.

Any decision problem can be put into the above context of alternative choices and possible outcomes under different eventualities. Such a paradigm renders explicit how rational decisions are implicitly made and can be used as an aid to decision-making.

Consider now how the systems approach can contribute to the problem of choice between research alternatives. In doing so I presume we have available a model of the system relevant to our research decision problem. In general this system, however tentative and poorly specified, will be some sub-system of the overall agricultural system. Certainly, the pre-research model available for making the choice between research alternatives will not be complete (in the sense of what we would like to have). The challenge is to decide what further research to undertake in order to enhance the performance of the system. If we already knew all about the system, we would have no need to carry out any research! The role of the model, to the degree of detail judged relevant, is to specify the state of current knowledge about the system.

By reference to the model, possible action (research) alternatives can then be specified by a process of synthesis. By applying the systems approach applied to the systems model, possible points of control, manipulation or adjustment relative to the goal(s) of interest are indicated. To what extent this control, manipulation or adjustment can be undertaken and how it will actually influence performance is the research question embedded in each alternative. One alternative, of course, may be to design a completely new system; another should be to do nothing.

Further, the specification of alternatives must include consideration of budget constraints. No one alternative can involve a research cost in excess of the available budget. To the same end, a set of complementary projects should be appraised initially as a single alternative.

The pre-research model of the system, relative to each research alternative, can provide an estimate of the size (magnitude) and dimensions (dollars and non-financial) of the outcomes for each alternative. Outcomes may involve many things: farmer incomes and their distribution; lower food prices and consumer savings; import savings and export earnings; farmer satisfaction with higher yielding crops and animals; displacement of farmers; closure of schools; stress on cities; promotion of researchers; etc. Some of the difficulties in measuring costs and benefits are discussed by de Veer (1971). These outcome estimates for each alternative provide answers to questions, such as: "In the various dimensions of relevance, what net system outputs would be expected under the possible states of the world that may occur"?

The states of the world across which the outcomes may lie will be a compound of two factors: (1) the stochasticity of the system's operation, as determined by its internal relationships and its environment (either because it is that way or because we are uncertain); and (2) the degree of success that may be attained in manipulating or restructuring the system via each alternative line of attack. This latter is a question of the degree of success, of course, and indicates, for example, that particular research alternatives should not be specified merely as "a chemical pesticide project", but as "a chemical pesticide project using $x over y years". In this fashion, a number of like projects of varying intensity and time span could be specified as relevant alternative actions.

The question of the degree of success also involves questions of real-world effects and their time patterns, not just success in research. As de Veer (1971) emphasizes, adoption in the real world is a crucial factor and hence implies considerations of communication and extension.

Having used the systems approach to generate alternative research projects and to specify the probability distribution of outcomes for each alternative, it only remains for the decision-maker to choose the preferred probability distribution of outcomes. The corresponding alternative, by definition, will be the optimal project choice.

Compared with other approaches suggested for project selection the systems approach has four broad advantages. First, it provides greater completeness in the sense of bringing to attention possible alternatives and the dimensions and directions of their consequences. Decisions made using the systems approach are less likely to err in overlooking the opportunity costs (i.e., foregone opportunities) associated with the choice of any particular alternative. Second, the systems approach facilitates and helps make explicit the judgements that are unavoidable in project selection. Third, it is goal-oriented and recognizes the hierarchical purposiveness to which the research system, as an instrument of the overall agricultural and national systems, should respond. In this sense, the systems approach recognizes that it is more important to have the "right" objective than to make the "right" choice between alternatives. While the wrong alternative merely means something less than best will be chosen, the wrong objective means the wrong problem is being studied. Fourth, the systems approach to project selection can be applied at any level of sophistication or detail. At the simplest it might involve back-of-an-envelope scribblings. On the other hand, it might involve the most complex specification of a model reflecting current knowledge, together with detailed synthesis of alternatives and analysis of outcomes using discounting, simulation, Bayes' Theorem, decision trees, multi-attribute utility functions, etc.

It is the orientation (philosophy) that is important, not the details of sophistication (technique). Like the assessment of commensurable, incommensurable and intangible benefits, the complexities of multiple-goal systems and interdependencies between alternative projects are difficulties to be faced under any selection procedure. Suffice it to say that there are pointers for the resolution of such difficulties. Using the logic of the systems approach, these difficulties will be brought to attention and handled with respect to the systems' goals.

EFFICIENCY IN SYSTEMS RESEARCH

As a basis for discussion, let me first outline what I mean by efficiency and by systems research.

Efficiency

Efficiency can only be defined in reference to goals and presumes that gross benefits exceed gross costs, however measured. If we have only a single goal, we are efficient when we achieve the goal of least input cost. Or, if input cost is constrained, we achieve the greatest possible degree of goal attainment with the limited inputs available. Regarding multiple goals, efficiency prevails when input costs are distributed efficiently relative to each individual goal, subject to the constraint that aggregate satisfaction across goals cannot be increased by shifting inputs from one goal to another. By their nature, goal-seeking and goal-setting systems will aim to be efficient. Note, too that the *ex ante* efficiency of stochastic systems can only be considered in probabilistic terms.

Since most scientists (including some economists) are confused about efficiency in the sense that they think it has only a monetary or physical connotation, I must emphasize that in my view neither the goals nor the input costs will always necessarily be measured in money or physical terms. In many situations non-monetary goals will be relevant (although it will generally be useful to know their financial opportunity costs), and inputs will often involve non-priced activities. As a result, efficiency may sometimes be a matter of judgement.

Both goals and costs may involve incommensurate, if not intangible, aspects -- so that personal judgements are inescapable and what seems most efficient to one decision-maker may not be so to another. Given these difficulties, I will, in discussing the efficiency of agricultural systems research, be implicitly referring to whatever goals are specified by the purposive nature of the research system and the systems which it serves. These system goals have often gone unrecognized by researchers who, at least in the past, have tended to misinterpret the overriding goal of agriculture as maximum physical production and to emphasize the personal goals of scientific status in their disciplinary peer group.

Systems Research

This, I feel, is best defined (after McGrath et al., 1973) as the process of studying a system through:

1) specification of the relevant system, system performance and environmental variables;

2) determination of the existence, degree and form of relationship among these variables; and

3) use of this information to arrange or redesign the system so that it operates optimally (i.e., most efficiently) with respect to its objectives.

Such a process implies a closed-loop cycle of four research stages, each of which carries out three mutually-dependent research functions -- mode development, information collection, and information synthesis. The first research stage emphasizes development of a research model so as to enable delineation of system performance requirements; the second aims at the synthesis and interpretation of information in order to assess the design consequences of the performance requirements; the third concerns the development of amended or redesigned models; and the fourth deals with the evaluation of the amended system's performance.

The need for a multidisciplinary team approach is implicit in such a research process. Without a suitably diverse team, it would be impossible to adequately handle the research functions of model development, information collection and information synthesis for any agricultural system worth researching. Further, insofar as the benefits of agricultural research often depend upon farmers' actions, systems research teams should involve personnel with an orientation to farm management and extension -- even farmers themselves in some cases -- and not just those traditionally labelled as agricultural scientists.

Given the above definitions of efficiency and of systems research, efficient systems research implies:

1) that the gross benefits, however measured, of the research exceed its costs; and

2) that the distribution of effort and resource between the four research stages and across the three research functions within each stage should be such that the research goal cannot be better achieved by some other allocation.

This, of course, is a mouthful of a statement and begs a number of questions. First, it assumes adequate specification of the research goal. In my view, this is the responsibility of a higher echelon in the agricultural system and should come down to the research team as a "what to do" instruction. Moreover, this instruction should include an initial specification of the total resource to be used over some defined time period.

Second, the statement assumes knowledge of costs and benefits. Both of these will be uncertain -- markedly so on the basis of experience. However, if the project has been selected on the rational grounds of expectations (as discussed earlier), we must assume its benefits are expected to exceed its costs.

Third, appropriate delineation of the relevant system is assumed. This is no easy problem. The choice of system boundaries is one of the most important determinants of the scope and intensity of the systems research effort, and yet it is one on which there are few guidelines. Like others, this decision must involve judgement. What is sure is that agricultural research needs to be conceived and appraised in a context extending beyond traditional disciplinary boundaries.

Fourth, the statement implies that is is possible to estimate the gain from marginal units of investment in each systems research function at each research stage. This, of course, is impossible because quantitative data for such an assessment is not available, and the outcome of each activity will be uncertain. Lacking precise data, all that can be done is to make judgements. In terms of hindsight, these judgements will not be error-free. They should, however, go some way toward the avoidance of gross errors -- such as, for example, the predilection of some researchers to over-invest in the development of design models and of others to over-invest in the collection of information for the evaluation of the design model's performance.

From a research management point of view, systems research demands a systems approach, i.e., an approach based not on disciplines or functions, but on goals. The systems approach to management has come to be known as PPB -- or Planning, Programming and Budgeting -- and has been discussed in an agricultural research context by a number of authors (Bayley, 1971; Fedkiw and Hjort, 1967; McGregor, 1971; Puterbaugh, 1971, 1973). PPB stands in direct contrast to the traditional approach of budgeting research on an incremental basis with a bit more or a bit less this year than last year.

Under PPB the orientation (if not the actuality) is to zero-base budgeting. Each year every budget item stands on trial, and there is no presumption that last year's budget provides the baseline guide for this year's (Hannah, 1973). With PPB, budget planning involves the determination of objectives, the evaluation of alternative courses of action and the consequent authorization of select programmes specified in terms of a budget and a time-expenditure pattern. In terms of research management, PPB implies the programming of approved goals into specific objectives and activities, followed up by the design and implementation of organizational units or teams to carry out the approved programs under regular review arrangements. As Schick (1971) puts it:

> "Traditionally, budgeting has defined its mission in terms of identifying the existing base and proposed departures from it: 'This is where we are; where do we go from here?' PPB defines its mission in terms of budgetary objectives and purposes: 'Where do we want to go? What do we do to get there?' The environment of choice under traditional circumstances is incremental; in PPB it is 'telestic'."

In parallel to the purposive and hierarchical nature of the agricultural system, the decisional process under a PPB approach to research management is disaggregative downwards. Policy at the top constrains choice at the bottom. In contrast, the traditional (incremental) approach gives a flow of budgetary decisions that is upward and aggregative, with a consequent research pattern that is exceedingly sluggish in response to (if at all guided by) top-level goals.

So much for the efficiency and economic management of systems research *per se*. From an economic point of view, how does the systems approach compare with the pre-systems approaches? In my judgement, used rationally the systems approach must be much better. For a given outlay of research resources, it must yield a greater return. My reasons for this belief are that with a systems approach:

a) Research is more purposeful, there is less danger of working on the wrong problem, and there is a greater chance of recognizing and responding to research needs and opportunities.

b) Better research management (e.g., PPB) is facilitated.

c) Agriculture is recognized for what it is -- a hierarchy of systems with a purposeful nature.

d) Much can be done more efficiently than otherwise (e.g., model rather than real-world experimentation) (Anderson and Dent, 1972; Sturgess, 1972).

SUMMARY

Having covered a wide range of territory, let me reiterate my main points.

I believe the systems approach constitutes a new technology for viewing the world. In recognizing the purposeful nature of much of the world, it has substantive implications for Science. Expansionism, teleology and synthesis must be admitted as valid elements of scientific methodology -- and Science takes its place as an instrument of higher systems. As a corollary, there are important implications for agricultural research in the areas of professional training, research organization and research directions. Decisions between and within these areas will largely determine how well agricultural research serves its masters.

In particular, the systems approach has a role to play in the allocation of agricultural research resources. It recognizes the agricultural research system as an instrument of higher purposive systems and provides frameworks both for bringing research alternatives to attention and for their assessment.

Finally, I have argued that the systems approach to research will generally be more efficient than the traditional disciplinary approach. Not only does the systems approach provide a better view of the agricultural system, but it also facilitates purposeful research management in terms of goal achievement and choice between research alternatives.

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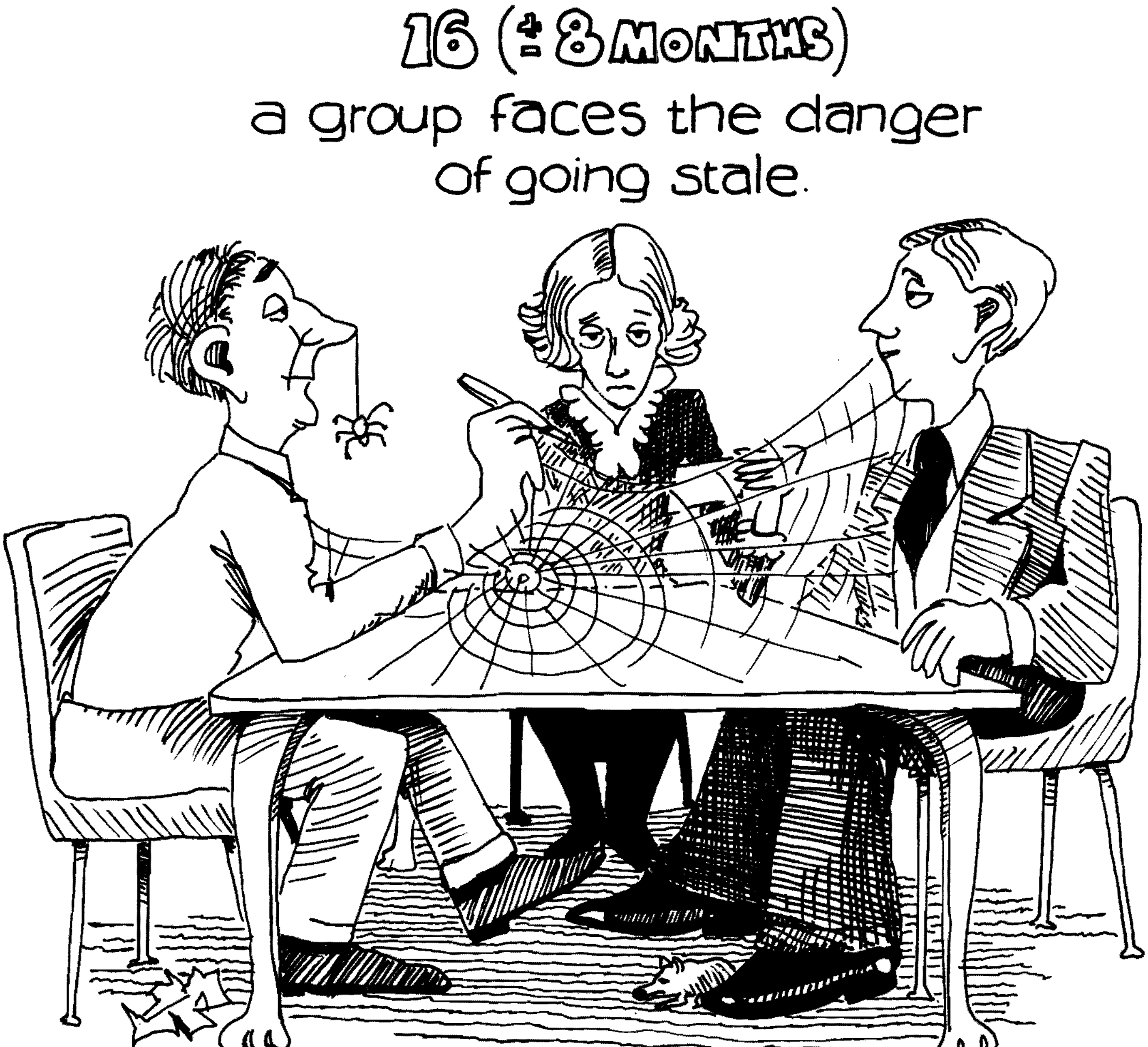
Challenges



The Challenges and Potentials of Interdisciplinary Research in Agriculture*

C. A. Francis, R. G. Arnold, J. A. Deshazer, D. G. Hanway, and I. T. Omtvedt

These authors point out that the key to successful research with limited resources is the efficient use of those resources. This paper discusses research objectives, team organization, leadership models, team interaction, budgeting and control, dissemination of results, credibility and accountability, research evaluation, rewards and recognition, and training as management issues for interdisciplinary research in agriculture. Opportunities for IDR in integrated crop improvement and cropping systems are identified.

As crop yields and levels of animal production reach new and higher plateaus, it becomes increasingly difficult to identify and solve the constraints which limit still greater gains in productivity and efficiency. Problems are becoming more complex, and the solutions are no longer easily attained through the application of research expertise available from a single discipline. Complexities such as crop-animal systems interacting with the natural environment, multiple cropping systems and small farmers in the tropics, and the efficient use of energy in crop production for the future are but a few examples of what the agricultural research scientist faces today.

When it has been determined that an objective is too broad for solution by a single scientist or discipline, too complex to be studied by a single department, or requires more and varied resources than are available in one narrowly-oriented research group, then a team approach is probably indicated. Only those research objectives which can be met more efficiently, rapidly, or completely by a team need be approached in this manner. The team is not an end in itself. Each problem must be approached with an objective assessment of the resources and expertise needed to solve that problem. If team research is the best way to meet a specific challenge, there are a number of ways to organize a group effort and encourage successful operations.

This paper explores a number of questions which often arise in relation to interdisciplinary teams and provides a series of examples and experiences which may serve as a guide. It is hoped that this paper will stimulate thought and discussion about the role of team research in agriculture and that additional ideas and opinions will then be brought into light.

RESEARCH OBJECTIVES

Essential to any successful research effort, and especially to those activities which involve an intimate interaction among a group of scientists, is the clear definition of research objectives.

Specific research objectives for a team of scientists must be consistent with the objectives of the organization, attainable within the expertise of the individual team members, and realistic within the context of other responsibilities of each person on the team. A clear definition of objectives can lead to a clear understanding of each individual's role in meeting those objectives, a realistic time frame

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for the team to follow, and a mechanism for evaluating the results. Without this definition of clear objectives, it is too easy for a team to get lost in other details, and for an individual to get lost within the team. The specific definition of each person's role on the team, budget, and responsibility is important to each individual's efficient and unique contribution to the team's effort and to continued identity with his or her discipline.

A definition of objectives is the logical first step in the design of team research. It could be argued that this definition would be most appropriate before the actual formation of a team and should be done by an individual or group which is to be held accountable for the results. A research director, board of directors, technical committee, or other group has the broadest perspective regarding these concerns, regarding the emphasis to be placed on each priority item in relation to others, and regarding simultaneous achievement of several objectives within that organization. Once the general objectives have been defined, specific teams can be organized, and much individual responsibility can be defined and carried out effectively by those directly involved with implementation. Periodic redefinition or modification of objectives, in light of the results of the research, may subsequently be needed.

TEAM ORGANIZATION

The formation and organization of a research team depend greatly upon the nature of the problems to be studied, the facilities and resources which can be mobilized to work on those problems, and the time constraints under which the team will operate. In addition, the organization of a team depends on the size and complexity of the problem, and the time and resource constraints under which the team is expected to complete the work.

There is no single model which will be appropriate for every situation, but the success of the team will depend upon the quality and orientation of the people who make up the team, their attitudes and personalities, and their specific preparation to do the task at hand. A team may include from two to any number of scientists. Some of the most important advances in plant breeding and pest resistance have come from an extended collaboration between breeder and pathologist -- for example, in the selection and testing of tolerant strains of crops. At the other end of the spectrum, international agricultural centers, such as the International Rice Research Institute, consist of integrated teams which involve over 60 people and include specialists in the basic sciences, economics, and extension/teaching activities. Thus, size of the team depends upon the size of the problem and the universe of action into which the results are destined to be applied.

Composition of the team depends, as well, on the nature of the research objectives and on the detail with which they are to be studied. To illustrate this, a team was formed in the International Center for Tropical Agriculture (CIAT) to study the problems which face small farmers in the quest for new technology and in the application of research results to tropical crop and animal programs. This team included an agronomist, a systems engineer, an economist, an animal scientist, a specialist in pest control, an agricultural engineer, a sociologist, and an anthropologist. The team was broadly based because of the complex nature of the interrelating biological, physical, cultural, economic, and political issues, all of which influence the small farmer and decisions related to production.

More common examples in the international centers are the crop improvement teams which focus on a single crop commodity and include such disciplines as plant breeding, agronomy and soils, economics, pathology, entomology, microbiology and plant physiology. These commodity-specific teams concentrate their research on the physical and biological variables which influence production and productivity.

Another model for team organization is the "task force" or "future shock" approach, wherein a team of specialists is brought together for some specific length of time to solve a special problem. In industry, this may be a team to design and test a new product, to research and implement a new crop sequence for an integrated cropping system, or to put a person on the moon. In the university context, this may be a committee to study long-term research priorities, to review the programs of one department, or to present a seminar and written treatise on interdisciplinary teams. People are generally reassigned from other duties and placed in a special group with a specific mission. Industry, universities and foreign governments use consultants in this way, and the time frame may vary from an hour or a day to several years on certain jobs.

LEADERSHIP MODELS

The leadership of an interdisciplinary team depends upon the size of the team and upon the experience and personalities of the members of the team. The administrative organization of the university, research center, or other entity also influences the type of leadership which is needed. Two contrasting examples of leadership situations illustrate this.

Team Leader Model

The "Team Leader Model" is an approach in which one designated individual has the total responsibility for defining the responsibilities of each person on the team. The leader is in charge of identifying specific objectives of each person's research, evaluating their productivity and allocating terms of pay increases, promotion, recognition, etc. The leader in this role is responsible to superiors for the success or failure of the team. It is up to this leader to select the individuals for the team and to add or subtract new persons or disciplines when this is in the interest of getting the job done.

This style of leadership can be very effective and efficient if the leader is competent, has the respect of the team, thoroughly understands the problem to be addressed, has the experience and forcefulness to lead a group, and will provide the team with the direction and support needed to get the job done. This leadership style functions efficiently in industry, in some of the crop improvement programs of the international centers, and when some of the participants are hired on a short-term basis as consultants. The success of this model in the university community is questionable.

Collegial Model

In contrast, the "Collegial Model" of leadership is characterized by an individual designated as committee chairman. In this approach, each senior member of the team shares equally in the decision making process. All are responsible for the results of the team and are held equally accountable for them. Most evaluation is done by someone higher up in the organization. The determination of specific research objectives of each member of the team is negotiated by the team.

In this style of leadership the chairperson is mainly an administrative leader who handles the details of budget, reports, and the day-to-day division of labor among team members. The chairperson usually participates as a contributing researcher on the team. This leadership role can be a rotating job among the qualified members of the team. The chairperson serves at the pleasure of the entire team -- the team members can ask for a change at any time or at some regular, specified interval.

An example of this type of organization is the bean improvement team in CIAT. With ten senior scientists (breeders, pathologist, entomologist, microbiologist, soils-agronomists, economist), decisions are made at group meetings in which all participate. At various times in the past seven years, the team has been lead by a breeder, an agronomist, a microbiologist, and is currently led by an entomologist. An alternate model is for a respected scientist or administrator who is not an active researcher-participant in the team to serve as a coordinator or facilitator.

The appropriate leadership style for a given situation is probably neither of these clear-cut cases. The previous experience of one or more team members often gives more weight to their opinions in the decision-making process, and personalities cannot be ignored. Leadership is complex. The model adopted for any team must reflect the nature of the problems to be studied, the time frame in which the team will operate, the additional responsibilities of each team member, and the reward system of the organization.

TEAM INTERACTION

Effective interaction among members of an interdisciplinary team is indispensable for success. The regular communication of ideas, results, and problems distinguishes a functioning team from a collection of specialists who happen to be together in a department, office or institute. One of the most successful teams on the Nebraska campus is one that regularly performs on the astroturf on Saturday afternoons in the fall of the year. Their success depends to no small degree on the interdependence among the disciplines, an ease of communication among the members of the team, and the common goals which are clearly shared by the individuals.

Interdisciplinary teams often have problems in communication. Scientists' specialization in discrete disciplines has led to the development of specialized languages or "dialects". One can become aware of this and of the difficulty in understanding or communicating ideas simply by visiting a seminar in another department here on campus. The ambiguity of such terms as "systems", "models", "elasticities", "functions", and many others is such that even colleagues in the same department may debate a term's precise meaning, if such really exists. An agronomist or plant breeder's replication of a field experiment may appear superfluous to the biochemist or extension specialist. Likewise, a soil scientist doing very basic research may be frustrated by the urgency of getting practical results from an experiment. The deeper we dig each hole in a quest for knowledge about a specific area, the less we are apt to see what is in the bottom of other holes around us, or even where others happen to be digging. Communication barriers do exist in teams, and ways to overcome these obstacles must be considered in the organization and leadership of a team.

The determination of short-term objectives for each member of the team may, to some degree, be a product of team interaction. One way to facilitate this is through periodic planning sessions, at which each scientist summarizes results from the previous year and outlines priorities for the coming year, with a justification and a brief description of the methodology for meeting each objective. The other members of the team then respond with questions or suggestions which may contribute to revision.

Since there is usually continuous interaction among the scientists during the entire year and since highly-trained specialists almost always have differences in opinion, these "bloodletting" sessions often generate heated debate among the individuals on the team. This process can help to produce a set of team objectives for the coming year, as well as specific complementary objectives for each discipline. Optimally, the entire team is then committed to these objectives. The team members' total capability to solve specific problems is expanded because each person has been a participant in setting objectives for all disciplines.

Efficiency in research is enhanced when people working on the same crop or animal species coordinate their measurements on similar genotypes or treatments in the field or laboratory. If an agronomist, a breeder and a physiologist are collectively interested in the genotypic response to different levels of fertilizer and the mechanisms by which this fertilizer is taken up by plants and metabolized to form the final yield, one trial could probably be designed to meet their various needs. Performing one trial rather than three would cut costs and either enable these three colleagues to put out three experiments of common interest to give triple the information to each or allow the resources to be applied to another project.

The principal cost of field research is in the logistics of putting out a trial and in caring for it during the season -- as compared to actual data collection. In field, greenhouse, growth chamber or laboratory research there often may be additional benefits in the shared use of equipment which would be impossible to obtain if each group were to attempt to purchase an entire set of the machinery or instruments required for an experiment.

Focus on team goals is essential to the success of an interdisciplinary team. There is a need for each specialist to identify with some part of the problem and an even greater need for all the pieces of the problem to fit together into a solution. The interaction of a team in setting priorities and in keeping up-to-date on progress of colleagues is essential to keep the work appropriately sequenced and to assure that resources are most efficiently directed at the most limiting components of the system.

BUDGETING AND CONTROL

The allocation of a budget to a research team will depend upon the size of both the team and the job to be accomplished, on the time in which the research is to be completed, and on the administrative organization within which the team will operate. For many research teams, a number of items (salaries of senior staff, basic research facilities, support staff and operating funds, etc.) may already be budgeted as a part of the support for each scientist. Additional activities to be addressed by the team may or may not require additional funding to be successful. Problems frequently arise, however, if the activities of each scientist on an interdisciplinary team are considered "additional" to already full-time jobs and must compete for time and resources with other projects.

If the team is formed to do a certain research job and is funded with a budget for the term of its activities, then quite another type of budgeting and distribution of funds is needed. Sometimes, the team must decide how to allocate the available resources in order to accomplish the objectives most efficiently. An equal distribution of funds among disciplines or individuals is rarely optimum. For example, in the early "tooling up" phase of research, there may be certain specialized instruments and equipment needed by the pathologist, which are much more expensive than the field equipment needed by the breeder or agronomist on a crop improvement team. Distribution of resources should reflect the members' needs so as to put the entire team into operation as quickly and efficiently as possible.

Nor should this distribution be static over time. As the research progresses and as new activities are initiated to follow the objectives set forth, the allocation of funds should reflect these new priorities. A phased and scheduled emphasis on different disciplines may be warranted as some of the constraints which limit production on a specific crop are resolved, for example, and other new factors are identified as priority research areas. A degree of flexibility both in staffing and in the allocation of resources from year to year is desirable. In the university setting, the traditional allocation of budgets by departments/disciplines has presented problems because flexibility in fund distribution is necessary in implementing new interdisciplinary approaches to research.

One of the advantages of team approaches to problems is the pooling of resources in the field or laboratory. Regular team review of priorities, specific activities and results can lead to a more efficient use of limited budget and other resources when simultaneous collection of data on the same set of treatments may be carried out by specialists in two or more disciplines. Team review of activities also serves as an internal control of the distribution of budget and assures that resources are constantly allocated to the highest priority activities.

REPORTING RESULTS

One logical outcome of a successful team effort in research is a team report. A report of progress or of final results is generally required by whomever provides the funding. The preparation of this report should involve all members of the team and be a team activity.

Scientific publications pose a different question, however. Those individuals who have contributed in the data collection, and made a significant effort in the analysis and interpretation of results should be included as authors. This may not include all team members. Other members of the team who have given suggestions or reviewed the publication in draft form can be recognized for the specific contribution made to each paper. One logical approach used by many authors in or out of teams is to include those who have made a significant contribution to the paper, in descending order according to the nature and magnitude of the contribution. The individual who takes primary responsibility for writing the article is generally the senior author, unless there is some arrangement to the contrary. In most cases, it is better to "over-author" in team publications than to "under-author", to include an individual if there is any doubt. This type of co-authoring calls attention to the nature of the interdisciplinary effort and recognizes the importance of the team interactions in reaching goals and summarizing and interpreting results.

CREDIBILITY AND ACCOUNTABILITY

The credibility of an interdisciplinary team depends upon the success of the group in achieving results and on the correspondence of these results to the team's stated objectives. This success has several aspects. The team's competence in efficiently using available resources to accomplish objectives generates respect from colleagues, administrators and sponsors. Especially if participation in team research is a part-time activity, each member's use of time in relation to other responsibility contributes to that person's credibility as an individual scientist and as a dedicated team member. Periodic communication with disciplinary colleagues about those assets of the team's activities which are most closely associated with that scientist's home discipline will help maintain stature as a specialist in that area.

Accountability of a research team's activities involves the regular and competent reporting of results and progress to the agency, board, or other group responsible for committing resources to the research. Sponsors need to justify the decision that an interdisciplinary approach was indeed the best route to achieve the research objectives. The close correspondence of results with defined objectives adds to the team's credibility. Likewise, the careful articulation of how resources were managed, how collaboration among disciplines was maximized and how facilities and manpower were efficiently used will support the individual and collective reputations of team members. In addition, the routine presentation of publications in major journals also indicates the group's success in reaching scientifically credible and reportable conclusions.

EVALUATION

An objective and critical evaluation of any research activity is difficult since the process of evaluating research is not standardized. Commercial activities can more easily be evaluated by looking at the bottom line, but this is not appropriate for most research organizations of the government or industry. The indicators most often used to evaluate the success of research in the current system include the number of publications, the number of papers presented, or the meetings with client groups. Scientists are reluctant to evaluate themselves and and their programs in terms of the advances in sorghum or swine production over a period of years. However, in the state of Nebraska, for example, increasing production is the stated mission, and most applied research should help increase the productivity of specific crops or animal species.

It is important that the results of a team's activities be evaluated according to the objectives which were outlined for the team at the outset. This evaluation procedure should ideally be defined before the research is initiated. If the evaluation is to be based on number of publications, this should be clear. On the other hand, if the productivity of new cropping systems for a certain state, region or country is the objective, evaluation should be based on identifying the means to design and implement those systems.

In addition to evaluating the team, there should be some provision to evaluate the unique contribution of each member of the team. Possibly the needs for certain types of input have changed. Perhaps an individual representing a certain discipline is overloaded and three scientists are needed rather than one. Evaluation offers an opportunity to reiterate the specialties required for the research and to assess the actual relationship or contribution of each to the team's success in reaching its goals. This is a part of the ongoing evaluation process, and it requires the team to internally review its objectives and its efficiency in reaching them.

REWARDS AND RECOGNITION

In order to provide an incentive for outstanding achievements in team research, both individuals and the team need to be recognized and rewarded for good performance. Whether in the form of salary increases, promotions, public recognition, or special awards, this type of incentive is critical to the success of any scientific research activity. In recognizing the achievements of a research team, it is important to both reward the team as a group and to single out individuals for any unique contributions. Individual recognition may come through the presentation of discipline-specific papers in technical journals or society meetings, in which either the individual's own contribution to the team or the entire team's results are recognized.

As mentioned earlier, the success of a team will depend to some degree on the priority which each individual places on his or her time with the team, and on the extent to which this activity competes with or complements other activities in which he or she is involved. The rewards built into the team activity must be consistent with the overall reward system of the organization. An individual is unlikely to perform efficiently with a group if the main recognition is a "certificate of appreciation" for efforts, while a "lone wolf" colleague in the same department is rewarded for individual endeavors with a promotion or raise in pay. In both team and individual research, a just and uniform system of recognizing good research must be established. If team activities in some research areas are high priority, it should be to a scientist's advantage -- and not a detriment to progress -- to be an active member of the team.

TRAINING

The period of graduate student training is a formative one for most young scientists. Depending on age and previous experience, this will often be the first intensive and structured experience in research. The organization and orientation of the university's, department's and major professor's program will likely have a lasting effect on the outlook of a potential scientist, as evidenced by the frequent statement, "Now the way this is done at Cornell is...". If the graduate student works exclusively with the major professor, as opposed to collaborating with others, the student will probably emulate the major professor's approach to research. That professor's methodology will be preserved, to some degree. Such is the "apprenticeship" method of advising graduate students.

If, on the other hand, the professor is engaged in activities which involve other specialties and departments, there is a good chance that each student will emerge from the research "apprenticeship" with a positive feeling about the potentials of collaborative research. Although sometimes structured informally, the frequent collaboration between breeder and pathologist, breeder and entomologist, or biochemist and animal nutritionist are examples of this type of research association which is often successful in the university context.

One example illustrates this potential. A comprehensive activity has been proposed in the Title XII Sorghum project. A number of specialists in sorghum research from various departments and disciplines have planned a formal seminar/field orientation for potential leaders of research from the tropics. In this course, the team will present results and methodology used in the different disciplines to achieve results leading to increases in yield and will use field experiences to illustrate the benefits of working together to solve problems. Additional topics will include the development of an information base for the crop, use of this base to determine research priorities, budgeting of resources to best achieve yield increases, and other integrative topics designed to prepare our current graduate students from foreign countries to be leaders of their respective research programs in the near future. This is the type of training that may be most needed. Unfortunately, our current training approach of apprenticeship within one discipline falls short of what is needed.

The team thesis approach is a logical extension of this concept. This model has been tried at Kansas by a team of students at the M.S. level and at Cornell at the Ph.D. level. Several graduate students were financed by the Rockefeller Foundation and CIMMTY to spend part of their thesis research time in Mexico working together in a production zone with farmers and on the nearby experiment station with experimental activities. Although each worked on an individual, discipline-specific topic, all were united in the objective of increasing yields in the zone by overcoming the principal constraints to production. This project is an attempt to train people to work in teams. Evaluation, when the time is appropriate will include this dimension of training.

SUMMARY -- POTENTIALS FOR TEAM RESEARCH IN THE UNIVERSITY

There are many opportunities within the university community for effective interdisciplinary research. With existing resources and staff in the university, it is possible to put together an integrated team, consisting of specialists in breeding, agronomy, soils, entomology, pathology, engineering, biochemistry and economics (among others) to identify the limiting constraints to production in the state and how they could most efficiently be overcome. For example, increased efficiency from integrated activities is possible when two or more scientists combine field plantings of predominant Nebraska crop species. Many such possibilities for integrated crop improvement efforts exist for maize, sorghum, wheat and soybeans. Additionally, the role of each of these crops in a long-term, sustained cropping system for each region of the state could be evaluated, thereby justifying the relevance and the direction of the improvement program. As yet another example, energy use efficiency in a series of alternative crop/ animal systems, could also be evaluated by such a team.

Plateaus in yield have been reached in many crop species, and new approaches are needed to reach new levels of productivity. Whether the goals of research are increased productivity, sustained levels of production in the state, or the reduction of production costs, the constraints to reach these goals are complex. It will take the dedicated and integrated efforts of specialists, working both together and as individuals, to find solutions. With resources limited, greatest research productivity requires their efficient use and necessitates rational decisions on each project for most effectively meeting the priority objectives. In many cases, solutions can come from the activities of interdisciplinary teams.

Problem Choice in Agricultural Research: Scientists' Initiatives*

William B. Lacy and Lawrence Busch

The disciplinary backgrounds and affiliations of agricultural scientists influence how they conduct their research. This paper identifies several major differences among fields of agricultural science in the choice of problems, the timing of research, the interaction of scientists with professionals and students from other fields, and patterns of publishing results. The authors suggest that disciplinary standards have replaced organizational standards in assessing the quality of scientists' work.

Agricultural research in the United States arose more in response to a variety of interest groups rather than to demands or concerns of scientists. By the end of the first decade of this century, however, agricultural scientists began to organize themselves along both disciplinary and institutional lines. This paper explores the complex differences that have developed among disciplines and the consequences for research.

Scientists in each of the disciplines have different perspectives and work within a somewhat different world, composed of a group of related research areas. These research areas are not spatial units nor are they necessarily particular objects in the everyday world. Instead, as Richard Whitley noted, "...a research area can be said to exist when scientists concur on the nature of the uncertainty common to a set of problem situations. Some agreement on what is problematic and what can be assumed is essential" (1974). Consequently, scientists in different research areas will see the same thing differently. For example, a corn pathologist, a corn breeder, a corn entomologist, and an animal nutritionist will each see a different scientific object when confronted with an ear of corn.

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*Excerpted from, Busch and Lacy, Science, Agriculture and the Politics of Research. Boulder, Colorado: Westview Press, forthcoming, 1983 by permission of the publishers.

At the disciplinary level a variety of assumptions are tacitly shared. Traditionally, each of the disciplines tends to see most -- if not all -- problems as resolvable through the body of knowledge and the approach offered by their discipline. Other solutions are often viewed with skepticism if not outright disbelief. The situation is analogous to an attempt to see a picture on a wall in a darkened room. One may utilize a white light, a blue one, a yellow one, etc. to illuminate the room. One may even mix colors or change them in sequence. However, one does not have the option of utilizing no color at all in an attempt to see the room as it really is.

Similarly, scientists may "illuminate" the world through their respective disciplines. They may "mix colors" by engaging in interdisciplinary research, and they may "change colors in sequence" by asking colleagues in other fields how they confront a particular problem. What they may not do is see their object of inquiry as it really is. The "facts" differ according to the scientist's theoretical perspective. As Einstein observed, "It is theory which decides what we can observe" (Mahoney, 1979). Indeed, the objects scientists see are in large part created by their disciplines (Knorr and Knorr, 1978; Latour and Woolgar, 1979). As Ravetz puts it, "the objects of scientific inquiry are of a very special sort: classes of intellectually constructed things and events" (1971). What concerns us in this paper, then, is how scientists in different disciplines come to see the world differently and how that affects problem choice.

PREVIOUS STUDIES

The broad organizational structure of the agricultural sciences (described in detail in Busch and Lacy, 1982, Chapter 2) provides the context in which key decisions and choices are negotiated. One of the critical decisions for these sciences, as well as for scientists' careers, is the selection of research problems. Little systematic work has been conducted to explore this process. Recently, however, researchers studying science have begun to adopt the position that problem choice must be a central issue in studies of scientific development (Busch and Lacy, 1981; Edge and Mulkay, 1976; Giergy, 1978; Weinstein, 1976; Zuckerman, 1978). The current investigations of research problem choice have taken basically four approaches.

Some researchers have examined problem choice in the sciences by exploring how problems become defined as central or peripheral. Zuckerman, in summarizing some of these studies, concluded "that scientists define some problems as pertinent and others as uninteresting or even illegitimate, primarily on the basis of theoretical commitments and other assumption structures" (1978). According to this position, theory and its associate concepts can preempt research attention in numerous ways: by defining certain observations as irrelevant; by specifying certain topics as unfeasible; by defining certain areas as not problematic; and by directing attention from certain issues. As Kenneth Burke observed, "a way of seeing is also a way of not seeing--a focus upon object 'A' involves a neglect of object 'B'" (quoted in Zuckerman, 1978).

Another approach to this important dimension of science has been to treat problem choice as a collateral issue, focusing primarily on the emergence of new science specializations. Often the background, social characteristics and research experiences of scientists are examined to discover patterns among scientists entering certain fields or specialties (Edge and Mulkay, 1976). Related to this approach is the identification of sequences of change and continuity in the problem choices of scientists. Here the issues have involved the social and intellectual conditions which have influenced continuity and change for research problems over the long term (Gieryn, 1978).

A third equally important research perspective regarding problem choice focuses on how scientists choose from the range of identified problems. Studies using this orientation have explored the determinants for problem selection (Busch, Lacy, and Sachs, forthcoming; Edge and Mulkay, 1976; Zuckerman, 1978). Several criteria have been suggested as the major determinants for problem choice. Zuckerman (1978) concluded that the two main criteria utilized in selecting research problems were (1) the assessed scientific importance of a problem, and (2) the feasibility of arriving at solutions.

The importance of avoiding error prone fields and focusing on solvable research problems has been stressed by Medawar (1967:

> "No scientist is admired for failing in the attempt to solve problems that lie beyond his competence. The most he can hope for is the kindly contempt earned by the Utopian politician. If politics is the art of the possible, research is surely the art of the solvable... <u>Good scientists study the most important problems they think they can solve</u>. It is, after all, their professional business to solve problems, not merely to grapple with them. The spectacle of a scientist locked in combat with the forces of ignorance is not an inspiring one if, in the outcome, the scientist is routed. <u>That is why some of the most important biological problems have not yet appeared on the agenda of practical research</u>" [emphasis added].

The interplay of scientific or paradigmatic criteria and social factors in problem choice is complicated, since judgments regarding appropriate research are also influenced by social processes internal to science. Merton noted that research problem choice may be influenced by "reactions to the inferred critical attitudes or actual criticism of other scientists and by an adjustment of behavior in accordance with these attitudes" (Merton 1970).

In addition, the social system of science provides institutionalized motivation and reward for achieving priority in solving major scientific problems. This reward system motivates large numbers of scientists to migrate to interesting and substantively important emerging areas often referred to as "hot topics". Sullivan, White, and Barboni (1977) noted that physicists try to maximize the chances of both achieving priority and solving significant problems. This emphasis on rewards may increase the motivation of scientists to choose research which has a high probability of being published in professional journals.

Lastly, in contrast to issues internal to the discipline, other researchers have raised the issue of important extrascientific influences on problem choice. Merton (1970) in his analysis of science in seventeenth century England concluded that research problem choice followed directly from intrinsic scientific and technical developments and indirectly from scientific concern with extrinsic military, economic, and technological problems. Weinstein (1976) has stated this more forcefully by arguing that the intrinsic scientific or paradigm related criteria constitute only one of four important groups of criteria for problem choice. The other three determinants of problem choice are based on dimensions of social action and consist of administrative directives, political commitment, and personal avocation. Administrative criteria refer to standards external to the scientific discipline or institution, but relevant to some other public or private institution.

Some argue that the key criteria for problem choice may simply be the research agendas established by those institutions providing the funding. For example, C. Wright Mills stated that since many studies in the social sciences are "quite expensive, they have had to be shaped by some concern for the problems of the interests that have paid for them" (1959:64). Similarly, Fujimoto and Kopper (1975) and Evenson and Kislev (1975) have proposed that the most important external influences upon agricultural scientists' research choices are various commodity groups which financially and politically support selected aspects of agricultural research.

Frequently, those who accuse scientists of prostituting themselves for grants and other benefits suggest alternative motivations that are equally non-scientific but involve political commitment. Weinstein (1976) noted that even natural sciences may utilize problem selection criteria which involve some political values. For example, the decision of a biologist to concentrate in the sub-field of ecology may be based on a value commitment to improving the quality of the environment. Similarly, a decision to refrain from certain types of genetic research may be based on the assumption that the results could be used for immoral purposes by certain powerful groups.

Finally, Weinstein notes that a scientist may choose a research program for personal reasons, such as indulging one's psychological hang-ups or turning a hobby into a professional service. Busch and Lacy noted in an earlier paper (1981) that a scientist's non-scientific interests and life-style may influence research problem choices. During an interview, one agricultural scientist, who engaged in a great deal of field research, indicated that his choices were greatly influenced by his desire to work outside. Likewise, a plant pathologist reported that his research interests developed from his hobby of ornamental horticulture.

A major debate regarding which of these criteria operate in the sciences has emerged among researchers studying science. This brief summary suggests that scientists' choices of research topics are influenced in a wide variety of ways by a diverse array of factors. Some of these criteria are clearly disciplinary in character; others represent criteria that are external to science; still others are influenced by both internal and external factors. Frequently, two or more criteria influence the choice of research problems, and it is difficult to judge which is the most important. Decisions made by scientists regarding problem choice are seldom the result of single-minded adherence to any particular criterion, but emerge from a complex process of prioritizing influences for each scientist, as well as among other scientists, administrators, and clients (Busch, 1980).

PROCEDURES

To examine the disciplinary differences and the criteria utilized by agricultural scientists in this complex process of selecting research problems, we began with issues identified in this literature. Added to this background were perspectives gained through a series of semi-structured interviews with key informants in the agricultural science fields of agronomy, agricultural engineering, animal science, biochemistry, entomology, food science, forestry, genetics, horticulture, nutrition, phytopathology, and rural sociology (Busch and Lacy, 1981).

From this series of in-depth interviews, we developed a two-hour mail questionnaire which included detailed information by discipline on educational background, research style and orientation, scientific communication and criteria for problem choice. Finally, a corrected random sample of scientists listed in the Current Research Information System (N = 1,876) was obtained using techniques developed by Dillman et al. (1974). These scientists were surveyed utilizing this mail questionnaire, and a 76% response rate was obtained (N = 1,431).

EDUCATIONAL BACKGROUND

Disciplinary differences are particularly marked in the educational background of agricultural scientists. Although scientists in a few fields have broad backgrounds, most others have had little exposure to other disciplines. In fact, the modal or most common career path in eight out of sixteen disciplines is to take all three degrees in the same field (Table 1). This is true especially for agricultural economists and social scientists. Most agricultural scientists are also characterized by the relatively small proportion of all degrees that were obtained from fields other than that in which they are employed (Table 2). For example, 90% of all degrees obtained by agricultural engineers were in engineering. Among agronomists, 75% of their degrees were obtained in agronomy, while 11% were in the basic sciences. A similar pattern held for animal science and entomology.

Virtually no crossover was found between agronomy and animal science.

On the other hand, crop scientists, environmental scientists, food scientists, and nutritionists most frequently take all of their degrees in disciplines other than the one in which they practice. This diversity in educational background for these fields results from very different circumstances. At the bachelor level, most crop scientists have degrees in general agronomy, a specialty which has evolved over several decades and is roughly equivalent to crop science. On the other hand, environmental and food science are relatively new fields, therefore, it is not surprising that few current practitioners have degrees in these fields. Finally, nutritionists tend to have their training from several different basic sciences which include biochemical perspectives fundamental to nutrition.

Some scientists trained in their own fields receive this training at graduate rather than at undergraduate levels. Soil scientists, for example, typically enter training for that field at the masters level, while most plant pathologists and geneticists enter at the doctoral level. Limited course offerings in these fields at the lower levels may partially account for this pattern and may reflect the academic labor market demand for scientists with that training.

Only a small number of scientists in the basic sciences were trained in fields other than the basic sciences. At the same time, basic scientists provide training for scientists in virtually all the other biological fields. The environmental sciences tend to draw scientists from the most diverse backgrounds, including the basic sciences and zoology. The animal

Table 1. Educational Background of Scientists by Discipline

Discipline	Percent of Scientists with All Degrees in Same Discipline	Percent of Scientists with One Degree in Another Discipline	Modal Educational Path[a]						
			Bachelors		Masters		Doctorate		
			In	Out	In	Out	In	Out	%[b]
Ag. Econ.	53	39	X		X		X		43
Agricultural Engineering	72	14	X		X		X		69
Agronomy									
All	53	26	X		X		X		45
Crop Science	8	15		X		X		X	41
Soil Science	29	37		X	X		X		29
Animal Science	56	21	X		X		X		51
Basic Sciences									
Biochemistry	6	22		X		X		X	39
Biology	32	14	X		X		X		31
Chemistry	57	4	X		X		X		36
Genetics	3	14		X		X	X		43
Entomology	41	35	X		X		X		33
Environmental Sciences	7	23		X		X		X	46
Food Science	8	26		X		X		X	45
Forestry	62	13	X		X		X		45
Horticulture	50	20	X		X		X		41
Nutrition	17	28		X		X		X	27
Plant Pathology	11	37		X		X	X		41
Social Sciences	37	35	X		X		X		33

[a]X's denote whether the modal educational path taken is within a given discipline or outside it.
[b]Indicates percentage of scientists following modal path.

Table 2. University Degrees Obtained by Agricultural Scientists, by Current Field of Science and Discipline[a]

Current Field of Science	Discipline in Which Degree Obtained (%)																			
	Ag. Economics	Ag. Engineering[c]	Agronomy	Animal Science	Basic Sciences	Entomology	Environ. Sciences	Food Science	Forestry	Horticulture	Nutrition	Plant Pathology	Ag. Education	Social Sciences	Gen. Agriculture	General Science	Humanities	Zoology	Miscellaneous	Total Degrees Obtained
Agricultural Economics	74	1	1	3	d	0	d	d	d	d	4	0	4	2	4	1	2	0	1	307
Agricultural Engineering	2	90	2	0	2	0	1	0	2	0	0	0	0	0	1	1	0	0	1	205
Agronomy	1	d	75	1	11	d	1	d	1	2	0	1	3	1	4	1	d	d	0	745
Animal Science	0	d	0	74	7	1	2	1	0	0	3	0	2	1	7	0	0	4	0	370
Basic Sciences	d	1	8	8	64	1	d	1	2	3	d	2	0	1	1	2	d	4	d	492
Entomology	0	0	2	1	12	70	d	0	2	0	0	0	d	0	1	1	1	8	0	246
Environmental Sciences	1	5	5	6	15	4	31	0	8	3	0	1	0	3	3	1	1	15	2	156
Food Science	3	8	2	18	23	0	1	31	0	3	5	0	0	1	3	2	0	1	1	132
Forestry	2	5	6	4	2	3	0	0	77	0	0	1	0	0	0	d	0	d	d	203
Horticulture	1	0	10	d	10	0	0	d	d	70	d	1	3	0	2	0	1	0	0	240
Nutrition	1	1	3	12	18	0	0	6	0	0	44	0	1	9	5	1	0	0	0	114
Plant Pathology	1	0	9	d	20	1	0	0	3	7	0	52	2	1	3	0	d	0	0	244
Social Sciences[b]	8	0	0	0	1	0	3	0	1	0	0	0	9	68	1	0	7	0	2	130

[a] Includes all graduate and undergraduate degrees; postdoctoral study is excluded.

[b] Includes Agricultural Education.

[c] Includes general engineering degrees.

[d] One-half of one percent or less.

science and basic science backgrounds of many food scientists and nutritionists reflect the biochemistry basis for research in those fields. Both agricultural economists and social scientists tend to have little training or professional contact with other fields.

In short, while some scientists have been trained in several fields, the majority of agricultural scientists obtain little exposure to fields not closely allied to their own. As one frustrated industry spokesperson observed:

> "There has been no real attempt on the part of graduate schools to teach students how to work on a multidisciplinary team. The language is different, the method of approach to problems is considerably different, and, for all practical purposes, it is difficult for members of a team to be able to communicate among themselves" (Bauman, 1979).

This trend is accentuated by accreditation criteria. In fact, so many demands for disciplinary sources are made by the disciplinary societies which accredit agricultural curricula that basic education in the humanities and social sciences is sometimes neglected (Kellogg and Knapp, 1966). Promising exceptions are the recently developed programs in integrated pest management.

TIME BUDGETING

The separate worlds of agricultural scientists become particularly evident when one examines the differences among the disciplines in the allocation of research time. For example, scientists in the various plant sciences spend a quarter or more of their research time out in the field. In contrast, those in the basic sciences, food science, and nutrition spend about the same amount of time in laboratory work. Similarly, horticulturists and plant pathologists conduct much of their research in greenhouses. Finally, social and economic scientists spend a great deal of their research time in their offices (Table 3).

These statistical data allude to several fundamental differences in the way research is perceived and carried out in each of the agricultural disciplines. For example, scientists in the basic sciences conform most closely to

Table 3. Percent of Research Time in Each Location by Discipline[a]

	Location						
Discipline	Office	Library	Field	Lab	Computing Facility	Greenhouse	Elsewhere
Ag. Economics	76	6	10	0	4	0	4
Ag. Engineering	50	7	18	14	6	1	2
Agronomy	43	6	27	14	2	5	2
Animal Science	47	7	16	22	3	0	4
Basic Sciences	41	10	8	36	1	2	2
Entomology	39	6	23	24	2	5	1
Environmental Sciences	42	8	28	16	4	0	2
Food Science	48	10	6	33	1	0	1
Forestry	53	8	18	15	4	2	1
Horticulture	36	7	30	13	3	8	2
Nutrition	38	13	12	27	6	0	3
Plant Pathology	33	6	24	22	2	10	2
Social Sciences	67	11	14	1	3	0	3

[a]Numbers may not add to 100 due to rounding.

the public image of the white-coated lab scientist. They spend most of their time in the lab and use their offices primarily to write up the results of their research. This may be contrasted with agricultural economists, three-quarters of whose time is spent in the office. Indeed, much agricultural economics research involves the statistical analysis of information gathered elsewhere and printed in tabular form, and the office itself may be a research site.

Special characteristics of the research also determine differences in the speed with which research can be accomplished. Biological field research is restricted not only by the objects being studied but also by the exigencies of weather, the distance necessary to reach research sites, and the wide variations found in field settings. At the other end of the spectrum, other limiting factors characterize laboratory analysis in soils research or the economic analysis of statistical data. These factors influence the differential budgeting of time on research in various fields.

Time budgets imply certain characteristics about the nature of the researchers themselves and their personal orientations. For example, it is likely that those who "love the great outdoors" would tend to enter the agronomic/crop sciences. In contrast, those who find heavy labor, the hot sun, dirt and grime unattractive are more likely to go into the basic sciences. Other fields, such as entomology and plant pathology offer scientists the opportunity to do either field or laboratory work or both.

Interestingly, scientists' time budgets appear to change over the course of their careers. In Harmon's study of doctorate cohorts in all the sciences 1935-1960, he found that the distribution of time for research, teaching and administration changed radically during scientists' worklives (Harmon, 1965). The most marked trend was the rapid growth of administrative responsibility chiefly at the expense of research time. He reported that 25 years after completing the doctorate, administrative activities occupied the greatest single segment of scientists' time.

This trend was evidenced in our sample also. The oldest cohort reported 21.3% of their time devoted to administration and 52.5% of their time devoted to research. This percentage of research time for this cohort was the lowest of all age groups. On the other hand, the cohort receiving their doctorates in the last four to eight years spent 5.7% of their time on administration and 63.3% on research. Despite the fact that all of these agricultural scientists were active researchers with projects listed in the Current Research Information System, the trend toward older scientists spending less time in research was clear. If the median age of agricultural scientists continues to increase during the next few decades and if this trend to increased administrative activities with age also persists, total research time may well be reduced.

CRITERIA FOR PROBLEM CHOICE

Our mail survey included 21 criteria for scientists' choice of research problem (Table 4). "Enjoyed doing this kind of research" emerged as the single most important criterion for research problem choice. The second most important criterion was "importance to society". However, personal interviews and correlation analysis suggest that this criterion glosses over a rather heterogenous group of meanings. Specifically, "importance to society" is very highly correlated with the "likelihood of clear empirical results" and the "marketability of the final product". Moreover, while scientists rate "importance to society" highly as a criterion for problem choice, they tend not to see "community improvement" or "improved level of living" as equally important research goals (Busch and Lacy, 1982).

The third item in Table 4 underscores the importance of available facilities to the conduct of the research. Clearly, no research can be undertaken unless space, equipment, libraries, and other paraphernalia of research are present.

Scientific curiosity ranked fourth among all scientists as a criteria for problem choice. As one respondent rhetorically asked,

> "The scientists' dream is science for the sake of science. Almost everything and everyone a scientist has to deal with is the antithesis of this. Who is wrong?"

In contrast, the potential creation of new methods, useful materials and devices was ranked fifth. Obviously, many agricultural scientists do see the development of substantive products as a central feature of their research.

Not surprisingly, the probability of publication in professional journals was rated high on the list of criteria. In contrast, the probability of publishing in bulletins and in farm and/or industry journals ranked 19 and 21 in the list of criteria. One scientist pointed out that the emphasis on journal publication has contributed to increasing specialization in the agricultural sciences:

> "I am all too familiar with theoretical researchers who have played the "publish or perish" syndrome to the hilt, but have not contributed one bushel increase to crop yields. These people, usually department heads or location leaders, have related

> almost none of their papers to the consuming public. The 'generalist' is almost an extinct species, and we will regret this situation in the future."

The importance of communication with clients in the choice of scientific research problems is reflected in three items, ranked 7, 13 and 20. Of particular note here is that scientists' personal assessments of client needs were considered substantially more important than either the demands raised by clientele or the feedback from extension staff. Of perhaps even greater importance was the strong positive relationship between the percent of one's time alloted to extension activities and the relative ranking given to the above items. Feedback from clients and extension staff appears most influential in shaping research decisions when researchers have a portion of their time devoted to extension activities.

Table 4. Criteria for Research Problem Choice

Rank	Criterion	Mean Score[a]
1	Enjoy doing this kind of research	5.86[n]
2	Importance to society	5.59[o]
3	Availability of research facilities	5.24[p]
4	Scientific curiosity	5.13[pq]
5	Potential creation of new methods, useful materials, and devices	5.03[qr]
6	Publication probability in professional journals	4.89[rs]
7	Client needs as assessed by you	4.87[rs]
8	Likelihood of clear empirical results	4.76[s]
9	Funding	4.75[s]
10	Evaluation of research by scientists in your field	4.34[t]
11	Priorities of the research organization	4.29[t]
12	Potential contribution to scientific theory	4.26[tu]
13	Demands raised by clientele	4.10[uv]
14	Credibility of other investigators doing similar research	3.95[vw]
15	Currently a "hot" topic	3.84[wx]
16	Length of time required to complete the research	3.79[x]
17	Potential marketability of final product	3.74[x]
18	Colleagues' approval	3.74[x]
19	Publication probability in experiment station or research service bulletins and reports	3.64[x]
20	Feedback from extension personnel	3.49[y]
21	Publication probability in farm and/or industry journals	2.98[z]

[a]Mean score based on seven point scale (1 = not important, 7 = very important).

[n-z]Criteria with different letters are significantly different, $p < .01$.

These rankings suggest that formal feedback mechanisms are relatively ineffective in directing research. With somewhat less clarity, they also suggest that scientists are more likely to rely on a common sense assessment of client needs rather than on a careful inquiry into the problems experienced by various client groups. An alternative explanation would be that scientists tend to take a broad, long-run view of research needs. However, many scientists complained of the short term character of most current research. According to one scientist,

> "Short term technological solutions only put off (delay) the immutable natural consequences. In general, the research in my field is short sighted in its attempt to increase productivity and the farmers' (and others') profit. As in most ecological systems an equilibrium solution is the only one which persists."

Importantly, virtually none of our respondents was concerned that long term research was overemphasized.

Funding ranked ninth among the 21 criteria but not significantly different from the sixth criterion (Table 4). Several respondents, however, felt that funding, and particularly the search for extramural grants, was greatly overemphasized. The following response captured much of that sentiment:

> "I strongly feel that so much attention has been paid to funding, to grants, and contracts, that the fundamental purpose of scientific research -- to increase organized knowledge and how to use it -- has suffered greatly. I do not hear colleagues talk of break-throughs or strong inferences anymore, only about 'this wonderful new program' or the 'number of Ph.D.'s we have turned out' or 'how we plan to expand into this or that'. The specific objective of the research is seldom mentioned, and when it

is, it sounds like more of the same old language: 'Improve our understanding of...,' 'study the relationship between...,' and 'build a model to account for....' All vague, all open-ended -- not a specific goal in sight. Program funding has become the objective of scientific research!" (emphasis in original)

Priorities of the research organization ranked 11 among the 21 criteria. As one respondent put it,

> "Most research in agricultural experiment stations is done by individuals with almost no directions from directors of stations. There is no priority 'plan' for (name of state). The department head is influential, however" [Emphasis in original]

Most scientists ranked the popularity of "hot topics" relatively low on their list of criteria for problem choice. While a small group did see this as an important criterion for problem choice, the majority of scientists appear to have taken a position similar to this:

> "I am interested in exploring the principles of important reactions and processes that are associated with soils. My aim is to test hypotheses that I have formulated, regardless of how unpopular they may be. I believe that, in so doing, the knowledge I accumulate will ultimately benefit my discipline and mankind. Therefore, I am not interested in pursuing topics that are currently hot or that will receive generous financial support unless they coincide with my interest and goals."

Finally, as suggested earlier these criteria emerge from a complex process of negotiation and often are operating together to shape research problem choice. As one scientist observed:

> "My choice of research areas is based on a mix of my perception of important limiting factors in yield and quality of my crops, my abilities and interests, availability of facilities and type of facilities, and availability of extramural funding that is consistent with the above."

DISCIPLINARY DIFFERENCES IN PROBLEM CHOICE

Responses to our survey indicate that scientists from different disciplines tend to define, choose, and execute research in quite different ways. In part due to training, in part due to reward systems, and in part due to peer interaction, scientists differ markedly across disciplines in the criteria they use for problem choice.

For example, confronted by the problem of low agricultural productivity in a given locale, an engineer would be likely to focus on tillage, drainage, and planting and harvesting practices; an economist might explore the availability of farm markets and credit; an agronomist would be likely to check the yield potential of the cultivars being grown and their rates of fertilization; and a sociologist might inquire into the role of the farm family as a source of labor, wealth and risk-taking behavior. Of course, any or all of these issues might be relevant to a given situation. Nevertheless, disciplinary differences would (and do) have a major impact on the way problems are chosen and defined. This is apparent from the marked differences in the relative weight given to 15 out of 21 criteria for problem choice by scientists from different disciplines (Table 5).

The single most important criterion for research problem choice, "enjoyed doing this kind of research," did not differ significantly across fields. However, the second most important criterion, "importance to society," did vary. Nutritionists rated this item most highly, and those in the basic sciences gave it the lowest score. Not surprisingly, those in agricultural economics and the social sciences, whose research requires smaller capital expenditures, were far less concerned about the availability of research facilities than those in biological sciences or engineering.

Importantly, scientific curiosity was ranked most important by nutritionists, followed by basic scientists and social scientists -- all fields traditionally separated somewhat from "production agriculture". Agricultural economists and agricultural engineers ranked scientific curiosity least important. On the other hand, the potential creation of new methods, useful materials and devices was ranked high by foresters, food scientists, and agricultural engineers, reflecting the product orientation common to these fields. Those disciplines lacking in material products -- agricultural economics and the social sciences -- scored lowest on this item.

Nutritionists and entomologists were most concerned about publication in professional journals ($\bar{x} = 5.3$) while agricultural economists were least concerned ($\bar{x} = 4.0$). Indeed, unlike those in all other fields, agricultural economists rated publication in experiment station or research service bulletins as far more important than publication in professional journals. While the means did differ among disciplines,

Table 5. Selected Criteria for Research Problem Choice by Field of Science[a]

Discipline	Criterion: Importance to Society	Availability of Facilities	Scientific Curiosity	Creation of new Methods, Devices	Publication in Professional Journals	Client Needs as Assessed by You	Likelihood of Clear Results	Priorities of the Organization
Ag Economics	5.6^{z}	3.5^{x}	4.4^{v}	4.4^{v}	4.0^{x}	5.3^{yz}	5.4^{z}	4.5^{yz}
Ag Engineering	5.8^{z}	5.3^{z}	4.5^{vw}	5.6^{yz}	4.5^{xy}	5.4^{yz}	4.5^{xy}	4.7^{z}
Agronomy	5.7^{z}	5.5^{z}	5.2^{xyz}	5.2^{wxyz}	4.9^{yz}	5.2^{xyz}	4.8^{xyz}	4.4^{yz}
Animal Science	5.7^{z}	5.8^{z}	5.1^{wxy}	5.2^{xyz}	5.1^{yz}	5.4^{z}	4.6^{xy}	4.4^{yz}
Basic Sciences	5.0^{y}	5.3^{z}	5.5^{yz}	4.7^{vwx}	5.0^{yz}	3.2^{v}	4.8^{xyz}	3.8^{xy}
Entomology	5.5^{yz}	5.5^{z}	5.4^{xyz}	5.0^{vwxyz}	5.3^{z}	4.8^{wxy}	5.0^{xyz}	4.6^{z}
Environmental Sciences	5.5^{yz}	5.2^{z}	5.4^{xyz}	4.9^{vwxy}	5.1^{yz}	4.6^{wx}	5.0^{yz}	4.2^{xyz}
Food Science	5.6^{yz}	5.8^{z}	5.0^{wxy}	5.6^{yz}	5.2^{yz}	4.9^{wxy}	4.5^{xy}	4.4^{yz}
Forestry	5.6^{yz}	5.2^{z}	5.0^{wxy}	5.7^{z}	4.6^{xyz}	5.2^{wxyz}	4.8^{xyz}	4.2^{xyz}
Horticulture	5.7^{z}	5.5^{z}	4.7^{vwx}	5.2^{xyz}	5.2^{yz}	5.8^{z}	4.5^{xy}	4.6^{z}
Nutrition	6.0^{z}	5.6^{z}	5.9^{z}	4.6^{vwx}	5.3^{z}	4.5^{w}	5.0^{xyz}	3.8^{xy}
Plant Pathology	5.6^{yz}	5.4^{z}	5.1^{wxy}	5.1^{wxyz}	5.0^{yz}	4.8^{wxy}	4.4^{x}	4.4^{xyz}
Social Sciences	5.7^{z}	4.2^{y}	5.5^{yz}	4.4^{vw}	4.6^{xyz}	4.8^{wxy}	4.9^{xyz}	3.7^{x}

[a]Mean scores based on seven point scale (1 = not important, 7 = very important).

[v-z]Means within a column with different letters are significantly different, $p < .01$.

Table 5. Continued

Discipline	Contribution to Theory	Demands of Clientele	Currently a "Hot" Topic	Marketability of Product	Publication in Bulletins	Feedback from Extension	Publication in Farm Journals
	Criterion						
Ag Economics	3.1^{v}	4.7^{yz}	4.7^{z}	4.2^{z}	5.1^{z}	3.7^{yz}	3.3^{y}
Ag Engineering	3.7^{vw}	4.5^{xyz}	4.5^{z}	4.2^{yz}	3.7^{vwx}	3.8^{yz}	3.2^{y}
Agronomy	4.2^{wxy}	4.4^{xyz}	3.6^{wxy}	4.0^{xyz}	4.1^{xy}	4.0^{z}	3.1^{xy}
Animal Science	4.3^{wxy}	4.3^{wxyz}	3.6^{wx}	4.0^{xyz}	3.5^{uvwx}	3.9^{yz}	3.4^{yz}
Basic Sciences	5.1^{z}	2.9^{v}	3.6^{wx}	3.0^{w}	2.2^{t}	2.2^{v}	2.1^{w}
Entomology	4.5^{xyz}	4.1^{wxy}	4.0^{wxyz}	3.5^{wxy}	3.3^{uvw}	3.6^{xyz}	2.9^{xy}
Environmental Sciences	4.3^{wxy}	3.8^{wx}	3.7^{wxy}	3.3^{wx}	3.3^{uvw}	3.1^{wxy}	2.5^{wx}
Food Science	4.4^{xyz}	3.9^{wx}	4.4^{yz}	3.9^{xyz}	3.0^{tuv}	2.8^{vw}	2.7^{wxy}
Forestry	4.1^{wx}	4.4^{wxyz}	3.9^{wxyz}	3.9^{xyz}	4.0^{wxy}	3.1^{wxy}	3.2^{xy}
Horticulture	3.6^{vw}	5.0^{z}	3.5^{w}	4.6^{z}	4.6^{yz}	4.2^{z}	4.1^{z}
Nutrition	4.9^{yz}	3.6^{vw}	3.6^{wx}	3.0^{w}	2.9^{tu}	2.9^{vwx}	2.6^{wx}
Plant Pathology	4.3^{wxy}	4.1^{wxy}	3.4^{w}	3.6^{wxy}	3.4^{uvwx}	3.7^{yz}	2.9^{xy}
Social Sciences	4.7^{xyz}	3.8^{wx}	4.3^{xyz}	3.8^{wxy}	4.0^{wxy}	3.6^{xyz}	2.8^{wxy}

[a] Mean scores based on seven point scale (1 = not important, 7 = very important).

[v-z] Means within a column with different letters are significantly different, $p < .01$.

only horticulturists ranked publication in farm and industry journals above the mid-point on the scale.

Scientists from different fields also varied in the degree to which they considered client needs, client demands, and extension feedback as important criteria for problem choice. Horticulturists appeared to be the most client-oriented while those in the basic sciences paid little attention to clients. However, for each of the disciplines, scientists' assessments of client needs were considered substantially more important than either client demands or extension feedback. Agricultural engineers, followed by entomologists and horticulturists, ranked the priorities of the research organization most highly. Even among these fields, though, these priorities were relatively low on the list of criteria.

An important way in which the disciplines differ is in the topics they find to be of major current concern. To find out what these topics were, we asked each respondent, "What are the current 'hot' specialties within your discipline?" The major research topics so identified are presented in Table 6. It is immediately apparent that energy, environmental issues, and integrated pest management are three threads which link together many of the disciplines.

On the other hand, within each discipline, there appears to be substantial divergence of opinion over what the current hot topics are. Only among agricultural engineers did more than 50% of the respondents identify the same theme. Moreover, among food scientists, nutritionists, and social scientists, no consensus emerged as to what current hot topics were. This implies that substantial fragmentation may exist

Table 6. Major "Hot" Topics Identified by Discipline[a]

Topic	Number who Mentioned Topic	Percent of Total Respondents from this Field Who Mentioned Topic
Agricultural Economics		
Environmental quality	31	27
Energy	25	22
International trade	15	13
Agricultural Engineering		
New energy forms	43	51
Energy conservation	17	20
Water quality	14	17
Environmental quality	14	17
Agronomy		
Environmental quality	52	19
Nitrogen fixation	42	15
Minimum tillage	16	6
Energy conservation	15	5
Photosynthesis	14	5
Genetic manipulation	14	5
Modeling	13	5
Integrated pest mgmt.	12	4
Herbicides	11	4
Tissue culture	10	4
Animal Science		
Food quality	14	11
Protein utilization	12	9
Reproductive efficiency	12	9
Basic Sciences		
Genetic manipulation	43	23
Environmental quality	14	7
Tissue culture	10	5

Table 6. Continued

Topic	Number who Mentioned Topic	Percent of Total Respondents from this Field Who Mentioned Topic
Entomology		
Integrated pest mgmt.	35	37
Pheremones	14	15
Insect behavior	14	15
Modeling	10	11
Environmental Sciences		
Environmental quality	17	31
Food Science		
(no topics identified by more than 6 persons)		
Forestry		
Biomass energy	22	29
Forest management	12	16
Environmental quality	10	13
Horticulture		
Environmental quality	12	13
Tissue culture	11	12
Nutrition		
(no topics identified by more than 4 persons)		
Plant Pathology		
Integrated pest mgmt.	19	21
Disease control	13	14
Biological control	11	12
Soil pathogens	10	11
Social Sciences		
(no topics identified by more than 5 persons)		

[a]Respondents were permitted to write replies. Up to four responses were coded for each respondent.

within disciplines, in addition to the already noted relatively sharp disciplinary differences. However, further word on this issue must await case studies of particular disciplines and is beyond the scope of this paper. (For information on the different disciplinary perceptions of research goals and important issues in agriculture, see Busch and Lacy, 1982, Chapters 8, 9, 10.)

PEER REVIEW

Each discipline influences problem choice in research through its control of the scientific publication system. This is done, in large part, through peer review. Peer review is the favored method for selecting papers for journal publication. Review of a paper for a journal may take anywhere from two weeks to six months and is frequently followed by requests for revisions and publication delays. In contrast, bulletins are generally reviewed within the research organization in a somewhat more cursory, and undoubtedly less uniform, fashion.

Both the review process and the probability of having a paper accepted for publication varies markedly from field to field (Lacy and Busch, 1982). These differences in publication probabilities are reflected in the acceptance rates for each discipline, reproduced in Table 7. Agricultural economics and rural sociology stand out markedly for their very low acceptance rates. This data parallels that collected by Lindsay (1978) and Beyer (1978) which shows equally low acceptance rates in other social sciences and high ones in the natural sciences. Moreover, it is usually argued that this difference is due in large part to the higher degree of agreement in the natural sciences over the central tenets of the disciplines. Because of such consensus, there is less disagreement among reviewers as to what constitutes a valid contribution to the literature.

Our data suggest another reason for the disparity. The low level of reliance on page charges in the natural sciences and their non-existence in agricultural economics and rural sociology appears to limit the number of pages available for scientific publication, thereby reducing acceptance rates. (For additional information on the role of journals in disciplinary research and interdisciplinary communication, see Busch and Lacy, 1982, Chapters 4 and 5.).

IMPLICATIONS

In summary, several trends can be identified in the way in which disciplinary differences influence scientists' choice of research problems. First, members of each discipline employ somewhat different criteria in their choice of research problems. This is to be expected as it is part and parcel of the process of professionalization.

Table 7. U.S. Scientific Journal Publishing Policies and Procedures by Discipline[a]

		Journal Information				
Discipline	Number of Journals Surveyed	Average Number Manuscripts Submitted Annually	Average Review Time (Weeks)	Percent Accepted	Percent With Page Charges	Mean Number Reviewers
Ag. Economics	4	186	6	27	25	2.0
Agronomy	10	196	7	78	70	2.3
Animal Science	19	223	6	68	37	2.0
Basic Sciences	12	264	5	69	58	2.0
Entomology	15	126	8	81	86	2.1
Environmental Sciences	8	202	7	58	38	2.5
Food Science	5	238	4	79	40	2.0
Forestry	6	124	10	74	67	2.8
Horticulture	3	177	5	62	33	2.3
Nutrition	1	400	4	70	100	2.0
Plant Pathology	2	258	4	79	100	2.0
Rural Sociology	2	119	8	21	0	3.0

[a]No editors of Agricultural Engineering journals responded to the questionnaire.

Secondly, scientists do not recognize peer pressure as an important criterion for problem choice. This is evidenced by the relatively low scores given to "Evaluation of research by scientists in your field" (overall rank 10th), "Credibility of others doing similar research" (overall rank 14th), and "Colleagues' approval" (overall rank 18th) -- by scientists in all disciplines. In contrast, scientists are responsive to the reward structures and ideals of their disciplines. This is particularly evidenced by the generally high scores given to "scientific curiosity" and "publication...in professional journals".

Finally, the various disciplines differ markedly in their reliance on feedback from clients and extension staff, as well as in the product orientation which guides choice of research problems. Horticulturists are most responsive, while basic scientists are least responsive to clients and extension (as evidenced by ranking of criteria). Food scientists, agricultural engineers, and foresters are more concerned than other agricultural scientists with the possibility of new products.

In short, agricultural scientists tend to maintain fairly sharp disciplinary divisions in their educational backgrounds, research styles and orientations, publication activities and scientific communication, and criteria for research problem choice. Clear divisions appear to exist between the plant and animal sciences, as well as between the socio-economic and natural sciences. Moreover, longitudinal data on publications suggests that disciplinary standards have replaced organizational standards in assessing the quality of scientists' work.

This insularity among the agricultural sciences has enormous scientific and social significance. First, it suggests that disciplinary problems are likely to receive more support than those that cross over disciplinary lines (Ruttan, 1971). Such problems are more easily defined, easier to assess in terms of (disciplinary) significance, and more likely to contribute to (disciplinary) knowledge. However, this also implies that the stock of knowledge produced by each of the disciplines may become divorced from that of other disciplines.

A second implication is that by focusing on only those aspects of the world that are deemed relevant by a particular discipline, scientists may ignore problems that lie outside their competence. As Berry suggests, disciplinary insularity may give scientists the illusion that total control over agricultural phenomena is possible:

> "The specialist puts himself in charge of one possibility. By leaving out other possibilities, he enfranchises his little fiction of total control. Leaving out all the "non-functional" or otherwise undesirable possibilities, he makes a rigid, exclusive boundary within which absolute control becomes, if not possible, at least conceivable" (1977).

Thus, entomologists may exclude the effects of pesticides on people and animals from their research on pesticide effectiveness, or animal scientists may exclude the waste disposal issues raised by confinement feeding of animals. In short, once having divided the world for study, it may be difficult to reintegrate it.

Nagi and Corwin have noted that:

> "Different disciplines are closed to varying degrees, depending on the characteristic balance between a discipline's internal structure and the external pressures on it. In mature disciplines researchers use their established paradigms to identify central problems and the criteria for assessing the significance of findings.... Scientists in disciplines still lacking formal paradigms, however, often derive their research topics from social problems, and their criteria of signific[a]nce from quasiscientific measures (1972).

By these criteria, it appears that most of the agricultural sciences have matured. Yet, if such is the case, it may have come only at the expense of the creation of a barrier between scientists and farmers. Scientific research may have become uncoupled from the everyday world of farmers and other clients, such that much disciplinary knowledge is no longer congruent with their knowledge and needs. Minckler (1976) has made a similar case for forestry research. This, in part, may account for the reduction in the rate of increase in U.S. agricultural productivity over the last decade, and the sharp rise in soil erosion rates.

Finally, the increased disciplinary emphasis appears to be partially responsible for increased specialization on the part of farmers. Much like agricultural scientists, American farmers have tended increasingly to specialize in the production of a single crop or commodity, while paying scant attention to soil erosion, farm runoff, and other long-term problems. In fact, scientific agriculture has resulted in the adoption of scientifically validated processes and products by farmers, as well as in the transposition of the social organization of science to farming.

Thus, both science and farming have become increasingly rationalized over the last several decades. Yet, ironically, the ordering of science and farming may not be the only way or even the most effective way to provide food and fiber for the world. Indeed, rationalization of agricultural production may show us the limits of rationalization itself and of our ability to control the natural world.

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Improving Interdisciplinary Research Management to Help Faculty Scientists

Paul E. Waibel

Interdisciplinary research on total animal production systems will be more successful if excellent collaborative research facilities are developed, if sufficient labor, funding and instrumental backup are available for the research, and if studies are planned with realistic consideration of the converging demands on faculty time. This paper explores the influences of interdisciplinary collaboration on the workload of faculty scientists. Suggestions are offered for easing the time consuming technical aspects of research so that the efficiency and effectiveness of scientists' efforts in both disciplinary and interdisciplinary research may be assured.

If we were to look into a farm operation of 100 years ago, about the time when many of our experiment stations were founded, we would observe very low intensity farming as compared to now. As practitioners began to apply the results of research, the scientists increased their efforts to study limiting factors: these specialized inquiries were studied best by scientists trained in the appropriate areas of disciplinary expertise. In those earlier days, departments or divisions were organized more or less on a commodity basis and as they added staff they sought disciplinary specialists. As these disciplinary specialists strengthened their affiliations with each other the disciplines became stronger, and over time the disciplines have proven to be very effective in moving toward in-depth solutions to problems. Presently, the national meetings of poultry, animal and dairy scientists are organized and focus on a particular species. Most of these same scientists also maintain affiliation with disciplinary groups, i.e., nutrition, genetics, physiology, which hold most of their meetings with peers trained in like disciplines.

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There are often limitations in conducting interdisciplinary research (IDR) within a university system which has been oriented to department and discipline objectives. In some respects, these difficulties are no different than those encountered in any new effort -- budget constraints, administrative commitment. A critical component of IDR is the team of scientists. It has often been said that the people involved are the most important ingredient in bringing about successful ID cooperation. This is true both for voluntary cooperation between scientists and for research collaboration initiated by an administrative or sponsoring agency, e.g., the experiment station. The faculty scientist's primary resources in responding to such initiatives are research expertise and time. Most experiment station scientists work only part-time on research activities. The rest of their time is spent on teaching and public service activities.

Consider for example, a nutritionist who conducts production-research with turkeys. The research facility at which this research is conducted is located 26 miles away from office and classroom on campus, and virtually all of the research observations (12 months a year) must be collected by students and technical help who are also based on campus. This scientist maintains strong interaction with persons working with poultry farms and allied industries. Due to these contacts, the scientist's research program has attracted 34 research grants, totalling $422,950, during 1977-81. At the same time, a portion of the scientist's time has been devoted to involvement with an interdisciplinary research problem on the bone development of turkeys, which stemmed from a strong industry desire to approach field leg disorders from a multidisciplinary base.

This faculty scientist is responsible for the reports, planning, and execution involved in these formal contracts, plus many informal contacts, teaching 2 1/2 courses a year, managing a graduate student program, making presentations at home and abroad, writing, reading, and committee activities. Because of the convergence of demands, it is imperative that administrators consider the impact of faculty load and recognize that planning and time management are paramount to continued balanced performance.

One primary challenge to experiment station scientists is to balance research with teaching and public service functions. A typical research scientist in a department may draw 50-75% of salary from funds dedicated to research. In order to meet requirements of accountability and evaluation, each of these job functions is defined according to specified time percentages in a job description. However, actual time spent is sometimes different from the amount of time specified. Responsibilities should be compatible with time available, and the job expectation should be adjusted as those responsibilities change. If substantial IDR is to be added to an otherwise full schedule, there must be an adjustment in other responsibilities. Commiting a scientist's research time for 5 year periods (in establishing a project) does not allow an easy redirection of efforts; it may be that greater flexibility is required in research project commitments.

<u>Selected Considerations to Enhance Research Capability</u>

Presently, the twin challenges of enhancing ID research and of accounting for resources and productivity in research activities are converging with greater momentum than at any other time in experiment station history. Experiment stations for the most part have been deputy administrative and funding agencies, responding to the needs of constituent departments while being buffeted by the demands of granting agencies and the "guiding hand" of top university administration. The author's experiment station has attempted to improve efficiencies by providing certain needed services, e.g., a few large instruments in departments, statistical and computing services, and recently a central university experimental feed mill. At this time it is aggressively attempting to bring about improvements in project management with aid of computer technology. Given the mission orientation and the convergence of issues which involve more than one department, this writer feels that it is entirely appropriate for an experiment station to take on a "parent" role in seeing to it that the entire research enterprise is conducted on an efficient (cost effective) basis rather than as loosely coordinated academic activity. Indeed, as the departments have held a strong grasp of the disciplines in a very positive manner, it is equally important that a bonding research force transcend the disciplines.

There are 4 specific areas which the writer feels an experiment station can address in order to enhance the administrative climate for increasingly successful departmental and interdisciplinary research:

1) Requests should be developed and industry support obtained so that excellent production-research facilities will be available for all commodities important to the future of the state. Some of these facilities should have excellent environment control, some should have the feature of environment variation (e.g. temperature, humidity, air flow, lighting), and some should simulate on-farm conditions. These facilities should be in the care of individual departments or branch stations. However, means should be developed to assure cooperation and to permit use by other departments.

2) These production facilities should be funded separately from departments and should rely at least partially on generated income. The costs of operation should not come from the budgets of individual scientists. Each scientist needs separate fiscal support to pursue disciplinary and cooperative studies with technicians and graduate students. Industry grants are appropriate for very selected studies conducted by the individual scientists who use the production facility, but use of these facilities must be carefully managed so as to maintain a commitment to the facility for both disciplinary and interdisciplinary objectives and a recognition by faculty that the facility is an interdepartmental resource.

It is often possible that specialists from one department can use materials from another for purposes of a related but different study. As an example of this, suppose the experiment involved the study of a vitamin or trace mineral requirement. Food scientists, biochemists, physiologists, endocrinologists, and immunologists might well be interested in studying the relationship of their speciality to the expected variations in tissue levels of the study nutrient and related metabolites. Many times this can be done easily because the assay instrumentation is already in operation for other purposes. It seems important that such experiments be well publicized to potential cooperators before planning is completed so that policies and protocol can be anticipated and specified in advance. It is ironic that in one case a scientist studying taste characteristics secured experimental turkeys from the freezer case in a local supermarket, when studies were being done with the same species at about the same time in the same University on the relationship of vitamin E to various dietary fat sources and development of rancidity.

3) Shared laboratory services should be further developed. Specialized and highly technical instruments are usually needed by more than one scientist and by more than one department or one college. When such an instrument is available commercially and needed, the experiment station should be receptive (and able with adequate funds) to purchasing and setting up that instrument in a laboratory accessible to all scientists who need to use it. If the instrument requires specialized operation, there should be a qualified technical person on hand to operate it properly. The instrument should be made available to all who may wish to use it on a fee basis related to costs. Policies for the instrument's availability and conditions of use should be coordinated within the experiment station. Its use should be monitored so that management decisions can be made regarding upgrading or elimination of the service. Graduate and undergraduate students should be able to use the instrument under the supervision of the person in charge.

4) Experiment stations should be concerned with and make sure that university services are adequate. Perhaps special provisions, e.g., loading zones, can be established so that samples may be easily transported from building to building for analyses, or so that other items can be dropped off. (There is nothing in the world which is more frustrating than getting a parking ticket while stopping quickly on university business.) Experiment stations should push for more efficient physical plant services: a case in point -- $45,000 was available for a feed mill warehouse, and an approximate plan was conceived; the first Physical Plant estimate was $75,000; upon request the estimate was honed down to $45,000 and a plan developed; when constructed by a commercial building contractor, the cost was $25,000! These are just two examples: there are many others where the collective voice of the research enterprise should be heard and the inadequacy remedied.

From the viewpoint of the investigator, the mechanisms involved in organizing and reporting IDR are important. In enhancing the creative development of ID projects by scientists within the station, it would be helpful if informal collaborative arrangements could have a non-formal project identification, at least in the early stages. This identification would legitimize time spent in specifying the goals and objectives of the work and in identifying how the tasks will be carried out. This information is important to department heads' and other administrators' recognition of the project and will help guide the project through the early stages of existence. In time, the cooperation, if continued, can be appropriately documented as a regular experiment station project and other scientists may be brought into the effort to expand the disciplinary resources on which the collaborative research is based.

This same project identification procedure could facilitate station-initiated research. Experiment station directors are strategically positioned in their administrative affiliations to recognize research needs which warrant interdisciplinary collaboration. Station scientists are more apt to be receptive to such initiatives if experiment stations back up such requests with actions and policies which recognize the scientists' other time demands and attempt to maximize the use of scientists' time through providing services which promote efficiency in the conduct of research.

The question of how ID projects and budgets should be managed is a difficult one. It is suggested that ID projects be maintained by the experiment station in a separate listing. Funding would be split by tasks and assigned to contributing departments. While project proposals and reports would be administered by the experiment station, they should be copied to the departments with opportunity for input prior to finalization.

One might hold some pessimism on the basis that increasing the amount of interdisciplinary research will require increased funding to perform at a time when retrenchments are the mode. It may be, however, that the sharing of experimental facilities and new research funding opportunities will enable things to be done which now seem economically unfeasible.

Other efficiency-enhancing services will be heralded by the use of computer technology. Project planning, for instance, may be enhanced by terminal-monitor communication. It will be possible to retrieve project and grant account balances quickly and place orders for supplies and equipment without the paperwork and time delays now part of the system. Biographic and bibliographic information can be maintained automatically, as part of office procedure. Using key words it will be possible to identify other investigators having related interests which may yield interdisciplinary cooperation.

In closing, it is necessary to emphasize the primary importance of a sound disciplinary research program. This must be strongly protected and nurtured by the departments through vigorous selection of new faculty, strict promotion and tenure policies, and adequate laboratory and graduate student support. It is only through the presence of excellent disciplinary depth that good interdisciplinary research becomes possible.

The Maintenance of Professional Integrity in the Interdisciplinary Team Research Effort*

Roger L. Mitchell

Professional integrity is developed within a departmental framework which provides security, resources, rewards and stimulus. The organic nature of a discipline means that it will be further enhanced by new experiences and exposures that invigorate and transform. This paper addresses the importance of interdisciplinary team research in contributing to the professional excellence of each team member.

The identity and integrity of our professional disciplines is of special importance because those disciplines make unique contributions to the intellectual environment of the university, research laboratory of field station, and to citizens of our several states, the nation, and indeed the world community. As members of the scientific community, each of us has invested major portions of our lives toward building and maintaining a foundation and a framework for specialization and uniqueness. We find it necessary to keep others informed about the intellectual areas on which we are well qualified to serve. And while we have not resorted to unionization to maintain our positions, we do expend significant energy in committee meetings, in reviewing curricula, in building degree programs, in promotion and tenure review processes, and in professional society activities to ensure the quality performance of our peers and the preparation of future peers, as well as to maintain our own professional integrity.

In addressing the importance of interdisciplinary team research, it is necessary to balance the emphasis on collaboration and fusion with sufficient emphasis on the source of the strength which each team member brings to the venture -- that is, professional excellence.

An interdisciplinary team may be formally or informally identified and organized. Departments are sometimes interdisciplinary in that they have either a commodity orientation or draw together faculty with diverse backgrounds. Food Science is a useful example. At present, it may be that food scientists are brought from several different departments and participate in an interdisciplinary effort as a team, or perhaps as an institute, or perhaps they may form a new department. Each of these organizational approaches may provide significant interdisciplinary identity and activity. As another example, field station groups are usually interdisciplinary in nature. The success of research conducted in these situations centers on maintaining professional currency in each faculty members' own area.

ASSUMPTIONS

The assumptions behind the title "The Maintenance of Professional Integrity in the Interdisciplinary Team Research Effort" reflect my beliefs about professional disciplines, departments, and other aspects of the university faculty.

First, a few position statements:

1) The department (often conceived as analogous to the discipline) is the focal point of university faculty organization and action.

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*Adapted from "The Maintenance of Professional Integrity in the Interdisciplinary Team Effort," HortScience. 12:1, February 1977, 36-37, and included with permission of the publisher.

It is here that individual faculty strengths and specializations are understood and defended.

2) Individual faculty members are the most important ingredient of a high quality university enterprise. Most certainly students -- good students -- carefully advised and inspirationally taught -- are major partners with the faculty in the search for and extension of new knowledge, as well as in the maintenance of previous wisdom, but we begin with the faculty. As an aside, it has been my observation that very often the best collaborators are scientists who participate in interdisciplinary efforts most productively after they have built a base of strength in their own areas.

3) Departments and disciplines grew, in part, to provide faculty a stimulating and yet a secure environment in which to work. Department heads, I am convinced, are the key administrators in the university for stimulating and facilitating faculty efforts. Departments and disciplines are the first and strongest force through which strong individual scientists, secure enough to be willing to risk participation in a team, can be developed and supported. I believe a department can be evaluated, to a major degree, on the basis of how well it helps its members interact first within, and then outside, that enclave.

4) The professional society also has a key role to play: a) in providing the publication outlet that involves critique by peers; b) in bestowing recognition that signifies peer acceptance; and c) in providing forums for peer interaction.

5) The definition of disciplines and subdisciplines is an ambiguous process at best. We tend to arrive at these definitions by consensus, without either rigorous analysis or agreement among those in other disciplines. The breeders and the geneticists may be mixed, the physiologists and the biochemists may be separated or they may be lumped together as horticulturists or agronomists. The point is, we determine the boundaries, and I, like you, have contemplated the different ways I might identify myself: Professor, Professor of Agronomy (the department name), Professor of Crop Physiology (my area), Professor of Soybean Physiology (my special interest), and so it goes. If we make the titles sufficiently narrow, it marks us as the only one of a kind in the world--an urge I suspect is deep in the bones of us all.

6) Faculty members need resources with which to work. Such resources are often hard to come by. But, I suggest, we are often shorter on inspiration and creativity. These are elements which the discipline can help provide.

Having shared these views, let me reflect on a few additional assumptions and observations.

Interdisciplinary team efforts in research represent what I consider to be one of the classic organizational questions in the intellectual community. We generally expect that a professor will teach a subject--some relatively discreet portion of the discipline, hopefully interrelated to the continuum. We also expect that extension programs will call for team efforts and regular interactions from a rather broad faculty base. But should we put together interdisciplinary research teams? If so, how, when, and why?

Research is often thought of first as an individual enterprise. We have most rewarded those who pursue a specific topic and illuminate it to the fullest possible extent. Yet we readily agree, I believe, that both individual and team efforts are important. We need to be capable of supporting and rewarding both.

The thrust of my argument suggests that the maintenance of professional integrity is desirable. Why? Presumably, it provides the basis for the most effective contribution, either as an individual or as a part of the team.

BENEFITS OF INTERDISCIPLINARY RESEARCH

What benefits then do we expect to derive from the interdisciplinary effort?

My list includes: 1) Enhanced creativity; 2) Improved synthesis; 3) A more efficient use of resources; 4) A more intensive use of the data collected; 5) Improved communication of the results; and 6) Explanation of our discipline to others.

1) <u>Enhanced creativity</u>. It is my belief that an individual requires a delicate mixture of security and stimulation in order to be creative. My paper might well be titled "Developing a mindset and an environment for maximizing creativity in research". The interdisciplinary team has the potential for generating new stimuli. In fact, interaction with a colleague from a distant discipline may produce more challenging questions than interaction with those who are intellectually closest to an individual.

   The department can be the base for security, for rewards, for consideration of one's resource needs. The department serves as the base point to demonstrate what it means to be a specialist and to guide graduate education. The graduate experience will be most beneficial if it combines both specialization and exposure to the interdisciplinary activities.

Thus, it is my suggestion that this interdisciplinary mindset needs to be encouraged early in the professional career. Perhaps we do this very thing with our interdepartmental graduate committees, so that graduate students do have an interdisciplinary experience. But, I am concerned that the tools for the primary study are automatically drawn from the home discipline and that faculty members from both major and supporting disciplines are not full participants in asking more interdisciplinary questions about the research. Hopefully, an early start in appreciating interdisciplinary efforts comes from an advisor's demonstrating that his or her own research activities are aided by team participation. This helps to give the student an early start on interdisciplinary experience, while building primary strength in the speciality area. Creativity is enhanced by both.

2) Improved synthesis. Our vocabulary is increasingly filled with terms to suggest the need for synthesis. Perhaps the word used most often is systems -- building a system -- modeling systems. The interdisciplinary team has the potential to synthesize a system by drawing on a wide range of perspectives with which to address a problem. This approach, in turn, enables a more integrated conclusion.

3) More efficient use of resources. The more efficient use of human, as well as physical resources, allows an integrity and identity for each discipline which is not possible if each stands aloof and alone. This comes about through the development of a fuller understanding of the discipline's contribution.

4) More intensive use of the data collected. It is customary to analyze research results statistically. There are, however, additional ways in which the usefulness of the information gained in a research venture can be enhanced for use by a wider group of participants. In recent years I have become enthused about having an economic consultant added to many of our team efforts in order to add to the completeness with which the system is studied and to provide an economic perspective. Thus, both in problem identification and in application of new information, a team effort can broaden the scope.

5) Improved communcation of results. Interdisciplinary efforts can result in more publications because the collaborative interaction requires a tightly focused effort and may, in this way, improve the quality of the papers produced so that the acceptance rate is higher. The intellectual challenge brought about by participation in interdisciplinary research may also contribute to producing higher quality papers. Sometimes the audience for publications can be expanded to include the journal of the contributing disciplines. The participation of extension personnel and teachers in the research effort also offers opportunities to improve the communication of results through informal, as well as formal, channels.

6) Explanation of our disciplines to others. In the context of our professional disciplines, one of the primary needs that we have is to explain ourselves to others. I often hear this in meetings: "We aren't really understood; let's develop an information-public relations program". I find, as I suspect you do, that some of the first people to whom we need to explain ourselves are professional colleagues and administrators in the university system, and then legislators and the general public. Professional integrity can be enhanced by the definition, redefinition and communication of disciplinary identities which is required by working together in teams. This communication allows others to understand what insights a stress physiologist in horticulture can bring to a subject, or how a plant breeder for edible beans approaches a research topic. We think we know what a statistician or an analytical chemist or ornamental horticulturist does. Is that understanding correct and is it reciprocal?

The definition of any given discipline is constantly under change and adjustment. Research performed by interdisciplinary teams encourages these changes to be continual and gradual. Such gradual redirection allows faculty members to move into new areas of need, and, in turn, allows intellectual shifts without causing administrators outside the department or university to require program closeouts. How many times do we see a faculty member, faithful to a fault to professional integrity, become an anachronism? If the faculty member is an honest participant in a team effort, he or she is assisted in keeping up-to-date and in seeing possibilities for program shifts through interdisciplinary interaction. Thus, the transition to new research areas, for departments as well as for individual faculty becomes evolutionary rather that abrupt.

Professional integrity is developed within a departmental framework that provides security, resources, rewards and stimulus. The organic nature of a discipline means that it will be further enhanced by new experiences and exposures that invigorate and transform. The discipline may not look the same in twenty years, but it will contain good elements of the old and the new, and it will have made a greater contribution to the needs of humanity.

CONCLUSION

I suggest in conclusion that we maintain professional integrity by:

1) Developing strong departments and professional societies.

2) Encouraging interdisciplinary team efforts in which the department head, as well as the faculty, are fully involved in the process of planning, budgeting and execution.

3) Allow opportunity for specialization but anticipate stimulation from interacting with specialists. Arrange means to reward the group participants, as well as we do individual scientists.

4) Anticipate benefits of gradual adjustments in the definition of professions as interdisciplinary interactions occur.

The Role of the Department Head in Interdisciplinary Research*

Richard J. Sauer

In many universities and experiment stations, quality interdisciplinary research is performed largely in spite of the traditional university organization rather than because of it. This paper delineates the role of a department head in creating an environment in which faculty can successfully participate in interdisciplinary research efforts. Suggestions are made for station budget management for research which involves two or more departments.

"By the beginning of the 21st century, just 19 years away, we will have become a nation of over 300 million people and will have no more acres than we have now. The same amount of fresh water will fall from the skies as now, but we will need to make twice as much use of that water. We will be fed from the same thin layer of top soil that feeds us today, but we will need one third more food.

Land use will be more intensive than today. Housing for an additional 100 million Americans will be built. Space to recycle or dispose of several million tons of solid waste every year will have to be found. We must find more effective ways to prevent pollution of soil, water and air.

Our food, fiber and tree plants now have 1 to 4% efficiency in converting solar energy to food energy. We must somehow increase this if we are to make significant increases in food and fiber production. Unilateral dependence on pesticides has produced many undesirable side effects, and we must seek new and better ways to manage the pest complexes which attack our crops. Reproductive efficiency of cattle is now about 70 to 80%, but near 100% is a reasonable goal.

The nation's food system now requires about 12% of our annual energy consumption. Increased efficiency in processing, handling, transporting and marketing goods can contribute significantly to the nation's energy conservation goals." (Anderson, 1977).

It is with this background that we look at the challenges facing us in agricultural research. The challenges are two kinds: research challenges and management challenges. The research challenges extend to:

1) Conservation and efficient production in using land and water and all forms of energy. In agriculture, this includes food processing and other forms of agri-business, and in the home it includes alternative food systems;

2) New plant and animal productivity -- covering crops, livestock, forest lands, lakes and streams, and wildlife;

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*Adapted from a presentation at the 53rd Annual Kansas Agricultural Experiment Station Conference, January 4, 1978, at which time the author was Professor and Head of the Department of Entomology, Kansas State University.

3) Environmental monitoring and management in its wide ranging forms;

4) New or improved processing, packaging, storage, transportation and marketing of agricultural products and by-products;

5) Improved nutrition, health, education, employment opportunities, and other aspects of family living for persons in rural areas, and

6) Housing and energy conservation.

The key individual in meeting these research challenges is the individual research scientist, but more often by operating as a member of an interdisciplinary team rather than independently.

The management challenges exist side-by-side with the research challenges. The management challenges include:

1) Identifying and fostering needed research;

2) Maintaining effective communication with and between research units;

3) Dealing with growing accountability burdens;

4) Modifying research administration schemes such that all types of research are adequately considered and funded as justified by importance; and

5) Linking more effectively with application efforts by the Cooperative Extension Service and other groups.

The key person in meeting many of these management challenges is the department head.

The day of the lone scientist pursuing a traditional research program based solely on his or her individual interest, job description, or area of responsibility has largely come to an end, for two reasons. First, we live in a world where dollars for research have become increasingly scarce and the demand for accountability has grown, requiring that priorities must be established, because we will never have resources to pursue all of our interests. It is imperative that we be willing to make hard-nosed decisions and that we redirect research efforts into those areas having the highest potential return. Thus, we cannot afford the luxury of each scientist developing an independent research program around his or her own interests.

Second, since most of the easy problems have already been solved, major breakthroughs will be required before further significant gains in agriculture can be achieved. As the complexity of the problems we face increases, the expertise and involvement of more and more scientists working together on integrated objectives becomes a necessary requirement. For example, in my own area of entomology, we are painfully learning that any management action taken against an insect pest soon influences not only the well-being of other insect pests but also plant pathogens and perhaps even weeds. Further, the pest management decisions are often made within such complex and dynamic agricultural production systems that economists and sociologists must be involved in the required cost-benefit analyses. Out of this requirement of involving teams of scientists has grown a critical challenge -- one of coordination among many disciplines. As a way of achieving this coordination, we will need to plan and implement research on a systems approach involving a team of scientists (i.e., interdisciplinary research) rather than the traditional problem approach as conceived by individual scientists (i.e., disciplinary research).

Three levels or styles of research can be contrasted:

1) Disciplinary - research conducted by one or more people within a single discipline;

2) Multidisciplinary - research in which people from several disciplines are involved but in which the research is planned, executed and evaluated by each person separately. Management of the various segments of the research is usually provided by each disciplinary unit (department) involved; and

3) Interdisciplinary - research involving input from several disciplines and with the effort mutually planned, executed, evaluated, conclusions drawn and results disseminated through the mutual effort of the scientists involved.

Each of these styles or levels of research has a place in today's agricultural research program. However, I would maintain that many of the more complex problems facing us today must be attacked by either a multidisciplinary or an interdisciplinary approach. Further, I would maintain that the interdisciplinary approach is superior to the multidisciplinary approach in large, complex, mission-oriented programs such as agricultural experiment stations; however, it is much more difficult to organize, fund and manage, because disciplinary boundaries often prevent adequate interchange among scientists and make it difficult to maintain resource flexibility.

What is the role of administration, specifically of the department head, in meeting the challenge of organizing, funding and managing interdisciplinary research? Successful interdisciplinary research efforts usually do not evolve from the administration, but rather from the scientists themselves. The members of the interdisciplinary team must be the principals in conceiving and planning the research and must have a mutual commitment to its objectives. In fact, in all phases of the interdisciplinary program development, major emphasis must be placed upon the involvement of the individual scientists because the success of the entire research effort depends upon the commitment of each and every research scientist on the team and the professional excellence of each contributing team member.

The administration, on the other hand, must try to provide an environment that will break down existing barriers between the various academic departments or other disciplinary research units which the individual team members call home. Unless the entire administration is committed to the interdisciplinary endeavor, the effort is defeated before it gets started. The department head is the key person in this administrative structure, for it is the department head that is responsible for stimulating and facilitating the involvement of strong research faculty from his or her department. If the department head is not committed to the interdisciplinary endeavor, it is very difficult to develop any kind of interdisciplinary research program involving the faculty of that particular department. After all, the ground rules and the priorities for rewards (tenure, promotion and merit salary increases) in the department are strongly influenced by the department head, and most faculty members will respond and orient their research efforts accordingly. In other words, the department head must place a high priority on interdisciplinary research and reward faculty members for successful involvement in it. It must be recognized, however, that success will not necessarily be measured by the traditional criteria, such as single-authored publications in the professional disciplinary journals of that department. Further, all department heads of the faculty involved in the interdisciplinary research effort must have the same commitment. If one does not, the contribution of that disciplinary component by the faculty member in that less committed department is accordingly weakened.

Interdisciplinary research involving faculty from several departments also requires that adequate resources be made available to the individual members of the research team. Resource allocation cannot be left entirely to department heads, because they have too long operated in a competitive budget atmosphere with commitment to traditional disciplinary biases. It is more difficult for them to attach equal priority to interdisciplinary and disciplinary research when it involves a dollar commitment than when it involves just a professional manpower commitment. In other words, they are often willing to commit a faculty member to an interdisciplinary team research effort as long as they do not have to support that faculty member's contributions out of funds previously allocated to the department.

With respect to the experiment station, there has been a tendency on the part of department heads to want to support "in-house" research and leave support of the interdisciplinary effort to the director. This would seem not to be a completely realistic approach, because we are organized into departmental units. What then would be a viable alternative? One that I propose for consideration would be some type of mini-grant or special assistance fund held by the director's office (that is, not allocated to departments with their regular budgets) and earmarked for encouraging new interdisciplinary research efforts, but which also would require some type of matching commitment on the part of department heads. The teams of faculty members interested in initiating new interdisciplinary research projects could compete for the funds in much the same way as they would compete in preparing proposals for a federal granting agency (a side benefit being the practical experience they would gain from such proposal development). The requirement of some matching funds coming out of the regular departmental budgets would convey the commitment of the department heads to the involvement of their respective faculty in the effort. The projects would be funded for no more than two or three years, after which the dollars would revert back to the special fund to be awarded to some other proposed new effort. Hopefully, the previously funded project could by that time compete for extramural funding.

If the resource flexibility needed to set up a special mini-grant fund does not already exist in the director's office, how could such a fund be established? I would propose that the normal annual increase on experiment station funding from the State Legislature would be one way to start it. I, as a department head, would be willing to give up the increase normally provided in my departmental budget for one year, and I would hope other department heads would do likewise.

In the time allotted me, I have perhaps overstepped my bounds and gone beyond the topic assigned to me. I have suggested a way in which the director and department heads could change station budget management to stimulate exploration of new avenues of research, particularly that involving two or more departments. At the same time, I have attempted to delineate the role of a department head in creating an environment in which faculty can successfully participate in interdisciplinary research efforts.

In closing I would maintain that quality interdisciplinary research is performed in universities and experiment stations today largely in spite of the traditional university organization rather than because of it. I hope the comments by myself and others at this conference will lead to some necessary changes so that successful interdisciplinary research is truly catalyzed by the university, not performed in spite of it.

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Administration and Funding of Interdisciplinary Consumer Research*

Keith N. McFarland

Departmental goals and faculty reward systems oriented toward departmental activities and evaluated by departmental colleagues often impede interdisciplinary efforts in research. Administrative leadership can provide positive reinforcement for participation in interdisciplinary activities. Searches for interdisciplinary research funding require institutional, unit administrator and researcher cooperation.

An administrator new to a home economics program quickly becomes sensitive to interdisciplinary endeavor, since the field rationale focuses upon the interrelationship of people and the various aspects of their near environments. Undergraduate courses reach across disciplines in various degrees, and interdisciplinary instruction at this level is encouraged. However, interdisciplinary instruction at the graduate level and interdisciplinary research appear to be more honored in the abstract than in actual execution. To launch into an interdisciplinary research endeavor, the researcher must form the relationships that permit planning across departmental lines and must possess the determination that overcomes the added burdens incurred by work outside the department or discipline.

Fred Harvey Harrington describes the department as a "major obstacle to change within American colleges and universities" (Harrington, 1977). Indeed, the typical departmental structure poses special problems to those seeking cross-disciplinary outlets. Departmental goals and individual researcher's interest do not always coincide. Staffing needs may be met with choices more appropriate to the disciplinary concerns of the department than to the cross-disciplinary expressions fundamental to a mission-oriented institution. At a time when staff evaluation for tenure, promotion, and salary adjustment purposes is increasingly centered in departmental colleagues, the risks encountered by the cross-disciplinary worker operating "outside the pale", so to speak, are magnified.

In addition to the issue of departmental allegiance is the role of departmental priorities in interdisciplinary research. Seldom are two departments at the same stage of readiness to attack a problem of mutual concern. Other assignments or priorities lay claim to time and resources of researchers who are willing to work across departmental lines. Reassigning those time and fiscal resources requires the commitment of administrators, as well as researchers. Because cross-disciplinary work may be interpreted as being peripheral to departmental objectives, the resources of the department may be less available for promotion of the research. When one has limited time for research, efficiency favors the use of time on matters close at hand. In crowded daily routines the time and energy required for setting meetings and in traveling across the campus to work with colleagues of another discipline require a sustained commitment to the problem and to the collaboration.

This sustained commitment must likewise carry over into the final stages of the project. Reporting how the conceptual perspectives and terminologies were blended to serve the problem requires the collaborative researchers to main-

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*Adapted from "Administration and Funding of Interdisciplinary Consumer Research," in H. Keith Hunt and Frances M. Magrabi, eds. Interdisciplinary Consumer Research. (1980) Association for Consumer Research. It is included with permission of the Association for Consumer Research.

tain their vision and frequently requires publishers to expand theirs. Outlets for publishing results of cross-disciplinary efforts are frequently less available and sometimes held in lower academic esteem than disciplinary publications. The delicate question of who receives the credit in shared research can also complicate the scene. Those who work in interdisciplinary research must, in a sense, command two or more "languages".

To encourage researchers to broaden their skills and interests beyond the range of current departmental parameters requires administrators to not only look beyond the immediate concerns of the department but also to provide resources for efforts with scope beyond that of the department per se. Some administrative considerations helpful and encouraging to the conduct of interdisciplinary research include the following:

a) The department must be encouraged to identify its objectives with breadth sufficient to include interdisciplinary research as a completely appropriate and, indeed, an expected activity.

b) The departmental seminar and project review systems should encourage cooperative interchange with scientists in related disciplines. Joint discussions are useful in encouraging and legitimizing joint endeavors.

c) The assignment of teaching loads must permit the use of reasonable blocks of time for research, as well as for project development and proposal writing. Faculty members must be encouraged and expected to take initiative in making contacts, both professional and funding related; and this promotional activity must be recognized as legitimate by administrators.

d) Administrative staff resources can be utilized to identify possible sources of funding -- as a preliminary to the researcher's discussing specific parameters with personnel from the funding organization. Some travel funds must be available to bring the scientist face-to-face with the potential funding agency. One option for expanding funds available for this is to redirect some of the funds presently used to support attendance at society meetings to more focused visits with funding sources.

e) Faculty members should be given opportunities to modify research intentions. Discussions attendant to progress reviews can lend encouragement and support to new directions in research.

f) Interest in interdisciplinary research might well become a criterion in faculty recruitment.

g) Public or private funding agencies need to be made aware of problems of major public interest. This is a continuing process, never ending. Their fiscal response to attractive research proposals comes out of their aroused concern.

The researcher in the land grant colleges may have access to Agricultural Experiment Station funds, both federal and state in origin. State funding (legislative specials) most likely reflects defined problems of interest to legislators, who are very close to their constituencies. Interdisciplinary researchers must be aware of the problems defined by those constituencies and must exercise insightful creativity in identifying scientific tools which may contribute to the solutions. The home economics profession has enjoyed some successes in influencing public policy in recent years; the climate is changing from one of reacting to one of initiating change. Critical to initiating change is a solid understanding of the existing situation.

At the federal level the more comprehensive problems may be encompassed in the goals toward which funds are directed. Funding agencies tend to put their money where there is power in program. This bias works to the disadvantage of new thrusts or of groups just moving into research endeavor. Experienced researchers note the need for "something to sell" in going to funding sources, including defined rationale, pilot study results, and the promise of pay-off in ultimate outcomes. Here again, the researcher's need for planning time and for opportunities to meet with funding agencies is evident. Funding sources are partly in the eye of the beholder. Courage, agressiveness and imagination are important characteristics in researchers seeking funds for interdisciplinary research.

The experiences of the American Home Economics Association's Commission on the Family Research Act of 1975 and from the "Home Economics Research Assessment, Planning and Projection" study provide several lessons:

a) That a continuing relationship with funding sources needs to be developed and to be maintained over time;

b) That requests for legislative funding need to rest on a strong information base, be realistic, and promise information useful in dealing with questions held to be important by the funding sources;

c) That the faculty members need time to plan, to develop projects and to follow leads to funding sources;

d) That the race goes not to the meek or the conservative; and

e) That credibility of the petitioner grows out of action, and that research success breeds success.

Funding for public institutions of higher education increasingly rests upon measures of teaching load. In a future in which research may be largely funded separately from teaching, administrators will need to insightfully integrate the instructional and research goals of their departments. Interdisciplinary researchers will have to be responsive to scientific and lay interest groups in identifying problems and aggressive in seeking research support.

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Budgeting for Interdisciplinary Research*

R. J. Aldrich

The high costs of equipment, the extensive amounts of research budgets which are committed on a continual basis to salaries, and funding cutbacks have all influenced the budget setting of interdisciplinary research. This paper describes how interdisciplinary research is administered in one experiment station. It highlights the importance of the Director's office, of human resources and of accountability.

Budgeting may be at least as important as organization itself in determining the success of interdisciplinary research. It can be a powerful tool for both identifying researchable problems and in moving staff members to work together.

THE BUDGET SETTING

Changes in the budget settings in which the Experiment Stations find themselves have important implications for funding interdisciplinary research. Three have particular relevance.

Larger Share on Salaries

One is that a larger portion of our total budgets are now committted to salaries than was true a few years ago. In 1975 about 70% of Experiment Station funds went for salaries. In Missouri we were slightly higher than this, approximately 73%, as compared to approximately 50% ten years ago. There are many reasons for this change. Among the more important ones are:

1) Salaries have had first priority on increasingly scarce resources during a period of spiraling inflation;

2) Research and research hardware have become increasingly sophisticated and complex, resulting in a need for more highly trained supportive staff both to operate the hardware and participate in the research; and

3) Legislated labor policies have caused us to be less efficient with our funds used for personnel.

There are other reasons, but the explanation is not so important as recognizing the implications for funding interdisciplinary research. It means that we must manage people rather than merely manage operating funds if we are to get very far. In other words, our approach to funding interdisciplinary research must focus upon the 70% of our budget that is committed to salaries. Indeed, we must ask ourselves if our long standing practice of funding projects provides us the management leverage needed today.

Less Flexibility

A second significant aspect for interdisciplinary research funding is that there is less flexibility in managing research resources than a few years ago. Having a larger part of our funds committed to salaries in itself limits flexibility. Fixed pay scales automatically commit some of our funds to periodic pay raises. Some fiscal accounting has become computerized. The computer is important to many aspects of our operations; nevertheless, it removes some of the human element which previously gave us flexibility. Compliance with regulatory policies also restricts funding flexibility. Animal

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*Adapted from"Budgeting for Interdisciplinary Research," HortScience. 12:1, February 1977, 35-36. Reproduced by permission of the publisher.

welfare legislation has forced us to commit funds to facilities for our experimental animals and to their care. And the list could go on.

What this all means for funding interdisciplinary research is that individual projects and individual departments may not have sufficient funding flexibility to underwrite an interdisciplinary effort. Rather, the Director's Office may be the level at which the funding will have to be secured. Associated with this is the need for more direct management of day-to-day expenditures than most of us would like. But I see little alternative. The main reason is that there is an inverse relationship between the size of the budget unit and flexibility.

Take the matter of released salaries as a case in point. At the Missouri Agricultural Experiment Station, we know that 5 to 7% of our salary commitment at the beginning of the year will be released because of retirements, resignations, and deaths. This is currently a rather significant total of $250-300 thousand; also, the percentage released is fairly constant from year to year. However, for even our largest departments, the amount is not very large and, of course, will fluctuate a good deal from year to year. Thus, the Director must use the whole station budget as a base from which to obtain funds for interdisciplinary programs.

I should state at this point, that regarding interdisciplinary research I am referring to broad problems of our agriculture -- not to cooperative endeavors of a few scientists. The latter are indeed important, but likely do not pose unique funding problems. An analogy using our forage-livestock systems of production program may help clarify this dimension. Our grass breeder is working with other plant and animal scientists in developing a superior fescue, but it is the integration of this new fescue into a total beef production system which creates the funding challenge about which I am talking.

<u>Sophisticated and Costly Hardware</u>

The third aspect of our budget setting is that equipment is becoming increasingly complex and costly. We have already reached the point at which it is not possible for us to provide each scientist with all of the research equipment needed for his or her individual research. This is not new. Nevertheless, there are aspects which present unique problems in funding interdisciplinary research. Responsibility for the optimum operation of a highly complex research machine can hardly be vested in a team; one person must have that responsibility. Anyone who has had several children using the family car will understand this principle in machinery maintenance.

These are some of the broad aspects of our budget setting which must be dealt with in funding interdisciplinary research. The remainder of my comments will deal with the things we have done at Missouri to encourage interdisciplinary research and the associated budgetary implications.

APPROACHES TAKEN AT MISSOURI

The approaches I will discuss are presented as evolutionary -- possibly experimental rather than final -- answers being recommended. These approaches need to be evaluated in light of the unique circumstances in different institutions. Some may fit. Some may not.

<u>Coordinator</u>

One of the approaches we have taken is to use a participating staff member in a coordinating role for interdisciplinary research. The coordinator retains his or her departmental appointment for teaching and individual research and is appointed in the Director's Office for the coordinating role. Using a participating scientist to serve in a coordinating role, in my opinion, has facilitated the cooperative involvement of scientists from other departments. This approach focuses on managing and involving people. What I am saying is that someone who is an integral part of the team may do better in getting other scientists to work collaboratively than might a department chairman or someone in the Director's Office.

The coordinator has a research operating budget with latitude to use it to supplement the support of the individual scientists involved in the interdisciplinary program as well as to support the interdisciplinary project itself, as in the case of our forage-livestock research program. The coordinator operates much as a department chairman with respect to this operating budget, by agreeing with the Director of the Station on the funds to be used for the fiscal year. The coordinator then has nearly full autonomy and flexibility for use of the funds. The coordinator also has a maintenance budget when the research involves a facility.

In order that the coordinator's department not be penalized for the loss of his or her time, funds equal to the coordinator's salary in the Director's Office are returned to the department. At the present time, two departments supplying a coordinator are using these released funds for the stipends of postdoctorate associates. This is their choice. The funds could just as well have been used for graduate students, technicians or other costs and have, in fact, been used for such purposes in the past.

This method of funding research amounts to taking funds off-the-top of the total for operating the Station's research. This is an alternative to monies being allocated to interdisciplinary research by the departments. Thus, I see it as addressing the flexibility issue mentioned previously, as well as the people management issue.

Special Assistance Fund

Another approach we are using to encourage interdisciplinary research is a Special Assistance Fund. An amount of money is set aside annually again off-the-top, for this fund. To quote from the stated objectives, "The objectives for this fund are to stimulate exploration of new avenues; encourage change in emphasis; and add depth in our research, particularly that involving two or more departments. The Fund may be used to purchase equipment and cover other research costs beyond the funding capability of a single project or department or to cover costs which would be awkward to obtain from accounts in several departments. The extent of multidisciplinary effort involved is a key consideration. Of the 14 proposals funded from this source during its three years, all but 3 are interdisciplinary in nature.

The funding arrangement is relatively simple. The staff members compete for the funds in much the same way as they would in preparing proposals for a federal granting agency. The proposal shows the existing Station projects which will be involved. If a piece of equipment is needed, we normally award funds for equipment through one of these projects. If operating dollars are involved, these are split among the projects involved by simply adding dollars to those projects' operating accounts.

I hardly need to point out the incentives which the Special Assistance Fund and our choice of proposals provides for faculty to seek ways to cooperate in using facilities. This, in turn, creates settings which spawn interdisciplinary research. Here, too, we are responding to the need to fund people and not just projects. Although the amount of money in the Special Assistance Fund has not been large, it at least has communicated to our staff that there is an opportunity to obtain a major piece of research equipment or to explore a new avenue of research. Given the constraints of tight budgets, the psychological importance of this point cannot be overemphasized.

Service laboratories

Another approach to meeting the high costs of research equipment and the associated need to maximize the use of such equipment is through what we call service laboratories. We have Station Chemical Laboratories, a Station Electron Microscope Lab, and an Animal Whole Body Scintillation Counter which are available for the use of all of our staff. In each case an individual in an academic department is assigned supervisory responsibility for the lab. The Experiment Station provides an operating budget to cover all or part of the costs, but this budget is managed by the staff member in the academic department. The significance for interdisciplinary research lies in providing for management of the equipment so that this is not a hurdle in planning and funding.

Multidepartmental laboratory

Several years ago we completed the first stage of our Animal Science Research Center. In a very real sense, this is an interdisciplinary research facility. It houses staff from five departments who are doing research on the whole animal. The staff members have their individual laboratories, yet share in the use of much of the special equipment such as the amino acid analyzer and ultracentrifuge. This approach responds both to the need to encourage interaction among scientists who are working to improve the efficiency of livestock and also to our need to get the maximum use from funds available for specialized research equipment. In this case one of the two coordinators referred to earlier has the overall supervisory responsibility for this facility. Some of the funds available to the coordinator have been used to purchase special equipment. In addition, a stockroom clerk, a receptionist, and a secretary work as staff to the coordinator.

SUMMARY

These are our attempts to respond to three basic considerations in funding interdisciplinary research, namely:

1) The budgeting approach must take into account the fact that human resources will make up roughly three-fourths of the total resources committed to the research.

2) Funding of major programs will have to come at the Director's level since loss of flexibility precludes assembling the needed resources at the individual scientist and departmental levels.

3) Providing the necessary equipment and facilities must be done in a way which assures accountability for their satisfactory operations, as well as efficient use.

Of course, budgeting cannot be nearly separated from organizing and from maintaining the scientists' integrity in interdisciplinary research. Furthermore, we must always be mindful of the tremendous impact of budget decisions on our faculty as individuals and as members of departments and teams.

Institutional Policy and Operational Issues Affecting Interdisciplinary Research*

Robert P. Boger and Virginia T. Boyd

The institutional setting for interdisciplinary research is very important. This paper discusses the role of institutional policies for personnel, resources and organizational flexibility in establishing an "interdisciplinary milieu". Several organizational approaches are described, and operational assumptions essential to stimulating innovation in research are presented.

Michigan State University (MSU) is one of the larger institutions of higher education in the United States, enrolling approximately 45,000 students. It is a land-grant university with a long tradition of excellence in applied research. Perhaps as a function of this applications-oriented research philosophy, MSU has what may be called an "interdisciplinary milieu" within which the research program of the university functions. This milieu consists of a pervasive atmosphere generally conducive to cooperative, interactive, interdisciplinary study. This academic climate is particularly important for the Institute for Child and Family Study in its mission to facilitate the interdisciplinary research process with respect to studying the family and young child. The following remarks are made in this context.

This paper discusses the institutional setting for interdisciplinary research and focuses primarily on those factors which in our opinions most heavily influence the establishment of an institutional climate conducive to initiating and conducting interdisciplinary research. The institutional realities and the societal context of higher education today form the background for this focus.

The challenge before the sciences, particularly the humanistic sciences, has never been greater than it is at the present time. As technology outstrips the slowly emerging knowledge of humans, the magnitude of social difficulties within our own culture and throughout the world continues to increase. An acceleration in the development of social knowledge, particularly that relating to effective use of our total knowledge, is imperative. Further, the research which expands this knowledge base must have almost immediate relevance. The time is past when science can concentrate on a solely molecular bias in its approaches to the study of people. If we are to gain leverage on the complex issues of our time, both basic and applied research efforts must attend to the complementary synthesis of knowledge sometimes referred to as the "ecological" approach.

Conceptualizing issues for interdisciplinary research, however, is not enough. Critical policy and attitudinal changes must occur within most higher education institutions before any model for interdisciplinary research will have a chance to take root. Past emphasis on institutional environments have tended to concentrate

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too much on the tactical and strategic questions, rather than on the mission and organization of the university as an institution of contemporary society. Current efforts to develop models for education and research which involve the integration of basic disciplines have been regarded by many educators as crucial to the success of contemporary education. These have not, however, been overwhelmingly successful. Higher education in our society has developed established patterns that are extremely difficult to change.

In the rapid development of American universities from simple to highly complex institutions, the traditional old-time professor has been forced to evolve into the modern academician -- a trained specialist fluent with the rights, privileges and responsibilities of his or her profession. The faculty member of today has become indistinguishable in many ways from the classic "organization man". The organization, although larger and more complex in scope and support structure, has changed very little in basic organizational pattern. The full professor is still, and hopefully will continue to be, basically autonomous within the departmental and collegiate structures of organizational power. Indeed, the trustees and university administrators do influence faculty based policy. However, as Nilles (1975) pointed out recently, the academic organization, unlike industry, is not functionally pyramidal. That is, it does not have a widening but continuous chain of command from top to bottom. The department, and to a lesser degree the college, is still semi-autonomous. These units determine what accomplishments of the faculty and students will and will not be rewarded.

It seems clear that any institution which would conduct interdisciplinary research should extend considerable effort to establish policies and organizational structures which will expedite the interdisciplinary process in higher education without unduly tearing at this basic organizational fabric of the university. It has been pointed out that quality interdisciplinary research is performed in universities today in spite of the traditional university environment, not because of it. We raise the challenge of supplementing existing organizational structures with policies which will catalyze and support quality interdisciplinary research.

THE CONCEPT OF INTERDISCIPLINARITY

The terms "interdisciplinary" and "interdisciplinary research" have become cliches in discussions of educational research. At the same time, these are misunderstood academic colloquialisms. As White (1972) recently pointed out, there is no generally accepted definition of interdisciplinary or multidisciplinary research in present research circles. A pattern emerges, however, when one reviews various positions with regard to the concept.

Blackwell (1955) defined a three-dimensional continuum of research undertakings based on six alternatives. The dimensions included the number of people involved (X), and the degree of interaction among researchers (Y) and the number of disciplines (Z). A description of the combinations, leaving out the impossible alternatives, results in a continuum of increasing interdisciplinarity ranging from the lone researcher working in one discipline to the team of researchers from multiple disciplines working in interaction.

The use of systems science has, perhaps more than any other process dimension, allowed for quantum leaps in the development of interdisciplinary research approaches to social issues. In this regard, the model proposed by Jantsch at MIT (1970) introduces a fourth level of disciplinary integration which he terms "transdisciplinary". For Jantsch the critical dimension which delineates levels of interdisciplinary effort is the degree to which concepts from multiple disciplines are synthesized, as well as the degree to which the acceptable goal structures inherent in each are coordinated. Transdisciplinarity is a most important addition to the continuum of cross disciplinary effort because it demands organizational commitment and innovation, including the delineation of specific programmatic or system goals.

The following definitions are drafted for purposes of this discussion:

> disciplinary research -- research by one or more individuals involving a single discipline;
>
> multidisciplinary research -- group research whereby individuals from different disciplines work together on a common problem but with limited interaction;
>
> interdisciplinary research -- group research whereby individuals from different disciplines work as a team, with continual intellectual interaction and conceptual synthesis;
>
> transdisciplinary research -- group research whereby individuals from different disciplines work as a team within a mutually accepted systems organization with an overall set of systems goals.

ORGANIZATIONAL ASSUMPTIONS AND ISSUES

Unique policy, organizational and operational assumptions are necessary if any model of interdisciplinary or transdisciplinary research is to succeed within a given university community.

Commitment to Transdisciplinarity

The ability to establish an interdisciplinary research program is not enough. Commitment to these efforts at the top levels of academic administration is an even more basic necessity for the success of interdisciplinary programs. Faculty participation will often be guarded initially. If the administration reflects an explicit commitment to a policy of organizational innovation, however, models of interdisciplinary collaboration will have an opportunity to emerge and to prove themselves. Without such a commitment, the traditional disciplinary biases and the vested power concerns of departmental structures will, in most cases, overcome any long-term progress toward encouraging interdisciplinary interaction in research.

Those who cry for more "accountability" on the part of higher education today cite the overlapping pragmatism that has pervaded postsecondary academics as one of the reasons why this is needed. Administrative commitment to the organizational concepts of innovation and change is essential to the continued identification and implementation of experimental programs. The "foot in the door" approach has been overused. Without longitudinal administrative commitment to the philosophical concepts of interdisciplinary organization, the probability is very high that any efforts at such change will fail.

Institutional Openness to Change

Certainly many institutions of higher education are not now and will not in the forseeable future be interested in or ready for interdisciplinary approaches to academics. A climate of openness to goal redefinition and concomitant organizational innovation does exist, however, in many institutions involved in large-scale research and research training. If the grafting of an innovative model for interdisciplinary research is to "take" at any given institution, administrative commitment must include an assumption that such a climate can exist. This "climate" consists of an attitudinal phenomenon based upon an underlying philosophy of education.

Faculty negativism toward models of interdisciplinary study is often the result of professional and personal pragmatism. Interdisciplinary research has a credibility gap even with many of its most logical supporters (i.e., innovative faculty dedicated to the philosophical essentials of transdisciplinarity). The reasons for negativism in many cases can be traced to past attempts at implementing such efforts which failed because they lacked critical administrative support and organizational soundness. These failures have hurt some participating faculty members by placing them in inappropriate and awkward positions with regard to their disciplinary departments.

Faculty depend on departmental support for academic and fiscal well-being. Their consequent hesitancy to continue to commit professional energies to efforts which have failed is understandable. It underlies the critical need for sound organizational and professional policies to form the basis for external support and internal conduct of innovative interdisciplinary research.

Personnel Policies

Personnel policies and decisions at all levels are crucial. If a commitment to interdisciplinarity exists, it will be reflected in all aspects of an institution's personnel decision-making from the president to the research assistant. These policies and decisions are perhaps the most crucial at the level of deans and department chairpersons because these administrators can expedite or undermine any movement toward interdisciplinarity. Unless these administrators are committed to helping the interdisciplinary programs of the university succeed, the start-up, experimental programs are likely to burn out before their potential value is even determined.

To build an administration and faculty with an interdisciplinary orientation takes time. Many excellent administrators and faculty simply do not see things this way, or do not have the personality characteristics which support interdisciplinary interaction. Again, well-directed policies, maintained over a period of time, are necessary in order to form a critical mass of such professionals and then for them to identify each other within a given institution.

Size

Interdisciplinary research programs can be found in small as well as large institutions. The size of the institution influences interdisciplinary potential with regard to the flexibility of professional personnel resources. The large university has more faculty. Assuming that an environment conducive to interdisciplinary research does exist, department heads have more flexibility in allowing some faculty to participate in interdisciplinary research while simultaneously allocating other faculty resources to maintaining the disciplinary excellence of the department.

Resources

Large-scale interdisciplinary research demands extensive dollar resources to support a process which is more complex than individual scientist, single discipline research. The institutional costs for the personnel and supporting mechanisms necessary to implement large scale interdisciplinary research are rarely recovered in full from supporting grants. This has and will continue to be a crucial limiting factor in the reluctance of higher education institutions to wholeheartedly encourage IDR through their policies.

Sensitivity to Social Process

Even when policies to interdisciplinary research exist at the institutional level, other issues at the middle management level must be confronted if successful interdisciplinary programs are to be launched. One of these is a sensitivity to the complex social process which interdisciplinary research involves. Administrators of these efforts must, to some extent, be self effacing in the tradition of the ancient saying attributed to Lao-tse:

> "Of a good leader, who talks little
> When his work is done, his task fulfilled,
> They will all say, 'We did this ourselves.'"

Administrators cannot dictate the structure of interdisciplinary teamwork any more than they can dictate the direction of intellectual creativity. The former is as much a matter of professional freedom as is the latter. This is not to say, however, that the interdisciplinary research management should be "laissez faire". Interdisciplinary teamwork is, like so much of human cooperation, a fragile thing at best. Any organization that would portend to facilitate such efforts must remain sensitive and responsive to the social processes which underlie creativity in teams. Such social orchestration is an art and is influenced by many factors, the most critical of which are the personality(s) and social skills of those in positions of interdisciplinary leadership.

One of the most difficult things for a professional in an interdisciplinary group to acknowledge is that the high level of intellectual sophistication and elitism which contributes to recognized expertise in their particular field may not be functional in an interdisciplinary effort. It is encouraging, however, that specific techniques and processes for communicating and accomplishing tasks in teams can be learned and do, in fact, help individuals within the group to communicate precisely, meaningfully, efficiently, and (one hopes) genially.

Permanent State of Flexibility

In a recent conversation with an administrator of an interdisciplinary research center, the discussion turned to organizational influences which he felt were most important to the success of his program. Without hesitation, he said "a dedication to flexibility". By definition, interdisciplinarity and disciplinarity demand a state of continuous change. To provide this environment, organizations facilitating IDR need to be extremely dynamic. The attitudes of both the scientists and the administrators involved should reflect an acceptance of this lack of organizational stability. Certainly this does not reflect the traditional nature of university structure, and continuously maintaining such a state of flexibility presents difficulties.

Pragmatic Sensitivity to Traditional Organizational Patterns

However, any inference that interdisciplinary research implies the eventual dissolution of existing disciplinary structures is unfounded. Indeed, the demise of disciplinary structures would preclude interdisciplinary efforts. The need for interdisciplinary collaboration arises from the very fact of specialization, and disciplinary structures give form to such specialization.

If the neophytic interdisciplinary effort is to be successful within the university, its leadership should be sensitive to the pragmatic need for complementary rather than competitive relationships between traditional departments. This is not to say that cooperation will always be possible or that it will ever be easy. Most interdisciplinary units are atypical and, as Dressel (1972) points out, "atypical organizations are not easily geared into larger systems based on more traditional patterns". Interdisciplinary organizations offer for researchers alternative environments and opportunities not available within traditional departments -- greater autonomy, for example. They should be viewed as complementary, therefore, rather than competitive. If such interactive, complementary structuring is an administrative priority, creativity in management and organization can be achieved.

Communication

Communication is a critical process in training which prepares researchers from varied disciplines to function in integrated team research. Semantic difficulties, educators' predilection toward the over-use of jargon, and differences in levels of abstraction and quantification have handicapped many potentially successful interdisciplinary efforts. Clear preventive measures are available to overcome these problems and include: attending to the group process in staff seminars; providing time for the development of a common understanding; and specifying the relevant axiomatics and glossaries which team members have in common.

Most important, however, are the attitudes and interpersonal interactions of the professional members of the interdisciplinary team. An ideal interdisciplinary organization is an open, free, nonthreatening and informal arena for communication between participants. Professional egos, hidden agendas, personal insecurities, and inappropriate demands for special status (as a function of rank or experience) have been shown to reduce or eliminate such open communication (Kolka, 1972). When this occurs, the potential for integrating individual inputs is greatly reduced.

APPROACHES TO ORGANIZATION

What are the present organizational models for interdisciplinary teaching and research, and how far have they gone toward a transdisciplinary system? Four distinct types of interdisciplinary organization currently exist.

The new colleges organized around contemporary social concerns no doubt go the farthest toward integrating traditional disciplines. Good examples of this approach would be the University of California at Santa Cruz and the Green Bay campus of the University of Wisconsin. At Green Bay, the colleges have been organized around the socially relevant foci of environmental science, human biology, community sciences, and creative communications.

These university structures are on the cutting edge of contemporary education. Interdisciplinary efforts are the primary, not the peripheral, emphasis of the institution. Organizations such as these offer the greatest potential at the present time for transdisciplinary research and research training. However, higher education is entering a period of retrenchment, and the potential for establishing even a few new university communities like Green Bay or Santa Cruz in the immediate future does not appear to be great.

A second organizational format is the program or project organized as a part of one of the line organizational units (college or department) of the university. Here the interdisciplinary effort has the umbrella protection of the parent unit but is vulnerable to difficulties of limited autonomy and visibility. Efforts organized in this manner have limited long-term impact on the overall university climate for interdisciplinary activities.

A third organizational approach to interdisciplinary study is to establish within the traditional university structure a line unit which has a particular interdisciplinary concern as its mission. Although such an organizational approach is somewhat traditional, in that the unit fits into the existing fiscal and organizational fabric of the university, disciplinary departments are usually absent. The faculty of such units often have joint appointments with the more traditional academic units in order to maintain a more "basic" professional involvement and identity. Concerns for long-term stability of the interdisciplinary units and for flexibility in drawing from scientific expertise are most often the basis for using this organizational approach.

A most important point, however, is that such interdisciplinary organizational units are generally viewed by both faculty and administrators as supplements or appendages to the "basic" structure of the university -- the more traditional colleges and departments. At best, faculty and administrators lend partial support for the legitimate claim by such units to the university's limited resources. Seldom is there a university-wide commitment to the goals of the interdisciplinary unit. In spite of these drawbacks, however, these units do have a legitimate line to the university budget and can provide a relatively stable organization over a period of time.

The disadvantage of the traditional unit acting as a base for interdisciplinarity is, in fact, its organizational legitimacy. As competitive members of the "budget club", it is difficult for such organizations to provide organizational counterpoints or to act as agencies for disciplinary integration.

The institute or center is perhaps the most prevalent university-based organization for interdisciplinary research and research training and provides a fourth model. This structure is somewhat similar to the special department or college but has the added flexibility of being free of many constraints imposed by the traditional structure and/or organization. Most often responsible to a college dean and, as often as not, involved in instruction as well as research and/or service, the institute is not generally seen in the same light as a department. Its organization is extremely flexible. Therefore, it is better able to meet the dynamic organizational demands of transdisciplinarity. It is expected by definition to be atypical and is generally accepted by both faculty and administration as the most prevalent counter structure to the traditional disciplinary fabric of the university. Unfortunately, as indicated above, it seldom provides its administrator the luxury of line budget security.

It is interesting to note that of sixty-two scientific advances judged to be the most significant achievements made in social science between 1900 and 1965, twenty-seven were from projects conducted in institutes (Deutsch, Platt and Stenghaas, 1971). At the same time, the institute is frequently seen as being out of the university mainstream, as the result of inappropriate faculty entrepreneurship, and as generally "creating more problems than it solves" (Dressel, Johnson and Marcus, 1969). Dressel and his colleagues do qualify their position, however, by indicating that the institute has proliferated,

> "in great part, because of the fallibility of traditional academic departments whose instructional and research activities are tied tightly to the disciplines which justify their existence. Academic departments typically have neither the resources nor the interest to attack problems transcending their disciplines; their faculty members are uncomfortable when asked to operate outside the theoretical constructs with which they are most familiar, and the rigidity of departmental compartments provides no easy way to bring together faculty members and resources from several disciplines. Thus, when funds become available in problem areas not previously established as being of university concern -- or when the university is prodded into new concerns -- the institute provides a natural vehicle for assembling staff, attracting more funds, indicating institutional commitment, and determining responsibility and accountability of resources" (Dressel, Johnson and Marcus, 1969).

Stanley Ikenberry and Renee Friedman of Penn State University have written an extensive review and evaluation of the university institute that supports Dressel's qualifications. They comment that:

> "...institutes tend to be organized around tasks that, in contrast to those of academic departments, may involve more than one discipline. This essential difference, although elementary, is at the root of the added flexibility provided by their organizational form. Few indications suggest that institutes will replace academic departments in the forseeable future as the principal university organizational mode. Neither, however, is there evidence that demands for a task-oriented or mission-oriented posture on the part of universities will lessen. Thus, institutes are likely to continue to add a useful dimension to the overall organizational configuration. The issue is how institutes and centers can become more effective, better serve the purposes of the university as well as their own, and become more fully integrated in the life of the institution than they now are" (Ikenberry and Friedman, 1972).

The issues raised by Ikenberry and Friedman bring to mind Jantsch's (1970) conceptualization of transdisciplinarity. As previously indicated, he sees this as an integrated and dynamic goals system that brings the university more in sympathy with the contemporary needs of society. The authors visualize the institute as a medium within the traditional university organization for moving toward increasing transdisciplinarity.

If disciplines are to participate in a transdisciplinary structure, an explicit commitment to some organizational medium through which it can be accomplished is needed. To be meaningful, such commitment should have budgetary as well as philosophical reality. Just as individual professionals participate in interdisciplinary efforts through a joint effort of all members, several departments can band together through a neutral organizational "referee". Such a unit should not be a budgetary competitor in the line structure if it is to maintain its credibility as a neutral. The institute, organized in this context, can fill this role.

All of this assumes again that the university is, to a degree, Baconian in its orientation. It also assumes, however, that the functional and critical nature of disciplinary structures must be protected. It calls for complementary structures which can expedite interdisciplinarity as a supplement to disciplinary functioning.

SUMMARY

This paper has discussed some of the issues of organizational policy facing the university and its supporting agencies as academic scientists attempt to meet the problems of contemporary society, particularly as these problems focus upon educational research and research training and as they demand interdisciplinary efforts in the process of identifying solutions to these problems. Several points in this discussion seem focal, and for purposes of clarity they are summarized.

1) Any definition of interdisciplinarity leads to a model that necessitates coordinating goal structure with a system for integrating participating disciplines.

2) If any such interdisciplinary research innovation within the university is to be successful, it necessitates certain operational assumptions which include:

a) philosophical and budgetary commitment at all levels of university administration;

b) a university climate which is open to change;

c) a sensitivity by those who provide leadership to the social process of interdisciplinary efforts;

d) a dedication to flexibility and to a state of continual instability on the part of the agency facilitating the university's interdisciplinary thrust;

e) a well-developed definitional, attitudinal and procedural structure for expediting interdisciplinary communication;

f) and most of all a pragmatic sensitivity to the need for creative approaches to interdisciplinary innovation which complement rather than compete with the traditional organizational structure of the university.

3) Four organizational patterns are often employed in the establishment of interdisciplinarity at the university. They include:

a) the university organized around problem foci;

b) the traditional line unit (college and/or department) of the university;

c) a sub-program within such a unit; and

d) the institute or center.

4) The institute is seen as one creative organizational structure for the innovative development of transdisciplinarity in the university. Its atypical format allows it to flexibly meet the critical assumptions previously noted, and its position outside the budgetary line structure of the university (in most cases) allows it to assume the neutral role of transdisciplinary expediter for participating disciplinary units.

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Communication in International Interdisciplinary Research Management*

Dora G. Lodwick, Nancy Axinn, and
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International interdisciplinary research requires that scientists communicate with each other. Developing a common team language involves bridging cultural norms, personalities and disciplinary backgrounds. Critical aspects of communication in international research which is interdisciplinary are identified and discussed in this paper. The role of staff in a Management Office in facilitating this communication is described.

International interdisciplinary research management requires the development of a cohesive working team from a group of people with different professional perspectives, policy and procedural backgrounds, and national and cultural expectations. These characteristics create the dynamics to be managed in the formation of successfully functioning research units and in the conduct of research.

The Bean/Cowpea Collaborative Research Support Program (CRSP) described here is formed by eighteen research projects which have an applied, interdisciplinary goal--to address constraints to the production and consumption of beans and cowpeas, a major source of high quality, affordable protein and B vitamins in low income countries. Beans and cowpeas are dietary staples in the Latin American and African countries associated with this Program. They generally are grown on subsistence farms as food for household consumption, rather than as export crops.

These international interdisciplinary research projects bring together collaborating U. S. and Latin American or African institutions associated with this CRSP. Each project is coordinated by a principal investigator from one of nine U. S. lead institutions. These institutions act as the programmatic and financial agents for their respective research project(s).

The CRSP at Michigan State University is one of several international collaborative research programs developed under Title XII of the U. S. Foreign Assistance Act entitled "Famine Prevention and Freedom from Hunger." The Title XII Board for International Food and Agricultural Development (BIFAD) oversees the CRSP in conjunction with the Agency for International Development (AID), which is the fiscal agent for the Title XII Program (see Figure 1).

COORDINATION

The project teams for the Bean/Cowpea CRSP are coordinated through a Management Office at Michigan State University. This Office serves as staff to the Board of Directors, which is the policy-making component of the CRSP, whose members are elected from among the lead institutions. The Office also provides staff support to the Technical Committee, consisting of research scientists--five from the participating U. S. institutions, one from an international research center and one from a Host Country collaborating institution.

Axinn is currently the Women in Development Specialist, and Barnes-McConnell is the Deputy Director of the CRSP Management Office. Lodwick is a member of the Advisory Committee. They are social scientists from Michigan State University.

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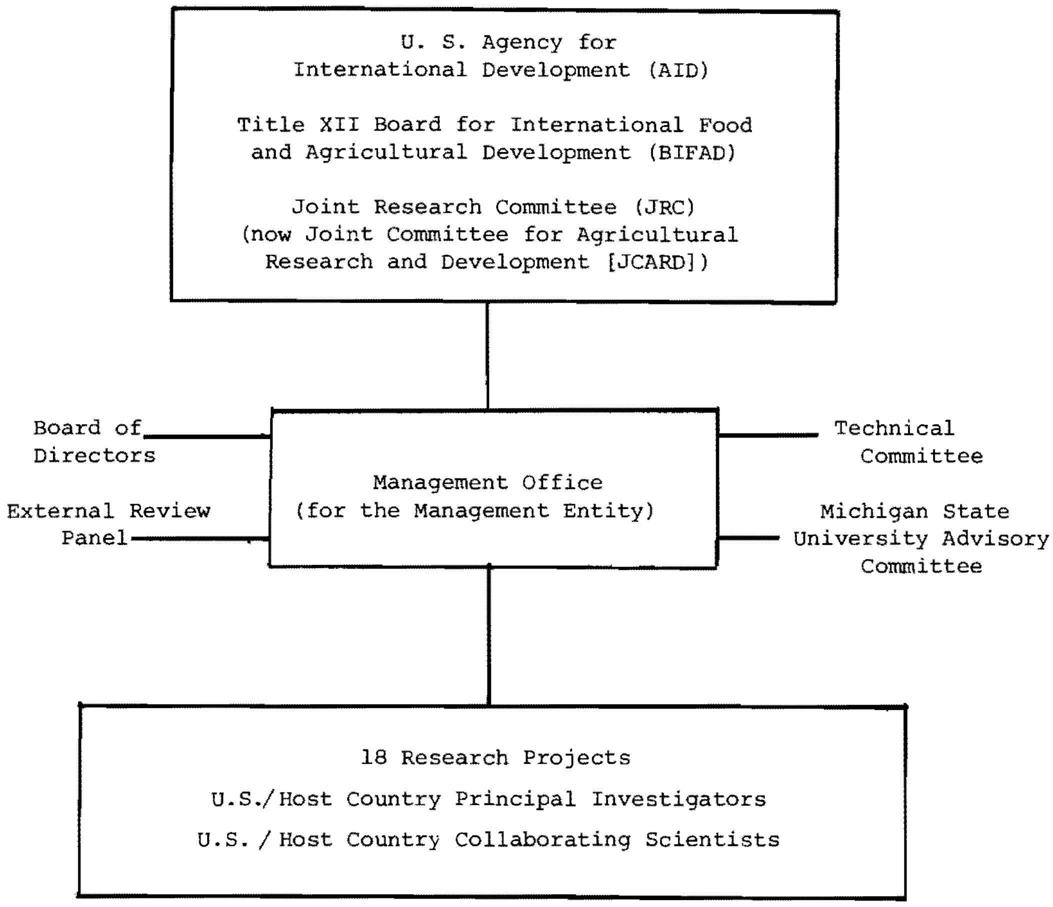
Figure 1. Program Management Within the Bean/Cowpea CRSP.



The Technical Committee advises the CRSP in areas of research technology, project management and technical review.

The Management Office brings together the various interests and resources to facilitate successful collaborative research. The development of research project goals consistent with the CRSP's tasks and philosophical orientation was facilitated by the Office. Subsequently, it has served as a center for accumulating lessons learned in implementing international interdisciplinary research, as well as for channeling scientific information generated by the research teams. The Office has also assisted with both fiscal management procedures and AID-mandated procedures, such as country travel clearances and equipment purchase approvals.

Additionally, the Office has facilitated communication necessary to the research process by encouraging open discussion, guiding team management and facilitating conflict resolution. Conflict management skills have been very important to CRSP scientists. Disagreements do arise, and working out ground rules for communication during conflict has enabled team members to move beyond these conflicts. Research team leaders and the Management Office staff have helped in conflict resolution, as well as in developing project evaluation procedures and other mid-course correction mechanisms. These procedures have been developed to accommodate the varying ways in which different social cultures handle collegial conflicts. The benefits of positive conflict within the interdisciplinary teams and the importance of open discussion during disagreements have been underscored (Birnbaum, 1979). The dynamics of this process are magnified in international interdisciplinary research teams.

LANGUAGE

Central to the long-term survival of the CRSP is the communication which occurs across disciplinary research (Cutler, 1979; Rossini et al., 1981; and Wilpert, 1979) as scientists learn to handle differences in meanings attached to similar words. For example, agricultural and social scientists mean very different things when they speak about "culture" and "race." Similarly, difficulties in associating equivalent meanings to terms occur in cross-national groups. For instance, a research "technician" in many Latin American countries is a field worker with minimal training and low budget cost, while in the U. S. a research "technician" is often a highly skilled specialist. These differences can be a source of genuine confusion or irritation in the early stages of research, until an understanding and an acceptance of these differences evolve into a team language (Wilpert, 1979).

This common team language is an essential component in interdisciplinary research and is, in international research, also infused with issues related to the historical patterns of national relationships. These patterns may determine the national language to be spoken during varying types of interactions. For example, one U. S. scientist discovered that during initial, formal negotiation meetings, the language spoken was that of the Host Country. However, during informal meetings among the same people, the group spoke English as a courtesy to the visitor.

International interdisciplinary research requires that scientists communicate with each other. In this respect, bilingual scientists have a great advantage, as they often are able to communicate more quickly and directly. In addition, they may become information coordinators since they are in a position to communicate and interpret for those who are not bilingual. This advantage has surfaced during planning discussions, fiscal allocation meetings, in conferences, and in scientific report writing. Translators who are not research team members are sometimes necessary to bridge both the disciplinary and national linguistic gaps. Although they provide an essential service, translators may also complicate team communications. By limiting the informal verbal and nonverbal cues which are so critical to full communication, translators are not able to completely overcome the communication barriers between colleagues. As documents and research instruments are translated and retranslated, this strained communication often persists, resulting in miscommunication due to unanticipated cultural connotation differences and to the cultural inappropriateness of the resulting research tools (Wagatsuma, 1977).

For a truly collaborative effort to develop, each scientist needs some language facility in the various disciplinary and national languages involved in the research activities. However elementary the language proficiencies may be, the effort to learn is seen by others as a courtesy and a sign of respect. Further, even minimal language skills have enriched the scientists' perceptions of the research tasks and the country.

However, even after scientists have carefully cultivated their language abilities and have developed consensus on team language, other impediments to communication have appeared. Electricity failures, telephone system breakdowns and undependable postal services have derailed communication efforts. U. S. scientists, in particular, have had to learn not to assume that messages sent were received. Furthermore, the three or more weeks needed for mail to be received tends to slow cross-national research progress rates and communication.

CULTURAL NORMS

International interdisciplinary communication is affected not only by the language(s) used and by the available communication technologies, but also by various cultural expectations. The Management Office has facilitated awareness of these through preparatory group meetings and with the distribution of pertinent materials to U. S. scientists selected to participate in CRSP projects.

Time and punctuality are concepts which vary across nations and disciplines. These can be great sources of frustration unless the differences are recognized and negotiated.

Cross-nationally, concepts of "working hours" vary. U. S. scientists working in some Latin American countries are surprised to find their colleagues stopping work for two hours in the middle of the day to join their families for a large meal. Cross-national understanding of punctuality varies. For professional appointments, being "on time" can mean anything from 15 minutes to hours "later." Deadlines, imposed on scientists by funding agencies and the CRSP administrative groups, have been the most difficult to manage, since they combine communication technology lags with differences in cultural responses to time. Norms related to the confidentiality of personal and professional communication vary among cultures. A letter may be read by many people on its way to the receiver or a telephone call may be shared by everyone in a large office.

The use of formal titles and positions in communications is more important in many Latin American and African countries than is usual in the U. S. The titles will often influence the quality of service and attention paid to the scientists.

Norms also differ regarding some of the inherent characteristics of the scientists, such as age, gender, and nationality. Age is highly respected in many countries of Africa and Latin America, thus conferring older scientists great authority. Since age is usually associated with more scientific accomplishments, older scientists are valued by the young Third World scientific communities.

Within the CRSP teams, women scientists have generally been fully participating colleagues.

However, in the social gatherings preceding or following business meetings, women scientists have sometimes been omitted. The importance of this omission varies with the culture, for in some places social gatherings are important professional communication events. Support staff, especially women, are often protective of U. S. women scientists when they are in a Host Country. Often U. S. collaborators--both male and female--are defined as "guests" being thus accorded greater freedoms than Host Country female collaborators, in particular.

Even scientists' communications across national boundaries are affected by nationalistic norms. U. S. scientists often have expectations based on their participation in a large, mature scientific community from a nation providing the monetary resource for the CRSP. Latin American and African scientists usually belong to small, youthful scientific communities which are building scientific infrastructure as well as research (Moravcsik, 1976). The Third World scientists provide access to resources such as indigenous germplasms, valuable farm management techniques, and field research sites in ecologies and social-cultural contexts unknown or not available in the U. S. The respect which Third World scientists have for the scientific abilities and resources of the U. S. is sustained when there is recognition of their own expertise. They are usually more experienced and skilled in interdisciplinary international research. The meaning of these differences becomes part of the team's communication processes.

The opportunity for frequent face-to-face interaction has been extremely important in overcoming cross-disciplinary and cross-national communication obstacles and for cementing personal relationships between scientists. In such contexts, nonverbal behaviors are important communication cues. The appropriate physical distances between people when speaking, standing, or sitting, for example, are culturally defined (Hall, 1959, 1966).

An important but often overlooked characteristic of long-term international interdisciplinary research projects is the personal relationships on which they are built. These relationships provide access to various professional and personal groups. African and Latin American scientists are often seeking methods of communicating with the international scientific community to overcome their own scientific isolation (Moravcsik, 1976). U. S. scientists can frequently provide a bridge to this group. The Third World scientists provide the U. S. scientists with links to their colleagues, political and economic leaders of their country, and to their family network. Communication links to the U. S. and Latin American or African scientific groups can operate through a supply line of journals and recent publications which is created by the collaborating scientists. This is especially valuable for Third World scientists. Thus, it has been important not only that U. S. and Host Country scientists establish open communication but also that, whenever appropriate, the visiting scientists become acquainted with the families, as well as the professional colleagues, of their counterparts.

PERSONAL CHARACTERISTICS

Because of the complexities, rewards and costs of long-term international interdisciplinary research, certain personal characteristics are advantageous. These factors loom particularly large because, as Rossini et al. (1981) have indicated, the social and intellectual processes are tightly intertwined in interdisciplinary work. This is particularly true if the work is also international.

Some criteria for identifying skilled cross-cultural research team members have been suggested by Brislin (1981). It helps, for example, if the person is:

1) Empathic or able to understand another person's perspective. The team member should be able to personalize information because so much of the communication occurs between people who do not share the same knowledge base;

2) Sociable or able to make others feel comfortable. In international team work, scientists depend on each other to interpret unfamiliar environments, provide reassurance and support collegial interactions';

3) Flexible and able to change roles rapidly, moving from leader to follower or vice-versa as appropriate. The capacity to relate to others with different competencies, cultures, disciplines, and gender without patronizing them has been extremely important in encouraging team interactions; and

4) Cooperative and possesses other personal attributes and skills, such as willingness to keep others informed, sharing ideas and reactions. These help promote effective team interaction.

Because of the many things which happen unexpectedly in international interdisciplinary research, successful team scientists also need a sense of humor, a willingness to "roll with the punches," and an openness to shared authority.

CONCLUSIONS

International interdisciplinary research management has great potential for handling significant global issues in complex institutional settings.

The Bean/Cowpea Collaborative Research Support Program is an example of such an effort which has attempted to bridge the cross-national and disciplinary gaps in building applied scientific research.

The Program has been developed on the commitment of the participants and on the initial professional and personal relationships they have established. The Management Office has facilitated the development of this commitment through communication which refines and supports the decision-making processes and through services which help to bridge the interpersonal aspects of international interdisciplinary research.

The Bean/Cowpea CRSP is still very new. How the structure will evolve to sustain the professional commitment, excitement and challenge over the long term is yet to be determined. However, early experience indicates that open communication is critical to maintaining the viability and dynamism of international interdisciplinary research teams. Such communication strengthens the use of international and interdisciplinary resources in building the knowledge necessary to address the limited availability of food for large segments of the global population.

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Farming Systems Research: Interdisciplinary Response to Problems*

Randolph Barker

This paper describes several aspects of interdisciplinary training needed to prepare people for research in farming systems. In its initial stages, farming systems research requires a holistic systems perspective on the farm and farm family (including non-farm activities) and an interaction or dialogue with the farmer or farm family. Especially in building an interdisciplinary perspective, both of these components can aid in identifying researchable issues which are appropriate to the context in which solutions are to be applied.

Scientists argue about the definition and meaning of "farming systems". There is, however, general agreement that farming systems research requires a holistic approach, and this in turn implies a team or interdisciplinary research effort. In this paper I propose first to define the problems associated with interdisciplinary research in farming systems. Then I will discuss the implications for training. However, before discussing the problems, I would like to characterize the various forms of interdisciplinary research.

BACKGROUND

In the early part of this century, interdisciplinary research was not recognized as such. The research staffs of the colleges of agriculture were small. As a result, there was a good deal of informal interaction among disciplines, just as there is today in the international agricultural research centers. In such an environment each individual's disciplinary research is likely to reflect to an important degree the influence of other disciplines.

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*Adapted from "Problems of Interdisciplinarity in Farming Systems Research," prepared for the AID-USDA sponsored Workshop on Farming Systems Research, December 8-10, 1980, Washington, D.C.

As the disciplines grew, however, communication among disciplines in the agricultural colleges declined until today it is not uncommon for scientists to work and socialize exclusively with people in their own discipline. Formal interdisciplinary research studies in agriculture began in the 1940's. The earliest that I am aware of is the research by Jensen et al. (1942) on input-output relationships in milk. In the 1950's and 60's, there was a great deal of interdisciplinary research of this type conducted between agricultural economists and biological scientists, particularly at Iowa State and Michigan State Universities (see for example, Hoffnar and Johnson, 1966). The degree of formal collaboration among researchers varied considerably from project to project. In some cases, individuals worked together under a broad mandate that allowed each to pursue his/her own interests. In other cases, a tightly managed group worked under a project director, meeting frequently to disucss the design and execution of the research and to integrate the results into a coordinated project.

Today it is still more common to find multidisciplinary projects loosely organized in such a way to enable each individual scientist to do his/her own thing than it is to find interdisciplinary projects. An obvious reason for this is the difficulty of conducting truly integrated interdisciplinary research. The obstacles encountered in undertaking such research are described below.

DEFINING THE OBSTACLES

There are three elements in farming systems research which, when analyzed, help to explain the difficulties associated with conducting interdisciplinary work in this area. These three, which I refer to as: (1) group dynamics; (2) the systems approach; and (3) the farmers' systems, are discussed in the subsections which follow.

Group Dynamics

A considerable amount of research has been conducted on group dynamics (see for example, Hare et al., 1955). Whether one is engaged in research or in other types of activities, factors such as size, composition, and leadership make a considerable difference in the efficient functioning of the group.

Interdisciplinary research adds still another obstacle, the problem of communication. In this respect, a distinction is often drawn to the gap between social and biological sciences. My remarks in this section draw heavily on a recent article by Dillon (1976) which I recommend as must reading to anyone with the general topic of farming systems research.

Most of us have grown up professionally in a world in which the furtherance of the discipline is taken as more important than the solution of people's problems. This is but the inevitable consequence of specialization and reductionism. Since the 1950's there has been a growing reaction to this trend; and expansionism, teleology, and synthesis are now being recognized by science as necessary modes of thought for understanding the world. It follows that a systems approach, based on teleological concepts and means-ends analysis, is slowly being introduced as a necessary corollary to the inadequacies of the old hypothetic-deductive method of research as a means of assessing goal seeking and goal setting systems. In this new approach, people consumate the system; and defining the "right" objective becomes more important than making the "right" choice between alternatives.

The Systems Approach

Dillon (1976) refers to the systems approach as a new technology. He is quick to point out that rapid adoption is unlikely since academics represent perhaps the most conservative purposive element of the agricultural system.

There is, in addition, not a single system within agriculture but rather a whole hierarchy of systems of which the farmer's system is only one sub-system. However, given the dominant role of the farmer in this sub-system and of people as either producers or consumers in higher order systems, the implication is that the socio-economic sphere is more important than the physical and biological sphere in the choice of research directions.

Traditionally, our research has been production oriented. This production orientation has been transferred to the developing countries through the training of scientists in our traidition and is still dominant today in the international agricultural research network. The success of this system in terms of the traditional orientation of research has tended to emphasize its short-comings in the modern context of the systems approach. Improving merely a part of the system or a sub-system cannot be presumed to lead to enhanced performance of the overall system. Although it is politically more expedient in the short run for agricultural scientists to ignore the socio-economic consequences of their work, there is obviously no guarantee that research progress in the traditional mode will lead to social justice. The growing interest in farming systems research stems in large measure from a recognition that research efforts of the recent past have not resulted in social justice for the resource-poor farmers of either the developed or the developing world.

The Farmers' Systems

Gotsch (1977) suggests that "the farmer's system" is a more appropriate term than "farming systems" since it lays the emphasis where it should be. During the early part of this century, students and faculty alike came primarily from farming backgrounds; and they brought with them an appreciation of rural values and a capacity to communicate with farmers. Today most of our graduate students and a good many of our faculty do not have farming backgrounds. Even more have not had contact with farmers in the past year. I would argue that a lack of experience in American farming is a handicap to many scientists engaged in research directed to assist the farmers of developing as well as developed countries.

Often the weakest link in farming systems research is the communication (or lack there of) between scientist and farmer. This problem and some suggestions to improve communications are the subject of an excellent paper by Chambers (1980), "Understanding Professionals: Small Farmers and Scientists".

Unfortunately, much of what passes for communication today takes the form of an interview questionnaire now referred to professionally as the "survey instrument". The trend toward the use of the survey questionnaire is as pronounced in the developing as in the developed countries, since surveys tend to be rather cheap and easy to administer. The problems come later when one attempts to transfer the data to computer tape and analyze the results. Without minimizing the importance and utility of good surveys, we need to more carefully consider alternative ways of communicating with farmers, particularly in identifying their goals and objectives, in evaluating their methods of classifying resources, and in valuing indigenous technological knowledge. In this task it would appear that rural sociology and anthropology should have increasingly important roles to play on the farming system team.

IMPLICATIONS FOR TRAINING

Dillon (1976) indicates that the adoption of the systems approach to research will require a complete shift in the emphasis on professional training. He suggests, instead of the typical pattern, an initial (one year) introduction to the systems approach followed by a (two year) period of disciplinary specialization, capped off with bringing together different disciplines in the context of some relevant agricultural system.

In its most fully developed form, this approach would call for a new set of majors to replace traditional disciplines such as agronomy, plant breeding, and agricultural economics. Systems majors might include such titles as crop-soil systems, plant-animal systems, or farming systems. Research funds would be reallocated accordingly to multidisciplinary groups or systems teams. Dillon's view may represent nothing more than a vision of the distant future which many scientists would regard with fear and trepidation. But there are already signs that some of these changes are coming to pass, not only in the newly formed international research centers, but also in the more hallowed universities and colleges of agriculture.

Ealey (1979) describes a program for training environmental managers to work in multidisciplinary teams. Monash University in Australia offers a Masters of Environmental Science. A core staff of three faculty organize and administer the program and coordinate the activities of about 80 other staff members from all faculties who teach or supervise over 100 candidates.

The program consists of a course work component and a research component. The course work is intended to broaden insight and provide an opportunity to improve the depth of understanding in areas of previous training. The research component is designed to provide practical training in multi-disciplinary group research. Teams of three to five candidates, each one from a different discipline, work together on a part-time basis over a two-year period. Each candidate is involved in the production of two documents -- a group report integrating the work of the team and a minor thesis which details the work which each individual performed as his/her contribution to the group report.

Efforts at interdisciplinary research and training at Cornell have been more modest than those described above, being based on individual projects rather than on a program. Perhaps the most ambitious of these involved 14 faculty and graduate students from 7 disciplines in a study of nitrogen and phosphorous in the environment (Porter, 1975).

Another project involved six disciplines at Cornell linked with the CIMMYT corn program. Ph.D. students from each of these disciplines -- agricultural economics, agronomy, biometry, entomology, plant breeding, and plant pathology -- conducted their research on various aspects of corn production with field work being conducted in Mexico (Contreeras et al., 1977).

A course in water management was initiated in the mid-1970's with instruction from faculty in three disciplines -- agricultural engineering, agricultural economics, and rural sociology. The faculty teaching this course developed a US/AID supported research project, "The Determination of Developing Country Irrigation Project Problems," with field work being conducted in South and Southeast Asia. The course provides training for graduate students who have later participated in the US/AID project or have returned to conduct water management research in their own countries.

Currently, our only ongoing farming systems research project is entitled, "Technology Introduction into Traditional Farming at High Elevations". Two graduate students, one from rural sociology and one from plant breeding, are just completing a study of farming systems in the Andean Mountains of Equador where they have lived in a small village with their families for the past two years.

Following the model used in water management, we have been experimenting this fall in the development of a course in farming systems. Ten faculty and twenty-four students have been involved in this experiment. Approximately 50 percent of the time was devoted to weekly discussions, usually based on presentations of one or two faculty members. The other 50 percent of the time was spent in small group exercises. Students were divided into four mixed-discipline teams of six members each and were asked:
(1) to present a case study of farming systems

research, and (2) to identify and study a local (New York State) group of farmers in order to be able to describe their farming system and suggest a research agenda.

I will not go into any further details regarding this course, other than to say that it has tended to highlight the barriers to interdisciplinary research that I have described in this paper. Faculty presentations tended to emphasize the familiar discipline approach rather than the systems approach. The field exercises proved to be valuable, if for no other reason than providing most graduate students with their first opportunity to interview American farmers.

CONCLUDING REMARKS

In this paper I have not dealt directly with what we understand by the term "farming systems". But as a minimum it would seem that farming systems research should, in its initial stages as an interdisciplinary team undertaking consist of: (1) a holistic systems look at the farm and farm family, including non-farm activities; and (2) an interaction or dialogue with the farmer or farm family. Both of the above elements should have the purpose of aiding in the identification of the appropriate, researchable issues. Based on these criteria, there are very few studies in the literature today which could properly be classified as farming systems. New concepts of interdisciplinary training are clearly needed to prepare people for research in farming systems.

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(FINELY TUNED?!?) INTERDISCIPLINARY
PROJECT TEAM
I guess we all Know why we are all here.
who will offer the opening prayer?
I tink dat I haff the answer to all prayers
How are we to evaluate that
Wot the hell are Y'all talking about?
what are the payoffs?
Can we process this?

Creating Administrative Environments for Interdisciplinary Research*

Martha Garrett Russell and Richard J. Sauer

The leadership of experiment station directors is essential in maintaining steady funding for research programs, in cultivating communication about research needs and outcomes, in promoting the transfer of new technologies and solutions generated by research, and in maintaining interfacing mechanisms between cooperating sectors. This paper addresses the role of state experiment station directors in creating an administrative environment conducive to interdisciplinary research and reports the perspectives of the directors on the management of IDR in their stations.

New technologies produced by research have changed agriculture in many ways. New knowledge, also generated by research, has increased our understanding of the complex linkages between agriculture and its socio-political, economic, biological and human contexts. These new technologies and knowledge have emerged when scientists have successfully applied disciplined analysis to the important problems and issues confronting agriculture and its related concerns.

Interdisciplinary efforts in research have provided a mechanism by which real problems can lead the development and application of science. This paper addresses the role of interdisciplinary research in the agricultural sciences and reports the perspectives of state agricultural experiment station directors on the management of interdisciplinary research in their stations.

The potential of interdisciplinary research to address the complex issues of today has been widely acknowledged. This potential is comprised of basically three opportunities. First, interdisciplinary research efforts provide a scientific arena in which the conceptual frameworks, the methodological approaches and the analytical tools of a discipline are challenged (Russell, 1982). This challenge offers to scientists using that disciplinary approach the opportunity to clarify the scope of that approach. In this confrontation lies a tremendous potential for the refinement and advancement of the disciplines. A second related opportunity challenges a scientist to defend and expand his or her own repetoire of methodological approaches and analytical tools. In this opportunity lies the potential for the ongoing professional development of scientists.

Thirdly, interdisciplinary research efforts offer to students working with the scientists the opportunities to learn scientific approaches to the contradiction of simultaneous phenomena, the analysis of complexity, and the integration of multiple criteria. Interdisciplinary research efforts relate disciplined analytical approaches to the wider environment and develop tools to deal with complex, real problems.

This complexity in research problems has heightened the need for skillful administrators to balance the sometimes conflicting interests of scientists and of the institutions in which they work. The demands of today's research enterprise challenge both scientists and administrators to work within the constraints of multiple sponsorship, shifting financial parameters, multi-layered administrative structures, and a changing cultural

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milieu. This paper addresses the role of administrators in creating an administrative environment conducive to interdisciplinary research.

The state agricultural experiment stations (SAES's) have, for one hundred years, been applying expertise from many academic areas to address complex agricultural problems. In many cases, these problems have been larger than any one discipline or field of study and have required the best efforts of several fields. The state agricultural experiment stations are well suited to conduct interdisciplinary research because, according to the associate director of one station,

> "State Agricultural Experiment Stations have the primary research leadership and capabilities in the U.S. and consequently have the responsibility for focusing these resources on agricultural problems.
>
> "The private sector is unable to invest in many types of research since the investment cannot be protected through patents or other proprietary interests. Unless SAES's conduct interdisciplinary research, these needs will go unmet in the agricultural sector."

Indeed, some of the successes have demonstrated a handsome return on investment in agricultural research (Evenson, Waggoner, and Ruttan, 1979).

To reflect on how SAES's have mobilized scientific expertise to solve problems of the agriculture and people of each state, a survey was developed to draw out insights from the SAES directors on thirteen issues considered by the authors to be essential in administering IDR. One survey was sent to each SAES - 53 in all. Open-ended questions were phrased to shed light on the role of the experiment station director's office in facilitating these successes. Specifically, the survey asked directors to identify the administrative strategies and practices which have been useful in accomplishing interdisciplinary research. Answers were often multiple, although not all of the questions were answered by every director.

Because the state experiment stations are organized somewhat differently from each other, we asked that the survey be completed by the director or his designee responsible for interdisciplinary efforts. Completed surveys were returned by 37 of the 53 state experiment stations. Responses came from 15 directors and 22 assistant or associate directors. The median history of administrative experience for respondents was 12 years and ranged from 1 year to 26 years.

Their responses provide important insights into the considerations important for successful interdisciplinary research efforts and into the administrative strategies which have been useful to achieve those successes. Such considerations stem from many issues and converge on station directors' decisions, initiatives and responses.

All of the respondents indicated that interdisciplinary initiatives are an important dimension of leadership from the director's office. While the importance of station leadership in IDR varies among SAES's, the majority of respondents emphasized that IDR management is a top priority in station administration. As one director reflected,

> "Aside from the responsibility for maintaining the financial resources of the Experiment Station, facilitating interdisciplinary research is the most important function of the director's office".

According to another,

> "IDR is the natural result when scientists see the benefits of working together. We do not think this can be accomplished by administrators, but it is important to provide the climate for natural cooperation among scientists and encourage IDR when a genuine interest is present."

And from another director, a somewhat different perspective,

> "I don't know that anybody other than the administrators of the experiment stations who can encourage IDR as well across the entire organization. The role of the experiment station director in facilitating IDR varies depending on the particular situation. For example, should an emergency problem arise, the role of the director should be to initiate an IDR effort as quickly as possible by direct intervention across departmental lines. In less critical situations, the director should, through departmental administrators and where needed, encourage IDR. In direct contact with the faculty, the director should at all times encourage teamwork as an important ingredient in building a strong research organization."

INCENTIVES TO INDIVIDUAL SCIENTISTS

Some directors indicated that while facilitating IDR is an important concern of the station office, the responsibility for initiating, planning and carrying out interdisciplinary research resides with the researcher.

> "We can suggest, urge, indicate, recommend, and so forth, but in the final analysis the concept can only get off the ground if two or more scientists are willing to pool their knowledge and dedicate their talents in a joint undertaking that hopefully will result in the synergism (hybrid vigor) increment that comes from the team concept."

Directors were asked to indicate what, in their opinions, motivates individual scientists to participate in interdisciplinary research. Their responses described two types of motivators -- expectations of internal satisfaction and expectations of external rewards (See Table 1).

Table 1. Incentives to Individual Scientists to Participate in IDR, as Indicated by SAES Directors

	No. of respondents mentioning factor (37 total)
Incentives to Individual Scientists	
•Internal Satisfaction	
Opportunity to work on real problems	18
Synergy of group effort	28
Extend knowledge base	15
•External Motivators	
Expand own research program	25
Increased visibility and recognition	11

Internal Satisfaction

Three internal motivators were identified in the directors' responses. One of these centers on the scientists' perceptions of the problems to be addressed by interdisciplinary research. Eighteen out of 37 respondents indicated that the opportunity to work on real problems motivates scientists to participate in IDR. These directors explained that many experiment station scientists feel a sense of responsibility to the agriculture and people of the state in which they live and work. In keeping with the land grant philosophy, many scientists want to solve problems which confront their state's agriculture and rural communities.

Directors indicated that scientists who consider a particular problem important participate in IDR because they want to help identify solutions. These scientists recognize that those solutions would not be attainable through individual expertise. According to some directors, the scientists who express the need for additional expertise in addressing problems are apt to be those who have a broad perspective on the importance of research and who are attracted to the type of problems for which the interdisciplinary approach is appropriate.

The other two internal motivators identified by directors' focus on the synergy of team efforts. Twenty eight out of 37 respondents indicated that scientists are motivated by the belief that the group effort will be stronger than an individual effort. These scientists expect that they will be better able to achieve their own goals, as well as the solution of the problem, by working with a group of scientists.

Fifteen directors replied that some scientists were motivated by the desire to broaden their own base of knowledge and by the attractiveness of working with successful associates. In addition, a few directors indicated that scientists drawn to IDR are likely to be those who enjoy opportunities for intellectual challenge and idea exchange. Internal considerations seem to be important to scientists such as these.

External Motivators

The perceived potential to expand their own research program is also a primary attraction to participate in IDR programs. Twenty five of the 37 directors indicated that the promise of funding encourages scientists' interests in IDR efforts. Some directors stated that scientists are attracted to IDR efforts by the expectation that they can obtain additional funding for their projects. In one director's opinion, "interdisciplinary research proposals have been developed almost entirely in response to new funding opportunities".

Incentives provided through the scientists' home departments have been major motivators for interdisciplinary collaboration. One viewpoint stated that:

> "Department incentives are principally the signals given by the unit administrators regarding their appreciation and encouragement of involvement in interdisciplinary research. Scientists will respond to what they perceive to be their unit administrator's expectations of their

performance. If that view is very narrow, they will respond accordingly. If they perceive that involvement in interdisciplinary research is encouraged and will result in positive evaluations of their performances, they will more aggressively seek such opportunities."

Eleven directors reported that increased visibility and public recognition for scientific accomplishment were important motivators. Other incentives identified by directors included the opportunity for scientists to enhance their publication records, increased access to facilities and equipment, and the promise of personal reputation being enhanced.

One associate director stated the case this way,

> "The overall recognition by the Experiment Station and Department Chairperson of the importance of interdisciplinary research and the possibility of additional funding are the main incentives that have influenced participation of individual scientists in interdisciplinary research.
>
> "A clear recognition of the policy and positive reinforcement of interdisciplinary research plus the hiring of individuals that are interested in team efforts and supplying of funds from the Station for interdisciplinary efforts are the major ingredients in providing for successful new interdisciplinary activities.
>
> "A considerable amount of time is necessary at the initiation of any interdisciplinary research for scientists in different disciplines to learn to communicate with each other. Thus, in any interdisciplinary research effort, one must realize that a considerable amount of time up front is required so that individual disciplines, spheres of terminology, and differences in background can be overcome so that a commonality starts to evolve."

DEPARTMENTAL INCENTIVES

Some directors emphasized the roles of departmental administrators in enabling interdisciplinary research. Directors were asked what strategies have been successful in bringing about collaboration between departments which are otherwise competitive for resources. The climate of departmental competition was described by one director as follows:

> "Our traditional structure of academic departments with strong department heads and the placement of essentially all Station appropriations in the continuing base budgets of these departments has resulted in strong disciplinary research programs. However, it has not allowed us to reach an equal level of quality and responsiveness in interdisciplinary research. Department heads are by nature very competitive beasts and some are not willing to share any "departmental" resources to facilitate collaboration with other departments. Much of the interdisciplinary research that is underway currently in the ... Station is being conducted in spite of the administration and our research management operation, rather than being facilitated by us."

Strategies to encourage interdepartmental collaboration clustered into basically four categories: recognition, rewards, leadership, and factors associated with the problem itself (See Table 2).

Table 2. Departmental Incentives to Participate in IDR, as Indicated by SAES Directors

	No. of respondents mentioning factor (37 total)
Departmental Incentives	
• Recognition	
Acknowledgement by superiors	10
Peer recognition	9
• Rewards	
Financial support	7
Performance evaluation	5
• Leadership	14
• Problem Focus	7
• Communication	7

Recognition

One half of the directors responded that recognition is a very important incentive for departmental participation. Ten of these 19 directors indicated that recognition by superiors is important in getting department heads to encourage their faculty to participate in interdisciplinary research. The enhanced image of the department vis a vis central administrators was mentioned as one such type of recognition. Many of the directors who acknowledged recognition as an incentive to departmental participation indicated that station administrators play a key role in maintaining an environment toward IDR which department heads recognize as enthusiastic and supportive.

Peer recognition was also mentioned as an incentive for departmental participation. Departmental administrators, the directors felt, are encouraged by recognition from their colleagues in two ways - publications and prestige. Jointly authored publications resulting from the collaborative work of their scientists and scientists from other departments boost the department's overall publication productivity rate. Collaboration also increases the visibility of the department within the scientific community and within the university. Especially when the collaborating scientists come from a notable department, the opportunities to enhance the prestige of the cooperating department through interdisciplinary efforts is an incentive.

Rewards

Twelve directors said that rewards are important in motivating department heads to encourage interdisciplinary efforts. Two types of rewards were mentioned. Seven of these twelve directors mentioned that providing facilities, equipment and financial resources for those who show a willingness to work together was a strategy they had found successful. One director stated that:

> "Since many Department Chairs feel that interdisciplinary research detracts from the work within the discipline, they are reluctant to encourage faculty to participate. By using outside funding earmarked for interdisciplinary research, a Department Chair can visualize such research as augmenting a Departmental program."

Five directors responded that the performance evaluation of both faculty and departmental administrators offers an excellent opportunity to recognize and reward interdisciplinary efforts.

Leadership

Encouragement, through rewards and recognition, is an important component in departmental leadership of a research program. Leadership was mentioned by 14 directors as an important force in enabling interdisciplinary research. About half said this leadership requires that department heads explicitly allow time for non-departmental research and facilitate linkages between departments. Other directors stated that the interdisciplinary leadership of department heads has been aided by encouraging communication concerning interdisciplinary research opportunities among department heads and by generously recognizing effective interdepartmental research efforts.

One respondent explained the departmental influence this way:

> "The attitude of the department head is the key determinant of superb participation by scientists in the department. Since motivation of the department head is sometimes influenced by factors outside the experiment station, it is incumbent upon the director to work closely with those who need counselling. When the department head insists upon singly authored refereed journal publications as the prime determinant of productivity, interdisciplinary research is virtually impossible. When the department head encourages and rewards cooperation across departmental lines, most faculty members are anxious to participate."

Problem Focus

The importance of the problem in encouraging interdepartmental collaboration was identified by seven directors. When the problem becomes the research focus, the need for expertise in other departments is more apt to be recognized. Through the synergy of interdepartmental efforts, individuals are more apt to be successful in accomplishing mission-oriented objectives and contributing to complex agricultural problems.

Seven directors said that open, trusting communication among all parties about the problem and the collaboration is the key to eliciting the participation of various departments and their faculties, and to effectively maintaining cooperation. By encouraging communication at all stages of a project's development -- from identifying the problem to implementing the research program to evaluating project success -- the collaborating departmental administrators and scientists are better able to understand their own roles vis-a-vis the expertise and commitment of others. It is in the reciprocity of this understanding that trust between interdisciplinary cooperators is nurtured and that commitment to collaborative problem solving is established.

Intersectoral Collaboration

Departmental and station administrators respond to a variety of sponsorship and accountability demands in managing the research activities conducted at state agricultural experiment stations. These directives encompass both local and national influences through the integration of research initiatives generated by feedback from state and local constituencies. They include regional and national research planning and priority setting. Frequently, these demands also reflect the research missions of various federal agencies -- NSF, NIH, DOD, DOC, and EPA, as well as USDA. Yet another dimension of accountability grows out of the experiment stations' affiliation with land grant colleges and state universities and the traditional research, teaching and extension interactions.

Such a variety of influences requires multi-faceted administrative vision and leadership. Because some of the unnegotiable demands on the research program are in conflict with each other, station (and departmental) administration requires a creative integration of activities. The tensions between national, regional and state research missions are one such challenge in directing the research programs of state agricultural experiment stations. The antagonism in universities between academically- and mission-oriented research is another. Some of these activities serve the station's major objectives more directly than others.

UNIVERSITY INCENTIVES

Six out of 37 directors responded that incentives to participate in interdisciplinary efforts were comparable between their experiment station's and their university's administrations. Of the others who felt there were discrepancies between station and university incentives, most of the differences clustered around three major considerations --research objectives, funding of research, and faculty evaluation (See Table 3).

Table 3. University Incentives for IDR Collaboration, as Indicated by SAES Directors.

	No. of respondents mentioning factor (37 Total)
University Incentives	
•Research objectives	3
•Funding	11
•Faculty evaluations	6

Research Objectives

Related to central administration's endorsement of interdisciplinary activities is the concern, mentioned by three directors, that a major disincentive to interdisciplinary efforts can be the cleavage between university and station research objectives. University research is typically academically oriented, emphasizing the establishment of departments in which students can be educated and the conduct of research which builds the knowledge base of that department. Experiment station research is, by nature of the federal legislation which provides its funding, problem-oriented.

The faculty who perform station research are also responsible to educate students. To provide the highest quality of instruction, to maintain their academic reputations, and to cultivate their best expression of expertise, experiment station faculty also have a stake in disciplinary research.

These two orientations - discipline and problem - are brought together differently in various experiment stations. They represent not only the basic and applied distinction in research but, in some institutions, represent the competition or cooperation between station and university research thrusts. When these orientations are polarized, the challenge to station directors to achieve station objectives within the university environment becomes much more difficult. The optimum climate for interdisciplinary research in experiment stations includes a compatibility between discipline and problem oriented research - not only in the experiment station, but also in the university as a whole.

Funding

Some stations enjoy a great deal of autonomy in administering research programs; others are more constrained by university-based goals. In the words of one station director: "He who has the gold rules." Eleven directors regarded funding incentives for interdisciplinary research to be superior at the station level, rather than at the university level. Some considered the dependency of station scientists on university funding sources to be an impediment to interdisciplinary research efforts. Such dependency tends to work against a problem focus and encourages individual rather than collaborative efforts, according to several directors.

The important role of funding in influencing interdisciplinary research was described by one director:

> "Funding opportunities have positively influenced the development of interdisciplinary research efforts within the ... Agricultural Experiment Station. Interdisciplinary projects have been developed in response to funding oppor-

tunities from the various commodity check-off boards. The .. Center funding of research projects has encouraged several interdisciplinary projects. Seed money funding from the Experiment Station Director's office encouraged the development of the .. Research Group. Other funding opportunities such as research initiation grants, university foundation funds, etc., have attracted numerous interdisciplinary research proposals.

"As a general observation, small investment in terms of research initiation funding of seed money has had the positive result of creating the setting for greater interaction among research scientists from different disciplines, which in turn has stimulated further development of their common interests into greater interdisciplinary involvement."

Another director identified two ways in which funding influences the initiation of interdisciplinary research projects:

"IDR is fostered by two extremes in resource availability. (1) Availability of extra funds - when IDR is a requirement of receiving extra funds, IDR programs are fostered. The availability of new resources solicits cooperation. (2) Extreme poverty or limited resources foster IDR, where scientists pool limited funds to accomplish common goals for their common benefit."

The principal impact of dependency on university funding for research may be that it constrains the ability of directors to respond to research initiatives which might contribute substantially to SAES goals but less to university research priorities.

According to one respondent,

"The key is to have enough flexibility and a capacity to move resources within the system."

Faculty Evaluation

Another very important factor in the relative influence of station and university incentives is the authority for promotion and tenure decisions. Four directors indicated that at their universities, central administration's influence on promotion and tenure was stronger than the experiment station's influence. Two other directors maintained that the more centralized a university's administration was, the less powerful were the incentives created by the colleges or the experiment stations. Yet several directors acknowledged the positive influence that university incentives can have on interdisciplinary activities. Encouragement, endorsement of the interdisciplinary concept, and policies which permit interdepartmental flexibility in grant accountability were all mentioned as ways that university incentives can augment experiment stations' initiatives for IDR.

The prestige of a university is built by the prestige of its academic departments, which is founded on the academic reputation of its faculty and the accomplishments of their graduate students. Faculty are evaluated by peers in scientific communities and by peers and administrators within academic institutions. Peer evaluations influence the standing of faculty with respect to their academic reputations. Administrator evaluations determine the external rewards by which faculty accomplishments are recognized.

The evaluation and endorsement of interdisciplinary activities by university administrators are not always consistent at each level. When top level administrators favor disciplinary research, scientists who participate in interdisciplinary efforts are viewed as, according to one director, second rate scientists, even though their research has significant impact on real problems.

When, on the other hand, the value of interdisciplinary collaboration is recognized by key academic administrators in a university, promotion and tenure decisions are apt to reflect such participation in a positive manner.

In interdisciplinary research, problems are usually tackled by a team of researchers. Performance evaluation of the team, however, is specific to the individual. At both the departmental and individual levels, congruency in the acknowledged value of interdisciplinary research and consistency in the rewards and incentives are major considerations in providing an environment which encourages interdisciplinary activities.

SUMMARY

The responses of the directors indicate what, in their opinions and from their experiences, are the important factors which enable interdisciplinary research in agricultural experiment stations. In other words, each director as respondent was firstly aware of the factor and, secondly, considered it sufficiently important to include in the response. These responses were made to open ended questions and consequently varied in format and specificity.

Table 4. Factors Influencing AES Encouragement of IDR, as Mentioned by SAES Directors.

	# respondents mentioning factor (37 Total)
•Incentives to Individual Scientists	
Internal Satisfaction	
Opportunity to work on real problems	18
Synergy of group effort	28
Broaden base of knowledge	15
External Motivators	
Expand own research program	25
Increased visibility and recognition	11
•Departmental Incentives	
Recognition	
Acknowledgement by superiors	10
Peer recognition	9
Rewards	
Financial support	7
Performance evaluation	5
Leadership	14
Problem Focus	7
Communication	7
•University Incentives	
Research Objectives	3
Funding	11
Faculty Evaluations	6

Preliminary classification of the responses into categories was based on Russell's dimensions of collaboration (Russell, 1982). Further differentiation of clusters followed the confluence of concepts identified in the responses (Table 4). Responses within a particular category are not mutually exclusive. Therefore, no attempt has been made to establish a ranking of factors by importance. Rather, one may note that, for example, more directors were aware of and commented on the importance of group synergy and research project expansion as incentives for individual scientists to participate in IDR than on the importance of scientist recognition or broadening the knowledge base.

However, it is not possible from the responses to make any inferences about the relative importance of individual versus departmental or university incentives in enabling IDR. That the same factors were expressed in many different ways is indicative of the variety in administrative challenges and leadership approaches of the directors who responded.

The success of a university is built by the academic reputation of its faculty. The success of an experiment station is also founded upon the academic prestige of its top quality faculty and graduate students. However, it goes beyond academic prestige to include the ability to marshall research resources to develop solutions to problems expressed by the agriculture and the people of that state. The ability to serve the mission and achieve identified goals is a key determinant of the success of an experiment station.

Experiment stations are in the business of solving problems through research. The best approach to complex problems may be the approach which permits the problem to lead the research in the definition of the question, in the selection of the method, in the analysis of the findings, and in the judgements about those findings. For some problems the research methods of one discipline may offer an appropriate approach. For other problems which go beyond the issues encompassed by that discipline a new approach must be identified. The best contributions of several disciplines, integrated specifically to address the problem, are needed. In either case, the critical issue is that the problem be successfully addressed.

Interdisciplinary research is not an end in itself. It is a tool, a tool which has great value in grasping complex issues, in developing new scientific methods, and in relating the scientific process to real problems. As a tool, it is best used in the context of a complete and well-managed workshop which includes a full set of basic tools, adequate facilities and reliable support systems.

The disciplinary strength of academic programs is a key determinant of the faculty who remain in a department and of the quality of graduate students who apply to and are accepted into graduate programs. These manpower resources are critical in the use of the disciplinary tools in research programs. In the same manner, departmental administration is a key management system through which the energy and resources of the workshop are maintained.

The best tools in the best workshop, however, must be skillfully used, each to its best use, depending on the desired outcome. The leadership of experiment station directors is essential in maintaining steady funding support for the research programs, in cultivating opportunities for feedback about research needs and outcomes to administrators and scientists, in promoting the transfer of new technologies and solutions generated by research, and in maintaining flexible and well-functioning interfacing mechanisms between cooperating sectors.

To forge a research environment which maximizes the skills of scientists while achieving the institution's objectives requires the integration of several layers of leadership and bureaucracy -- both formal and informal -- into a climate of scientific integrity. To identify solutions to complex issues demands that the best scientific tools be brought to bear on the problems at hand and that the continuous development of scientific expertise remain responsive to the highest priority problems. Finely tuned disciplinary expertise and well-directed interdisciplinary efforts are both essential for such a research environment. The administrative climate determines whether these resources will be used in competition or in cooperation.

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A New Approach for Planning and Coordination of a Large Project*

Keith R. Shea and Ned D. Bayley

This paper describes a new approach used to plan large research and development programs in the U.S. Department of Agriculture: the Adapted Convergence Technique for Agricultural Research (ACTAR). This is a technique for planning and organizing research and development activities into a series of flows and arrays through a program structure of phases and subphases to attain specified program objectives within a given time frame. ACTAR was used to plan a new USDA Combined Forest Pest R & D program in which four Federal agencies in USDA, many state agricultural experiment stations, universities and colleges, and several state forestry organizations are cooperating.

Thorough and well-coordinated planning can provide sound bases for program funding and implementation, for program management, for progress reviews and adjustment for disseminating results, and for obtaining public support for needed research and development efforts. Conversely, inadequate, fragmentary and unorganized planning results in programs implemented by whims and crises, in confused and ineffective management, in noncoherent reviews, and in deteriorating credibility with users and the supporting public. One approach which was used successfully in planning three major forest insect research programs is described in this paper.

ADAPTED CONVERGENCE TECHNIQUE FOR AGRICULTURAL RESEARCH (ACTAR)

The National Cancer Institute (NCI) developed and has used the Convergence Technique for research planning for several years (Carresse and Baker, 1967). The Convergence Technique utilizes some of the general features of systems and network approaches to planning but avoids the requirements of certainty in achievement and rigidity of time scheduling.

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The NCI Convergence Technique was modified and adapted in the U.S. Department of Agriculture. This modified technique, Adapted Convergence Technique for Agricultural Research (ACTAR) was used to plan the USDA Combined Forest Pest R&D Program. The Program is designed to provide technology and tactics for suppressing and preventing damage by three major forest insects -- gypsy moth, Porthetria dispar L. (Lymantriidae); southern pine beetle, Dendroctonus frontalis Zimm. (Scolytidae); and Douglas fir tussock moth, Orgyia pseudotsugata McD. (Lymantriidae). Budget projections for the three insect programs total $46.8 million over a 5-year period.

ACTAR involves the organization of research activities into a series of flows and arrays through a program structure of phases and subphases designed to meet a specified program objective. The program is developed sequentially on the basis of research logic. ACTAR, as presently developed for use in planning forestry and agricultural research, is described in this paper. The Douglas Fir Tussock Moth Program illustrates how the ACTAR technique was used.

The Planning Team

Planning must be done by a small group. Experience suggests the best teams are made with five to seven members. The planning team should include: a generalist with comprehensive knowledge of the whole field under consideration; two to five specialists in the subject matter being considered; and one person who understands the ACTAR technique.

A definite time frame should be established to complete the total planning effort. This imparts a sense of urgency and increases team efficiency. Team members should expect to devote full time to program planning without interruption. It is essential that secretarial services be adequate to permit rapid completion of drafts during the planning. Any major break in the planning process results in time losses for regrouping and reviewing progress.

Program Objective

Once the planning team has been assembled and the ACTAR process explained, a specific Program Objective or objectives is established. Care must be taken to avoid establishing an objective that is so broad as to be meaningless. Likewise, an objective which is too detailed can defeat the planning outcome. We found that one to several days may be required to define a suitable Program Objective. Time spent defining this objective helps to develop a cohesive group and establishes effective communications among team members.

For example, the Objectives of the Douglas Fir Tussock Moth Program are to within 3 years:

1) implement available technology to reduce losses caused by the insect; and

2) develop and evaluate new short and long-term forest and pest management systems which will effectively prevent or suppress tussock moth infestations.

Note that a specific time frame is indicated and that the two objectives which are to be achieved within that time frame are specified. All further planning, therefore, must relate both to the time limit and the objectives indicated. This automatically eliminates many activities which would be "nice to know" but are not essential for successful completion of the program or which may require years to complete. In the planning of long-term research, an ultimate objective can be developed and one or more limited objectives indicated for a specified time period.

Activity Flow

When the Program Objective has been defined, an Activity Flow is developed. The Activity Flow consists of Phases and Subphases, and Activities arranged in a series of Arrays in an outline format.

The Phases and Subphases are intermediate objectives and are the logical subdivisions of the program's major essential components. Successful completion of each Phase and Subphase will result in the realization of the Program Objective. The identification, selection and sequential arrangement of the Phases and Subphases is carried out in accordance with research logic.

The Activity Flow for the Douglas Fir Tussock Moth Program was divided into five Phases (intermediate objectives) as follows:

1) determine the population dynamics of the insect (Population Dynamics);

2) determine those factors affecting insect outbreaks and decline (Factors Affecting Outbreaks);

3) develop control methods for suppression and regulation of the insect (Control Methods);

4) assess the socio-economic impacts of the insect on the forest and related uses and values (Socio-Economic Impacts); and

5) develop pest management systems in which the information obtained from other Phases is integrated into systems for suppressing the insect or preventing attacks (Pest Management Systems).

In some instances, each Phase was divided further into Subphases.

The development of the Phases and Subphases can be illustrated by Subphase 3.1 of Phase 3 -- Control Methods.

Phase 3. Control Methods
- Subphase 3.1 Chemical Toxicants
- 3.2 Microbials (Natural virus and Bacillus)
- 3.3 Behavioral Chemicals
- 3.4 Aerial Application Technology
- 3.5 Forest Stand Manipulation
- 3.6 Environmental Effects of Selected Control Measures

Each Subphase represents an intermediate objective which was judged essential to the development of effective and safe alternative methods for controlling the Douglas fir tussock moth.

The Phases and Subphases should be sufficiently well defined to permit determining necessary Activities. An Activity is a technical approach that contributes to achievement of the intermediate objective of the Phase and Subphase within the program and ultimately the Program Objective. An Activity provides a basis for budget development and monitoring progress of the program.

Within each Phase or Subphase of the Activity Flow, Activities are grouped into a series of four Arrays as defined below:

1. Lead Array: The main effort -- includes Activities considered most plausible for successful achievement of the Phase or Subphase. Selection of Activities for the Lead Array is based on the scientific knowledge available at the time of planning.

2. Safeguard Array: Includes Activities which are the most likely substitute technical approaches to the Activities in the Lead Array. Activities in this Array constitute the essential protection of the outcome of the Program against the inherent uncertainties of Activities in the Lead Array.

3. Optimizing Array: Those Activities which could enhance or optimize the potential of Activities in the Lead Array to achieve the intermediate objective of the Phase or Subphase.

4. Supplementary Array: Those Activities for which the probability of a positive contribution to the Phase or Subphase objective is unknown; nevertheless, their results could bring about major changes in the Lead Array. Some of these Activities may be "high risk" or "far out" applied research. Some may be long range or fundamental research. At least some of these Activities are essential to protect the Lead Array from uncertainties of outcome and to encourage unusual technical approaches.

In the Douglas Fir Tussock Moth Program, all four Arrays were not required to achieve the intermediate objectives of all Phases or Subphases. Subphase 3.1 dealing with Chemical Toxicants illustrates the arrangement of Activities into Arrays.

Subphase 3.1 Chemical Toxicants

Lead Array:

- 3.1.1 Conduct field experiments with carbaryl and trichlorfon.
- 3.1.2 Conduct pilot tests with most effective chemicals.

Safeguard Array:

- 3.1.3 Conduct preliminary laboratory and field tests of promising new chemicals.

Optimizing Array:

- 3.1.4 Develop improved formulation for candidate chemicals.
- 3.1.5 Develop equipment to provide optimum spray and droplets for candidate chemicals.

Supplementary Array:

- 3.1.6 Screen and bioassay new chemical insecticides.

In planning this Subphase, two chemical toxicants were the most promising candidates available at the time; therefore, they were placed in the Linear Array (3.1.1) for use in carefully designed field experiments to determine their efficacy and the best dosage with available formulations. From these field experiments, the most effective dosages and formulations were to be carried forward in large pilot tests (3.1.2). Several other chemical toxicants which had been through laboratory screening and bioassay but which were not ready for field experiments were also available; activities associated with these were placed in the Safeguard Array (3.1.3) for further laboratory evaluation and small scale field tests.

To increase the effectiveness of the most promising candidate chemicals, work was scheduled in the Optimizing Array to improve formulations (3.1.4) and to determine the best spray droplet size (3.1.5). This work relates to chemicals being investigated in both the Safeguard and the Linear Arrays. As improvements are made, the chemicals are scheduled for further field experiments and pilot tests.

Since new chemical toxicants could provide effective breakthroughs, laboratory screening and bioassay of new toxicants (3.1.6) were scheduled during the early stages of the program in the Supplementary Array. Effort here is confined, however, to toxicants which are registered for other insects and for which field experiments and pilot tests can be completed within the program time frame.

As a program progresses, findings in any of the Arrays may point up inadequacies in the Lead Array. Furthermore, Activities in the Safeguard, Optimizing, and Supplementary Arrays may be moved to the Lead Array, or the results of some activities may point to new approaches for inclusion in the Lead Array. In the Douglas Fir Tussock Moth Program, two of the chemicals in the Safeguard Array proved highly effective, and they were subsequently included in field experiments under the Linear Array.

Designation of essential Activities and assigning them to logical Arrays requires a high degree of scientific objectivity, technical knowledge, and research logic. Both the generalist and the planning technique specialists on the planning team play significant roles in defining essential activities, in bringing about elimination of some proposed activities, and in appropriately assigning remaining activities or generating new activities needed to meet the goals.

Activity Schedule

Once the Activity Flow is developed, an Activity Schedule is prepared. The vertical arrangement of the Activity Schedule is the Activity Flow --

Table 1. Activity Schedule for Research and Development in Chemical Toxicants (Subphase 3.1) Douglas Fir Tussock Moth Program.

Activity	FY 1974	FY 1975	FY 1976	FY 1977
3.1 CHEMICAL TOXICANTS				
Lead Array:				
3.1.1 Conduct Field Experiments with carbaryl and trichlorfon.	Field Experiments with carbaryl and trichlorfon (100)[1/].	Field experiments with improved formulations of carbaryl and trichlorfon (200).	Replicated field experiments with 2-3 most promising chemicals or formulations under various field conditions (300).	
3.1.2 Conduct pilot tests with most effective chemicals.			Pilot tests as warranted. Carbaryl and trichlorfon and 1 or 2 others -- 3.1.1 and 3.1.3 (350).	Seek registration of most effective chemicals and additional pilot tests as warranted (300).
Safeguard Array:				
3.1.3 Laboratory, airport and field tests with promising new chemicals.		Conduct airport and limited field tests with new chemicals (50).	Continue and integrate results with 3.1.1 (50).	Complete studies and recommend future direction of R&D (20).
Optimizing Array:				
3.1.4 Develop, improve and test new formulations for candidate chemicals to support 3.1.1, 3.1.2 and 3.1.3.	Conduct laboratory R&D on candidate formulations and carriers (10).	Continue laboratory and airport studies on formulations, additives, carriers, etc. (40).	Further refine formulations (30).	Complete studies and integrate with field tests (20).
3.1.5 Determine optimum on spray droplet size for formulated chemicals to support 3.1.1, 3.1.2 and 3.1.3.		Laboratory and airport tests for candidate chemical formulations (60).	Continue and integrate results with 3.1.1 and 3.1.2 (60).	Verify laboratory airport tests in field studies and integrate with 3.1.2 (60).
Supplementary Array:				
3.1.6 Screen new chemicals and bioassay most promising formulations to support studies in 3.1.3 and 3.1.4.	Preliminary screening of candidate chemicals (20).	Laboratory tests including contact, feeding and bioassay (50).	Final laboratory tests of candidate chemicals and integrate with 3.1.3 (50).	

1/ Estimated funds needed ($ thousands) to conduct work planned for the specified years.

consisting of Phases, Subphases and the Arrays of Activities. The Activity Schedule is arranged horizontally by years. Within each year, the work anticipated for an Activity is stated concisely in one or two sentences followed by the estimated costs.

Table 1 illustrates the Activity Schedule for the Chemical Toxicant Subphase of the Douglas Fir Tussock Moth Program. The numbers in parenthesis are the funds in thousands of dollars which the activity will require in the year specified. Collation of these funds provides an estimate of total costs for the activity. When costs of all activities in the program are combined, they provide the total funds required for the program or for a specific year. They also serve as a means for checking actual versus planned costs when the program is implemented.

The Activity Schedule is the basic working document of the planned program. The Activity Schedule must be reviewed critically for the relationship of each Activity to the Phase or Subphase and to all other Activities in the Program. Some of the review can be accomplished by the planning team under the leadership of the planning technique specialist. Reviews by outside knowledgeable specialists also are essential to evaluating the "claims" of the planning team, the scientific merit of the program, the probabilities of completing the Activities within the allotted time, and the likelihood of success. Only the scientists, however, can be expected to provide the technical substance of the plan.

Examination of the Activity Schedule will reveal:

1) possible imbalance in the program;
2) need to complete one Activity before beginning another;
3) excessive workloads or resource requirements in certain years;
4) unreasonable or fluctuating funding requirements; and
5) voids or weaknesses in the program structure, to name a few.

When such situations are noted, adjustments in the Activity Schedule can be made. Furthermore, by exmaination of the total estimated costs for each Activity, Phase or Subphase, it is possible to determine where too much or too little effort is being placed or where erroneous assumptions have been made.

In addition to the above information, the Activity Schedule can be used to determine needs for additional facilities, specialized equipment and scientific expertise. It also provides a basis for explicit statement of accomplishments expected in a specified time frame and for determining effects of different levels of funding on program outcomes.

Once a program is implemented, the Activity Schedule serves as a basis for monitoring progress and for adjusting emphasis as new knowledge is gained. In an active program, the Activity Schedule is revised almost continuously as opportunities arise to exploit technological breakthroughs. It also provides a system for developing more detailed problem analysis and individual study plans for each Activity.

As developed by the U.S. Department of Agriculture, ACTAR is a technique for planning large-scale, biological research and development programs whereby essential research activities can be identified and the required funds, facilities, equipment, and skills organized and managed to converge on specific objectives.

ORGANIZATION, MANAGEMENT, AND COORDINATION

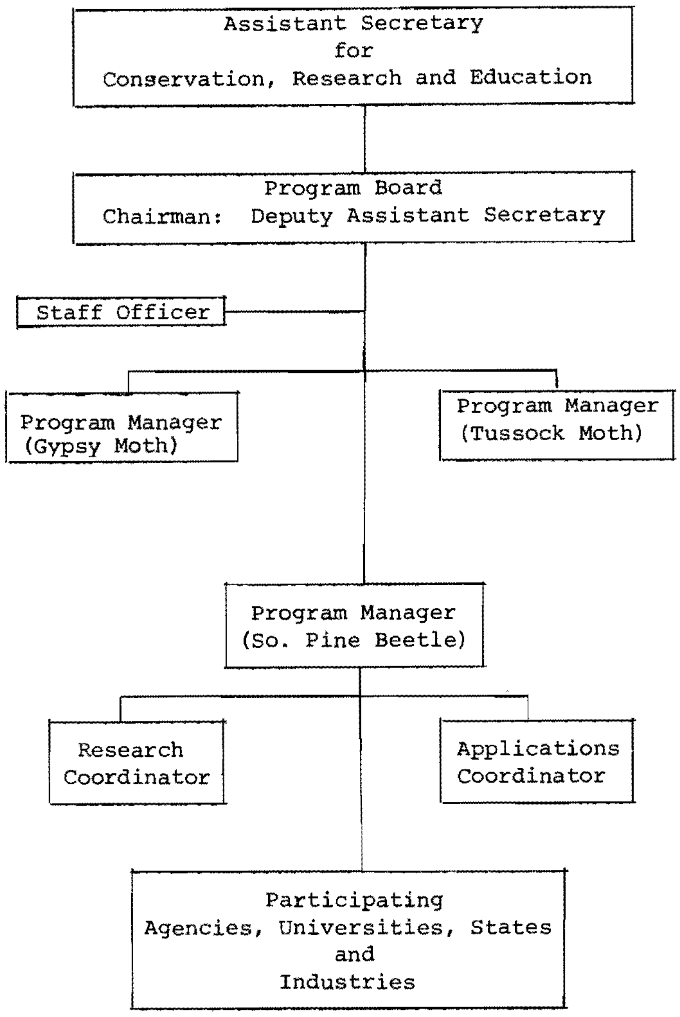
Implementation of the Combined Forest Pest R&D Program called for new approaches to organization and managment. ACTAR provided a basis for appraising the most effective way to organize program administration. The involvement of four Federal agencies, several State Agricultural Experiment Stations, universities and colleges, and other State organizations posed a complex task. An approach was needed which would maintain effective control of the program, but which would not disrupt the organization and administration of the participating units.

The organization for the Combined Forest Pest R&D Program is illustrated in Figure 1. Administration is centered in the Office of the Secretary, U.S. Department of Agriculture, but with delegation of responsibility to the lowest practical level (Program Manager).

A <u>Program Manager</u> is responsible and accountable for each of the three insect programs. The <u>Staff officer</u> provides staff support, coordinates activities among the three programs, and provides liaison among the participating Federal, State and private organizations. The three Program Managers and the Staff Officer are employees of the Office of the Secretary, USDA, and report directly to the Deputy Assistant Secretary for Conservation, Research and Education.

Each Program Manager is located in the field and is assisted by a <u>Research Coordinator</u>, who coordinates all research activities, and an <u>Applications Coordinator</u>, responsible for the

Figure 1. Organization, Combined Forest Pest R&D Program, USDA.



development aspects of the program and for liaison with potential user groups. The Research and Applications Coordinators report to the Program Manager, but retain their affiliation with their parent organization.

The Program Board (chaired by the Deputy Assistant Secretary and consisting of the administrative heads of the four participating Federal agencies and representatives of State forestry schools and organizations, scientific and professional societies, and other interested groups) provides national overview and guidance to the program.

The Program Manager develops an Annual Plan of Work and Budget based on the overall program plan prepared by the ACTAR planning technique. This Annual Plan of Work and Budget is reviewed by the Program Board, which determines progress toward program objectives and recommends any needed adjustments to the Assistant Secretary for Conservation, Research and Education for his approval.

The approved Annual Plan of Work and Budget is the guiding document for each insect program during the forthcoming year. Within broad guidelines, the Program Manager can make adjustments to meet new situations in the field or exploit unforeseen breakthroughs. Within the guidelines of the Annual Plan of Work and Budget, the Program Manager is responsible for soliciting, selecting, and funding proposals for research and development. The Program Manager is assisted in this task by an ad hoc Peer Review Panel of specialists who review research proposals for their scientific merit and their potential contribution to the objectives of the Program.

Once a research proposal related to an Activity is approved by the Program Manager and funded by one or the other of the participating Federal agencies, the Program Manager and the Research and Applications Coordinators continue to monitor progress and evaluate accomplishments. Frequent visits to the field by the Staff Officer also are designed to monitor overall progress, review management effectiveness, improve inter- and intra-program coordination, and assess implementation of research findings with potential user groups.

The organization and management scheme developed for this complex research and development program is working well. Adjustments are continually being made to facilitate coordination, reduce administrative procedures, improve overall management effectiveness and develop effective methods for monitoring progress without unduly constraining scientific creativity.

CONCLUSIONS

ACTAR provides an effective means for planning large and complex biological research and development programs. Because the program is planned by scientists, it is readily accepted and understood by those who must conduct the research. Administrators, budget analysts, and others concerned with allocation of scarce resources also readily comprehend ACTAR planning, although they may have little or no knowledge of the technical subject matter.

In developing ACTAR for use in the Department of Agriculture, certain distinguishing features were noted.

ACTAR can provide:

1. A clearly stated, well-defined objective for a program.

2. A logically ordered plan to reach that objective.

3. A flexibility in the plan which allows for the inherent uncertainties of research.

4. A chronological scheduling of the plan.

5. An identification of the most promising direct leads to reach that objective.

6. An identification of the research that will provide essential protection to the plan.

7. An identification of the research which will optimize attainment of objectives.

8. An identification of and basis for evaluating long-term or high-risk research.

9. A basis for screening proposed research activities within a program.

10. A basis for developing budget estimates and allocating resources within the program.

11. A basis for full participation of scientists in program planning.

12. A listing of anticipated accomplishments.

13. A basis for appraising the most effective way to organize program management.

14. A basis for monitoring and evaluating program progress.

ACTAR will not provide:

1. An evaluation of the proposed program as compared to other programs. However, it can provide some of the information needed for such an evaluation.

2. A substitute for careful, objective analysis. In fact, it requires such analysis to be successful.

3. A complete safeguard against exaggerated claims of research accomplishment. In fact, the technique can be exploited unless a process of rigorous questioning of the plans is carried out by the original planners and then by persons not in the organized group.

4. An automatic assurance of increased funding. In fact, ACTAR may be of greatest use in adjusting existing allocations within a program.

5. An automatic perpetuation and justification of on-going research. In fact, successful use of ACTAR requires that on-going research be ignored until technical plans have been completed. Only then is the on-going research evaluated to determine its pertinence to the program objective and the extent to which it meets program needs.

Although we are highly enthused about ACTAR as a means for planning large research and development projects, we emphasize that not all research programs are adaptable to the ACTAR planning process. Basically, ACTAR as developed in the U.S. Department of Agriculture is one method whereby complex biological research and development programs can be planned, organized, and implemented in a logical, systematic way.

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Solutions to Environmental and Economic Problems (STEEP)*

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This article describes one model for organizing and mobilizing scientific resources to address the highly complex and costly problem of soil erosion in the Pacific Northwest. With a U.S. Department of Agriculture grant to the agricultural experiment stations in Washington, Oregon, and Idaho, as well as supplementary state and federal funds, most of the research projects require collaboration across disciplines and, in some instances, across state boundaries. After 6 years of effort the results obtained with STEEP indicate that the model might be applicable to other regions and problems.

STEEP (Solutions to Environmental and Economic Problems) is a multidisciplinary research effort to develop new techniques and strategies to control soil erosion on the croplands of Washington, Oregon, and Idaho (Fig. 1). The STEEP concept is that erosion control requires major modifications in tillage practices, the development of new crop varieties, and different methods of weed, insect, disease, and rodent control -- all of which must be acceptable to farmers.

The innovators of STEEP were the wheat producer organizations in the Pacific Northwest. Producer groups arranged the initial discussions and obtained the supplemental congressional funding, and they continue to support and monitor STEEP's progress. Funds for STEEP research have been made available each year since 1976 by a special U.S. Department of Agriculture (USDA) grant to the agricultural experiment stations at the universities of Washington, Oregon, and Idaho, and by appropriations to the USDA-Agricultural Research Service (ARS).

The high rate of erosion in the croplands of the Pacific Northwest is caused by a combination of factors, including heavy winter rainfall, unusually steep topography, and a prevalence of winter wheat cropping with conventional management. In spite of decades of conservation efforts by concerned farmers, absentee landowners, and various government agencies, the productive capacity of relatively large areas of land is continually reduced because of topsoil losses, and because of environmental damage to land and water resources. The annual rate of soil erosion in the Columbia River drainage system is estimated at 110 million tons, of which approximately 30 million tons are deposited in Northwest streams, rivers, lakes, and harbors. The silt accumulations in reservoirs is shortening the life of critically needed hydroelectric generating facilities, reducing the capacity to store irrigation water, and impairing the water quality of rivers and reservoirs. Removal of silt from roadsides and ditches in eastern Washington, Oregon and western Idaho costs several millions of dollars each year.

Conservation tillage, which includes no-till and reduced tillage practices, is generally recognized as an effective method for reducing erosion and has been the subject of considerable research over the years. The value of conservation

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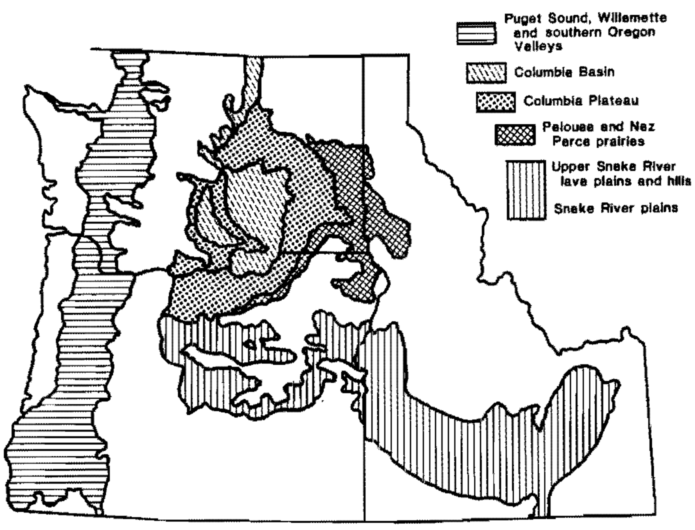
Figure 1. The Major Land Resource Areas in Washington, Oregon, and Idaho



tillage to farmers, however, has varied widely from area to area. Tillage practices that work in one area often do not work in another area because of differences in environmental characteristics. For example, seasonal precipitation, weeds, insects, and diseases frequently vary and can produce effects that override the benefits of conservation tillage.

CHARACTERISTICS OF THE STEEP APPROACH

STEEP represents a mode of research organization and implementation that differs from either the Cooperative State Research Service (CSRS) or competitive grant models of the USDA that guide national research efforts on agricultural problems. THE CSRS distributes funds to land grant universities on the basis of a formula that was established by Congress. These federal funds are combined with state dollars to support programs of agricultural research of state, regional and national interest.

Competitive grants are used by several federal agencies to meet specific program objectives. They are allocated for fixed periods of time to investigators whose proposals are reviewed and scored by peer panels as a basis for recommending whether or not the proposals get funded (Krogman and Key, 1981). Competitive grants complement and supplement the formula funded research system and assist other scientists whose main responsibility may be to educate undergraduate and graduate college students. However, the competitive grants program has been criticized for several reasons. One is that national agricultural problems that demand the continuity of long-term research programs involving several disciplines are regularly threatened by the vicissitudes of the peer review system. Another reason is that scientists who would otherwise use long-term approaches to the study of problems are often forced to develop truncated research designs without the support of needed expertise from other disciplines.

Neither the competitive grant nor the formula fund model are effective in stimulating multidisciplinary research across state boundaries with the aim of solving regional or national agricultural problems. More often, they bring together members of a single discipline for research projects with quite modest objectives that can be accomplished through a rather loose form of coordination as opposed to a concerted team effort. Also, because most of the funds for agricultural experiment stations are provided by the legislatures of individual states, regional research is often given a low priority.

STEEP is organized on the basis of intermediate-term (15-year) special grant incentives. This has made it possible to bring together the efforts of scientists from at least ten disciplines in an attempt to find a solution to the soil erosion problems in the Pacific Northwest. Such a program could, presumably, be used to solve other agricultural and environmental problems.

The keystone of the STEEP model is a special USDA grant to the Washington, Idaho and Oregon experiment stations to supplement the direct appropriations to ARS for STEEP-oriented erosion research. The funding has been modest for an effort of this magnitude: a special grant of $250,000 was made to the three state experiment stations when the project was first approved in 1976, and this was raised gradually to $648,000 in 1982; a total of $435,000 has been provided annually to ARS for STEEP since 1976. By agreement of the three experiment stations, the special grant funds are divided equally among the three states.

The specific research projects under STEEP are organized into five problem areas:

1) tillage and plant management;
2) plant design;
3) erosion and runoff predictions;
4) pest management; and
5) the socioeconomics of erosion control.

Nonfederal scientists from each state experiment station are encouraged to prepare proposals for research in these areas and compete for funds. The proposals are reviewed and arranged in order of priority by a coordinating committee composed of five participating scientists, with the final decision on funding being made by the experiment station directors. Priority is assigned on the basis of criteria as such relevance to objectives, and probability of success. This procedure has resulted in some 35 scientists from ten disciplines gaining financial support for 30 individual projects. The disciplines include soil science, agronomy, plant breeding, agricultural engineering, weed science, plant pathology, entomology, biology, agricultural economics, and rural sociology. Most of these projects have involved collaboration across disciplines and in some cases, across state boundaries.

The USDA-ARS appropriated funds are also directed to research within the five problem areas. Again, guidance is provided by the STEEP coordinating committee. Funds are allocated to general areas where research strengths lie and where contributions to the overall objectives are expected to be the greatest. STEEP has served to direct the research of about 20 federal scientists. Research outlines for specific problem areas are prepared and updated or revised and approved by staff specialists and administrators. Priority for federal research is assigned on the basis of the same criteria as for the state projects as well as relevance to national research programs. The ARS funds are redirected as needed to achieve the program goals. The ARS provides added expertise to STEEP through its national and regional research programs.

Funded scientists and their administrators meet annually for 2 to 3 days to discuss their research results and review research needs. Representatives of the wheat-producer organizations, cooperative extension, and regional conservation and environmental agencies also attend these meetings, which take place before the ensuing year's funds are allocated. The STEEP coordinating committee is responsible for oganizing the review session and assembling an annual report of the year's research activities.

Procedures set in motion by the STEEP program operate in subtle but effective ways to redirect research efforts within the agricultural experiment stations. The normal method of allocating research funds is by discipline, whereby department heads interact with research administrators to approve scientists' projects and budgets for those projects. Under this method, the concerns of a single discipline and the specific departmental goals are the major factors determining the allocation of the research funds. The STEEP model, in contrast, uses research objectives deemed important to solving a particular problem, in this case, erosion control, that transcends departmental boundaries.

The research budgets for agricultural experiment stations are spent mostly on fixed costs, scientists' salaries, technicians, and secretarial support. By providing funds for specific research activities (few of the STEEP funds are used for scientists' salaries), STEEP tends to draw the base resources of departments toward efforts that the stations and the coordinating committee have defined as high priority. The same is true of ARS research. For example, ARS weed science research at Pullman, Washington, although not funded by STEEP has redirected a substantial part of its program to weed control in conservation tillage systems as a result of the STEEP program. The overall result is that total funds spent on STEEP research are at least triple the amount that is actually allocated. Finally, the annual reporting and interaction requirements mandated by STEEP ensure that research under the program is not placed on the back burner and ignored by scientists with multiple projects.

Some of the types of research and technology development that have been initiated by STEEP are outlined below.

Tillage and Plant Management

Considerable effort has been devoted since the onset of STEEP to research on new and improved agronomic methods for erosion control. Several approaches have been used, but most in some way emphasize the development of no-tillage and reduced-tillage technology for the production of cereals and grain legumes. Reduced tillage planting, which usually involves no more than one or two cultivations for seedbed preparation (compared with four to seven for most conventional planting systems), and no-tillage, where the crop seeds are sowed directly on the land without any prior seedbed preparation, are both highly effective for soil erosion control. For example, an annual soil loss of 56 metric tons per hectare (25 tons per acre) or more, which is common on many conventionally cropped fields of the Palouse region, can be lowered to 11 metric tons per hectare or less with reduced tillage or no-tillage methods (USDA, 1979). This level of control, according to most workers, is sufficient to maintain the long-term soil productivity and minimize the adverse environmental effects of soil erosion. However, crop yields with these conservation tillage systems are often less than with conventional tillage planting. The reasons for this are not fully understood, although crop growth under the reduced tillage and no-till methods may be retarded because the residues of the previous crop, which may range in amount from 4 to 11 metric tons per hectare, are conserved on the soil surface and the seedbed soil is either more cloddy or harder.

The subsequent crop must take root and grow in the rough seedbed and in the presence of slowly decomposing surface cover.

STEEP research shows that crop residues left on the soil surface benefit overwinter storage of precipitation. For example, compared with clean fall tillage, overwinter water storage in intermediate precipitation areas was increased by 20 percent when the crop stubble was left undisturbed (Ramig, Rasmussen, Rickman, and Kraft, 1980). In a higher precipitation area (35 to 45 centimeters annually), surface residues increased water storage by about one-third on slopes and ridgetop positions of the field where runoff generally occurs (Cochran, Elliott, Papendick, 1979). However, the additional water saved with surface residues often does not result in higher crop yields. This may be because phytotoxins are produced in the slowly decomposing surface stubble which, upon leaching into the soil, inhibit seed germination or cause root and shoot injury to newly established wheat seedlings. Studies of wheat straw extracts showed the presence of short fatty acids that may account for the frequently observed temporary stunting of winter wheat planted under the no-tillage system (Cochran, Bikfasy, Elliott, Papendick, 1980). Other unidentified toxins are probably responsible for the more severe and permanent type of injury that often occurs in crops planted over heavy residues.

Physically removing the crop residues away from the seed row was shown to improve seedling establishment and early growth and to increase crop yields where phytotoxicity was a problem (Cochran, Elliott, Papendick, 1979). Such treatment also appears to alleviate the abnormal "high crown set" of wheat which often occurs with direct sowing into heavy crop residues. A high crown set impairs development of secondary roots and subjects the young plants to additional environmental stress caused by inadequate water and exposure to herbicides applied for weed control. Consequently, engineers have designed no-till planting equipment that removes straw and chaff from the seed row through use of specially designed drill openers (Wilkins, 1979).

Another related approach for crop residue management in dryland areas is the concept of strip-till-plant (Bolton, 1980). Wheat is planted in narrow tilled strips (10 centimeters, about 0.5 meter apart) with the interrow area left untilled. This once-over planting technique incorporates the crop residues in the seed row, reduces tillage energy requirements compared with conventional planting, conserves seed zone moisture, and provides good erosion control.

A novel engineering approach for crop residue management developed from STEEP research is the slot-mulch concept (Saxton, McCool, Papendick, 1982). This technique has the potential for reducing runoff and erosion from frozen soils and for enhancing the feasibility of no-till planting in cereal stubble fields. Crop residue is compacted into a narrow continuous slot, approximately 5 to 10 centimeters wide by 25 to 30 centimeters deep, formed approximately on the field slope contour every 10 to 16 meters. The residues of straw and chaff are left well exposed above the soil surface. Field trials and theoretical analysis show that during runoff, water will flow into the slot and readily move downward through the residue into the soil profile.

Studies of crop sequence effects show that the highest overall yields of winter wheat with no-till are obtained where the wheat follows a low-residue crop such as peas or lentils. This procedure is now becoming a common commercial practice in eastern Washington. Crop yields equal or sometimes exceed those obtained with conventional tillage planting. Yields are often much lower when the wheat is planted without tillage following a cereal crop, unless the cereal crop was sparse or the residues were removed by harvesting or burning.

No-till management offers possibilities for more intensive cropping in the low to intermediate precipitation areas (35 to 46 centimeters average annual precipitation) where alternate wheat and fallow is the more traditional practice. One scheme, which is under investigation through the joint efforts of agronomists and weed and soil scientists, is to control weeds chemically and allow the stubble of the previous crop to stand overwinter. In most years, overwinter water storage is greater than when fall cultivation is used. Instead of the usual fallow cultivation, a crop of barley or spring wheat is planted without tilling, as early as conditions permit, thus minimizing soil erosion and water loss.

A significant technological advance resulting from STEEP research is the no-till methods of planting cereal crops in killed grass sod (Elliott and Papendick, 1979). A considerable area of bluegrass for seed production (approximately 80 percent of the nation's seed supply) is grown in northern areas of the Palouse region on steep hills. Soils in sod are well protected and there is virtually no erosion. When the sod is removed by the usual practice of plowing and cultivation for seedbed preparation, the erosion hazard is increased. Studies showed that the registered herbicide glyphosate [N-(phosphonomethyl)glycine], when applied at an economical rate in early spring, would kill the bluegrass. Wheat or barley that is then planted by the no-till method produces yields equivalent to conventionally planted crops. This first crop can then be followed by winter wheat, a grain legume, or another spring crop and a rotation established that continues no-till planting on the undisturbed sod. The protection against erosion that is provided by the sod remains highly effective for several years.

The design of planting equipment has received considerable emphasis in the STEEP program. Several versions of no-till or reduced tillage planters for one-pass operations were developed and tested by researchers in all three states, with varying degrees of success. A chisel-type planter resulted in yields of winter wheat on commercial fields that were 107 percent of conventionally seeded yields and reduced soil erosion by 84 percent compared with conventional planting (Peterson, 1980). A planter with a hoe-type opener was designed to operate in heavy residues and provide for the precise vertical separation of seed and fertilizer, the fertilizer being placed below the seed to minimize damage to seedling roots from fertilizer burn (Wilkins, 1979). Another type of drill designed in the early years of STEEP is now in limited commercial use in the Palouse region. It appears that several different drill designs will be necessary for no-till planting because of the diverse soil, slope, crop residue, and moisture conditions that occur across the wheat region.

Plant Design

Prior to STEEP little was known about the breeding of wheat varieties adapted to reduced- and no-tillage management systems. Breeding efforts had focused exclusively on clean tillage and traditional wheat-fallow culture. Since 1977, extensive studies of wheat genotypes under different cultural practices have been conducted to select wheats specifically adapted to reduced- and no-tillage management. The genotypes have been grown on various residues, with the use of split plots maintained under conventional and conservation tillage. Under contrasting tillage treatments, wheat genotypes responded differently for traits such as yield and weight in about 35 percent of the tests. These results indicate that certain genotypes have several traits that are particularly advantageous for growth under conservation tillage. These include deep crown placement, early spring recovery, prolific tillering, early maturity, strong straw, and consistently high seed weights. No features have been identified that would limit the breeding of wheats adapted to conservation tillage.

A primary objective of Vogel and co-workers (1956) in their pioneering work on semidwarf wheat varieties grown in the Pacific Northwest was to breed strains that could efficiently use high levels of inorganic fertilizer, withstand lodging, and yet be sown early enough to provide maximum vegetative cover for erosion control. Early seeding has long been recognized as a practical way to prevent erosion of fall-sown wheat under summer fallow culture. However, there are several problems associated with early seeding, such as weeds, diseases, insects, poor stand establishment, excessive plant growth, and cold and water stress.

Results obtained through the STEEP project have shown that several of the barriers to early seeding can be removed. In particular, it is possible to breed for resistance to the diseases of early seeding, such as certain rusts and strawbreaker foot rot. By using several approaches, breeders have obtained wheats with stable resistance to stripe rust. Most varieties now recommended for early seeding have genes for non-race-specific and nonspecific resistance to the fungus. In another breeding strategy, similar wheat lines that have different genes for resistance are being blended. The mixture of divergent, genetic forms of resistance almost precludes the possibility of a new stripe rust biotype causing serious damage to early-sown wheat. Although progress has been slower, similar approaches are being used in the development of wheats resistant to leaf rust.

Achieving adequate cold hardiness in early-sown wheat came about through STEEP-sponsored research. Most wheats are particularly vulnerable to freezing temperatures and deacclimate readily after the five- to eight-tiller stage. After several cycles of breeding and selection, a few wheat lines are identified with the needed balance among crown placement, tiller number, heading date, and yield potential that could withstand unusually cold temperatures of moderate duraduration. Daws, a recently released variety, forms a deep crown and is the most cold hardy of currently grown soft white winter wheat varieties.

Recent research under the STEEP program has produced results that may explain why winter wheat seedlings develop poorly under conservation tillage. Roots of wheat plants grown under reduced tillage were found to be colonized by a species of bacteria that markedly inhibits plant growth. Bioassays with these microbes indicated less than others, suggesting that wheat varieties tolerant of these inhibiting bacteria can be developed.

Thus far, results show that wheat varieties that do well under conventional tillage do relatively well under conservation tillage. However, all currently tested germplasm has been developed under conventional tillage systems, and experiments are now under way to determine whether segregating wheat populations derived from early varieties grown exclusively under conservation tillage systems will yield progeny with features that are specially adaptive to conservation tillage.

Disease Management

Winter wheat in the Pacific Northwest probably suffers from more diseases than wheat in any other region of the same size in the world. Winter wheat is grown at elevations ranging from near sea level in the Puget Sound-Willamette Valley regions of western Oregon and Washington to 2100 meters above sea level near the Teton Mountains in eastern Idaho. Temperature and moisture vary greatly with elevation, and these influence disease development.

Changing technology, especially the extension of irrigation and efforts to reduce tillage, have accentuated the demands on research in plant pathology. Researchers face the disease problems of historic, conventional farming plus those of the experimental systems just reported. Therefore, the STEEP research has included studies of disease management in new and changing systems.

Stand establishment is often a severe problem in crops grown under the no-tillage system, especially when crop refuse is heavy. *Pythium ultimum* and *P. aristosporum* fungi attack under cool, wet conditions (Cook, Sitton, Waldher, 1980), and decomposing fragments of straw in the drill row stimulate the development of these pathogens. Toxins that are leached from masses of rotting straw (Elliott, Miller and Richards, 1979; Wallace and Elliott, 1979) weaken seedlings directly, predisposing them to attack by *Pythium*. Investigators found that Ridimil [N-(2,6-dimethylphenyl)-N-methoxyacetyl)alanine methyl ester], when used as a seed treatment or as granules in the drill row, reduces or controls the stand establishment problem caused by *Pythium*.

The most widespread, chronic soil-borne disease in the region is strawbreaker foot rot, caused by *Pseudocercosporella herpotrichoides*. This fungus rots the base of the wheat stem and survives from year to year on infested stubble. Crop infestation occurs in late fall and early spring. STEEP-sponsored research advanced the use of benomyl (Benlate) [methyl 1-(butycarbamoyl)-2-benzimidazolecarbamate] as a stopgap means of reducing losses in early-seeded fields. In Washington alone, over 80,900 hectares were sprayed with the chemical during 1980 and 1981, with considerable increases in yields.

Wheat breeders in all three states are working cooperatively to incoporate the best known foot-rot resistances into adapted wheat varieties. Investigators in Washington are concentrating on more efficient means of testing materials for resistance. Studies in Oregon on tillage management show that burning the crop residues does not materially reduce foot rot. Oregon State and ARS (Cook and Waldher, 1977) researchers have established that, compared to use of the moldboard plow, surface mulch does not significantly increase foot rot.

Studies in Idaho show that use of the no-tillage method increases stripe, which is caused by *Cephalosporium gramineum*. This fungus enters injured roots and infects wheat stems. In cool wet weather, the fungus sporulates on the straw left on the soil surface and water washes the spores into the soil. STEEP researchers are now attempting to breed wheat varieties that will resist the disease and to develop chemical control methods.

Seeding directly into undisturbed stubble increases "take-all", which is caused by the fungus *Gaeumannomyces graminis* var. *tritici*. This fungus persists on the roots and in the stubble of harvested crops, and is more severe if host remains are unbroken. With conventional tillage the wheat residues are fragmented and the fungus is less effective in initiating the disease. Both take-all and *Pythium* root infections are favored by weakened plants. Proper nutrition of crops planted without tillage is a critical factor in preventing spread of the disease, and this requires the application of fertilizers near the seed, a problem that in turn requires advances in machinery and knowledge.

Bacteria antagonistic to the take-all pathogen and adapted to life on the surface of wheat roots have been applied to wheat seeds prior to planting. The bacteria colonize the surfaces of emerging roots and grow with the root as it ramifies the soil. Take-all lesions are reduced to minute, nondamaging size and the disease is controlled. This method has promise for commercial adoption. The bacteria are strains of *Pseudomonas fluorescens*.

Fusarium avenaceum is another pathogen that has increased in importance with the use of conservation tillage. This fungus causes severe losses in lentils (*Lens culinaris* Medic.) that are seeded directly into sod of Kentucky bluegrass killed by herbicide (Lin and Cook, 1977).

The most destructive airborne diseases of the region are stripe and leaf rust, caused by *Puccinia striiformis* and *P. recondita*, respectively. Serious losses due to these rusts occurred in the 1980-81 season. Both of these rusts have increased as a result of irrigation. Precipitation in July and August is normally so low that little green, herbaceous foliage existed in the region prior to irrigation. The rusts, as well as the cereal aphids that transmit barley yellow dwarf virus, require green leaves to survive over the summer in quantity, and these are now available.

The number of races of both *P. striiformis* and *P. recondita* is increasing rapidly (Line, 1980; Milus and Line, 1980) and resistance to specific races is becoming less reliable. Means of detecting nonspecific resistance, or resistance effective against many races, are being developed.

Epidemiological studies of stripe rust have shown that on the basis of the cumulative degree days during winter and early spring it is possible to predict outbreaks of stripe rust well in advance. Warm winters and cool springs made for severe losses in susceptible varieties (Cookley and Line, 1981). The winter of 1980-1981 was mild and the spring was exceptionally cool and wet. Heavy losses were prevented by timely application of Bayleton (triadimefon) (Line and Rakotondradona, 1980).

The barley yellow dwarf virus causes serious losses in wheat, oats, and barley, and the predominant strains are transmitted by the bird cherry-oat aphid, the greenbug, and the English grain aphid. All corn varieties and hybrids extensively grown in Washington are hosts of barley yellow dwarf virus. The bird cherry-oat aphid reached 1000 individuals per ear of corn in August 1979, and winged adults migrated from the corn to the early-seeded winter wheats. Yellow dwarf losses have increased dramatically in recent years, probably as a result of increased corn production under irrigation. In 1981, spring barley yields were reduced by one-third to one-half in widespread areas by yellow dwarf. No resistant, adapted cereals are available in Washington.

Other workers have discovered that herbicides sometimes alter disease relationships. The expanded use of chemicals to control weeds on untilled fallow ground may result in herbicide-disease interactions of which we have no knowledge.

Research into disease management has provided challenges for other areas of STEEP research. No-till drills are needed for commercial use that will move the straw out of the seed furrow. Fertilizers must be properly placed to give the seedling greater vigor. Crops with increased resistance to several diseases are required so that reliance on chemical controls can be minimized. Methods for bacterial control of take-all must be perfected, and similar efforts must be made to control other diseases. At the same time, diverse breeding programs and germplasm resources must be maintained to insure against unpredicted outbreaks of disease.

Weed Management

The development of conservation tillage systems under the STEEP program required new research on combinations of chemicals and tillage for weed control. Reduced tillage, trashy fallow, and no-till encourage the establishment of wild oat (*Avena fatua*), downy brome (*Bromus tectorum*), and broadleaf weeds in the crop seeded after the fallow season. For the benefits of substituting chemicals for tillage during fallow to be preserved, the use of selective grass and broadleaf herbicides is required.

The chemical control of weeds during fallow can be highly beneficial for water conservation. Tests in Oregon during 1977 to 1981 showed that an additional 0.5 to 2.54 centimeters of moisture was stored in such fields compared to mechanically tilled fallow fields. The resulting extra yield was complemented by a reduction of two tillage operations with a combined net benefit of $62 per hectare. Similar results have been reported in Washington and Idaho.

The herbicides used for chemical fallow in the noncrop phase of each season include cyanazine, atrazine, metribuzin, dalapon, propham, paraquat, and glyphosate. These herbicides are also used for reduced tillage cultures. Herbicides for selective downy brome control in wheat include trifluralin and metribuzin. Metribuzin is a postemergence herbicide that is effective on both grass and broadleaf weeds, and was registered for crop use for the first time in 1979.

Weedy grasses such as downy brome are difficult to control with the use of stubble mulch fallow, sweep fallow, trashy fallow, and no-till practices, and sometimes with conventional tillage. Successful programs for downy brome control in winter cereals could increase yields by 35 percent on average in the Pacific Northwest. Chemical herbicides are not only essential for selective weed control in the crop but also are vital for nonselective control in the fallow period.

Tillage costs increased by 9 percent in 1978, 14 percent in 1979, 15 percent in 1980, and 16 percent in 1981. Mechanical tillage costs for weed control have escalated at a much faster rate than herbicide costs. The concept of reduced tillage by the use of chemical fallow and selective weed control in the crop can be of great economic value at a time when conservation practices are essential, and when energy and production costs are increasing.

Socioeconomic Considerations

Unless farmers adopt some of the soil conserving methods that are developed, the STEEP program will not have accomplished its objectives. Rural sociology and economic components were added to STEEP to contribute an understanding of the acceptance and rejection of soil-saving innovations. Recent evidence suggested that the adoption process for environmentally related technology might differ from the traditional pattern observed in the Midwest for technology primarily aimed at improving the farmer's economic well-being (Pampel and Vanes, 1977).

STEEP agricultural economists have studied both the short- and long-term effects of using erosion control practices, and the effects of improved technology on short- and long-term scenarios. The question farmers consider is the trade-off between short-term costs of erosion control practices, measured largely by reduced yields, and long-term benefits, measured by maintaining productivity over time.

Studies of the economics of conservation practices (Harker and Michalson, 1978; Berglund and Michalson, 1981) revealed that the short-term (annual) cost of conservation is approximately $10 per acre. The annual reduction in soil loss with conservation practices varied from 5 to 8 tons per acre. Analyses of long-term costs related to the use or non-use of conservation practices (Walter, 1980) indicated that over a 75-year period, farmers would receive an additional $8.50 (present value) per acre annually for a wheat-pea rotation with the use of conservation tillage. In another study (Taylor and Young, 1981) it was concluded that the long-term effects of soil erosion and technological progress need to be evaluated together when estimates of future agricultural productivity are being made. The findings indicated that until there are technical substitutes for energy intensive inputs, rates of production will decline.

The rural sociological component of STEEP gave highest priority to conducting an immediate bench-mark survey of current erosion control practices and perceived barriers to greater usage (Carlson, Dillman, and Lassey, 1981). This survey was conducted in 1976 in a natural geographical area of exceptionally high erosion potential straddling the Washington-Idaho border. Among the results of this survey was that farmers frequently cite absentee landlords as a major reason for their reluctance to make greater efforts to control soil erosion. The same farmers were surveyed again in 1980, at which time their detailed perceptions of the landowner-farmer relationships were determined. The results showed that absentee landlords were not a significant factor in inhibiting the use of erosion control practices (Dillman and Carlson, 1982).

The 1980 survey was conducted jointly with agricultural economists who, over the course of the STEEP program, had developed their own decision-making models for the use of minimum tillage practices. The availability of the 1976 bench-mark survey (Carlson, Dillman and Lassey, 1981) provided an invaluable set of background variables for testing the model. The interaction between the rural sociologists and agricultural economists caused the two groups to examine each other's models and influenced the questions they asked in the joint survey.

Data were thus collected on the ways in which farmers changed their soil erosion control practices between 1976 and 1980, the effects of absentee landowners, and the role of risk aversion in decision-making. On the basis of these data, scientists are now developing models for encouraging the adoption of new soil conserving practices developed by the STEEP program. The first step of diffusion has already been realized. The surprising discovery (Dillman and Carlson, 1982) that absentee landowners are primarily females of retirement age who do not participate in farm decisions has led to the realization that farm operators themselves must make the necessary improvements. This finding has provided the basis for nearly 20 extension education programs for soil conservation service personnel and farmers.

CONCLUSIONS

The STEEP research program has directed the work of 55 scientists in the ARS and the agricultural experiment stations of Washington, Oregon, and Idaho toward a solution of the erosion problem in the Pacific Northwest. The success of the effort, now 6 years old, must ultimately be judged on the basis of whether soil erosion in this part of the United States is brought under control within the next 5 to 10 years, the expected life of the STEEP program.

The USDA special grant to the three universities and an appropriation to the ARS which, since 1976, has provided annual funding for the research, has resulted in long-term multidisciplinary research across state boundaries. Results from the STEEP program have shown that significant reduction in soil loss occurs when crops are grown without tillage or with minimum tillage and have identified some of the factors responsible for the decreased yields that are associated with these practices.

As a result of the STEEP program, some scientific resources that were previously devoted to other concerns have been mobilized toward the higher priority research problem, soil erosion. This mobilization was accomplished, in part, by supplementing the traditional cooperative research model with special, intermediate-term grants.

Experiences with STEEP suggest that this model may be useful for research aimed at the solution of other important regional and national agricultural problems and perhaps problems in other sectors. However, a proliferation of STEEP-type programs might result in the diversion of scientific effort away from other priority problems, and the model should therefore be subjected to additional testing both within and outside the agricultural sector.

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Regional Coordination of Scientists' Initiatives in Interdisciplinary Research

John M. Barnes

Scientists specializing in corn virology, genetics/breeding, agronomy and entomology have collaborated on an interregional approach to corn virus research. This paper reports the multidisciplinary and interdisciplinary stages of the research, as the problems were identified, clarified, researched, further clarified and further researched. Communication and coordination between practitioners and scientists, between scientists and administrators and among scientists made these accomplishments possible.

In the early 1960's, a disorder of corn plantings in the northcentral and southern corn producing states of the U.S. caused widespread concern. The overall nature of the problem and the seriousness of the concerns were brought to the attention of researchers through a complex, highly interactive network of communication -- involving farmers, extension specialists, federal and state land-grant university scientists and industry experts. The important position of corn as a domestic and international commodity had, in years before this problem, made this crop one of continuing, thorough study. This research base provided a foundation upon which new research initiatives addressing these disorders could build.

One key ingredient in scientific work is the observation of phenomena. Observations by the farmers, extension specialists, federal and state scientists, and industry experts in this communication network led to the realization that a critical disease was occurring in corn fields, and that it more than likely was caused by a virus or virus-like agent that was carried by flying insects. Word was passed at farm field days, extension meetings, workshops, informal meetings of research and extension specialists at the state university or in the field, at regional or national meetings of scientific societies, and through farm news media (radio, television, newspapers), and a few research efforts were initiated by individual scientists.

The earliest research efforts on these disorders were conducted primarily within the individual disciplines of plant pathology and virology, genetics/plant breeding, agronomy, and entomology. These preliminary assessments took place in an environment of unstructured inquiry. Such inquiry has an openness that invites both scientists and non-scientists, as well as experts from different fields of science, to come together in search of explanations for particular phenomena. This is the spirit of interdisciplinary dialogue, and this openness is the catalyst for interdisciplinary research. During subsequent stages of assessing the situation and identifying the problems, the complex chains of events led observers to the conclusion that there were no simple explanations.

In order to maximize diagnostic efforts and solidify problem-solving approaches, scientists who worked on various aspects of corn production and protection recognized the need for working together to gather information. They communicated this to the department chairpersons of their respective disciplines. Such discussions

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at individual state land-grant universities led to planning and research priority-setting efforts with directors of the State Agricultural Experiment Stations (SAES) in the northcentral region of the U.S. Dialogue between directors in this region led to the awareness that the problems were multi-state, or regional, in scope and that efforts by individual SAES's to address the disorders would be facilitated by collaboration among scientists from the various states affected by the problem. Meetings of SAES directors in the north central region culminated in the agreement that a regional research project was the best approach to such collaboration.

The development of a funded regional research project is described as follows:

- State Agricultural Experiment Stations receive funds from the U.S. Department of Agriculture (USDA) on a formula basis through provisions of the Hatch Act.

- This Act provides that a minimum of 25 percent of these funds be devoted to the support of regional research in the SAES.

- A regional research project may be proposed by two or more SAES's.

- Scientists cooperating in a regional research project comprise a Technical Committee which sets the research approaches and priorities and identifies the responsibilities for individual and collaborative investigations.

- The proposal developed by the Technical Committee is reviewed and approved at the regional level by SAES administrators and at the national level by the Cooperative State Research Service of the USDA.

- An SAES Director from the sponsoring region is designated as Administrative Advisor for the Technical Committee and provides authorization for expenditures of funds and guidance in the planning and review of research activities conducted by members of the Technical Committee.

- SAES scientists from other regions, as well as scientists from federal agencies, may join the project Technical Committee following appropriate approvals.

Through this process, SAES scientists in the north central region developed a project proposal that was approved and initiated in 1965. Scientists specializing in corn virology, genetics/breeding, agronomy, and entomology at north central SAES's and at certain northeast SAES's joined with federal researchers. Together they became the Technical Committee of the Regional Project NC-79, "Dwarfing Virus Diseases of Corn".

Initially, these scientists collaborated using a multi-disciplinary approach to conduct research according to the established objectives and to refine research priorities. The achievements and problems of these scientists' individual research efforts were communicated effectively within and among disciplines through direct contact at periodic meetings of the Technical Committee and through publication of research findings in scientific and popular agricultural literature.

As research progressed, science administrators were kept informed of project NC-79's achievements through reports from the federal and SAES scientists and from the Technical Committee's administrative advisor. Their research documented the association of specific strains of a virus with most of the diseased corn and the appearance (with different frequencies of occurrence) of these viral strains in many of the north central, northeastern, and southern states. The dwarfing diseases of corn then became known as Maize Dwarf Mosaic (MDM). In addition, it was determined that the disease known as corn stunt, caused by mycoplasma-like organisms, was not at that time a problem in the northeast and north central regions but was apparently confined to the southern corn producing states.

The NC-79 project provided agronomists with information on differences in susceptibility to the dwarfing virus which characterized currently-available corn varieties. The geneticists and breeders developed improved sources of the virus and studied the inheritance of resistance to the virus so that resistance factors could be bred into new corn hybrids. Virologists studied the virus particle and provided antisera to the virus. This was used in strain comparisons and diagnosis. Entomologists and virologists determined that aphids carried MDMV and that Johnson grass was the alternate host. Thus, significant accomplishments were achieved by the NC-79 project scientists.

During a short period, these accomplishments, coupled with research administrator's and scientists' concerns regarding the virus and mycoplasma-like diseases of corn in the south, established interest in a southern region research project on these problems. Following the regional communication and planning procedures mentioned earlier for project NC-79, research efforts expanded in the northeast region and were initiated in the southern region.

Dialogue among scientists in the three regions recognized that the undefined virus and virus-like problems in the south were major threats to the entire U.S. corn crop. A multidisciplinary team of scientists in the southern region prepared a proposal for a southern regional research project and submitted it to SAES directors in that region. The scientist-administrator communication network in the SAES's provided a mechanism for all levels of the USDA research enterprise to remain informed of changing needs and priorities with respect to scientist-defined approaches to a problem. Through this network, administrators and scientists in the north central and northeast regions were involved in, and were supportive of, the development of a strong southern regional research project on corn virus problems. At the same time, they recognized the diminishing needs for coordinating research on corn virus through a north central project.

These changes in research thrust and area of focus were reflected in the termination of the Technical Committee for project NC-79 in 1970 and in its replacement by a Regional Coordinating Committee (NCR-85). Although the Regional Coordinating Committee did not have regional research funds, it provided a mechanism for the former NC-79 scientists to continue to meet and to maintain linkages with scientists both in the north central states and in other regions while a new project (S-70) was being initiated.

From 1969 to 1972, research on corn virus continued in the north central and northeastern states. In addition, three significant administrative changes occurred. The southern regional research project S-70, "Viruses and Mycoplasma-like Organisms of Corn and Sorghum", was approved; the NCR-85 committee officially terminated; and scientists formerly working on corn viruses in project NC-79 joined the southern regional research project. During this period, the etiology of the MDM diseases was further defined. Additional work on the relationships of MDMV strains and their insect vectors was performed. Further research identified additional host plants, geographic occurrences and corn varietal reactions to the disease agents. Project S-70 began with the multidisciplinary participation of virologists, geneticists, breeders and entomologists. As progress was made, the cooperating scientists recognized the need for further expertise in virology and entomology; and the multidisciplinary framework grew.

Scientists working on the NC-79 and S-70 projects have been extremely productive. Collaboration and coordination between individual scientists and between states has resulted both in several significant findings about the virus and mycoplasma complexes occurring in the U.S. corn crop and in contributions toward solutions for the problems faced by corn producers. In addition to the MDMV, several other viruses have now been identified. The influences of the disease agents alone and in multiple infections have now been identified and significantly clarified. The development of virus-resistant corn cultivars, stimulated by this regional and interregional, interdisciplinary effort, has been successfully accomplished.

Many of these discoveries would not have occurred, particularly on such a timely basis, if this collaborative, regional research activity had not developed. The work has also been enhanced and accelerated by several federal special research grants that were obtained through the initiative of the project's Technical Committee. These additional resources funded a collaborative effort in obtaining and assaying diseased corn samples from throughout the nation and in identifying and characterizing the causal agents for those diseases. As a result, several previously undescribed causal agents were identified through this project.

This group of scientists has also been productive in developing new knowledge. Over 190 publications covering all aspects of corn virology, genetics, culture and entomology, have been published. The broad scope of subject matter covered in these research papers and in a widely-acclaimed technical bulletin on corn viruses reflects the value of multi-disciplinary collaboration in addressing complex disease problems through research and in identifying practical solutions for those problems.

Experience has shown that the regional and interregional, multidisciplinary technical committee approach encourages collaborative planning and research performance among scientists who are motivated to do it. For example, one team of scientists collaborated to conduct uniform corn variety tests for virus reaction. As a service to (and later for use by) scientists from other participating states, another team provided purified virus preparations and also serological assays and tests. In order to obtain critically needed data for the development of predictive models which will relate the virus disease cycle to losses in corn production due to these diseases, systems model experts have recently joined the project, further stimulating interdisciplinary interaction.

The many successes of the NC-79, NCR-85, and the S-70 group efforts reflect the deep commitment to the concept of a regional/interregional cooperation on the parts of both scientists and research administrators. They serve as examples of the administrative mechanisms which enable scientists in this system to initiate collaboration between scientists at different locations and between science disciplines within given institutions in order to form research team partnerships necessary to address the complex issues of agriculture.

Interdisciplinary Teamwork in Farming Systems Research and Development*

James R. Meiman

Successful farming systems research and development (FSR&D) requires many things, but an interdisciplinary approach is one of its fundamental requirements. This paper discusses a general model for successful interdisciplinary teamwork and its applicability to FSR&D.

Farming systems research and development requires teams whose members represent different professions or disciplines. Although much is written about interdisciplinarity and its definitions, the key ingredient for true interdisciplinarity is interaction. This interaction invariably leads to synthesis and synergism. Synthesis of knowledge among interacting disciplines produces new ideas, concepts, and solutions. This productive interaction is called synergism. Synergism implies that the whole is greater than the sum of the parts. A distinction should be made between multidisciplinarity, which simply means a combination of disciplines, and interdisciplinarity, which implies a combination of disciplines with frequent and significant interaction.

Generalizations pertaining to interdisciplinary management should be treated with the same caution as any management prescription. What works in one situation may not work in another. This is especially true in the international field where large cultural differences are encountered. Thus, the following "model for interdisciplinarity is intended as a guide for integrating individual situations.

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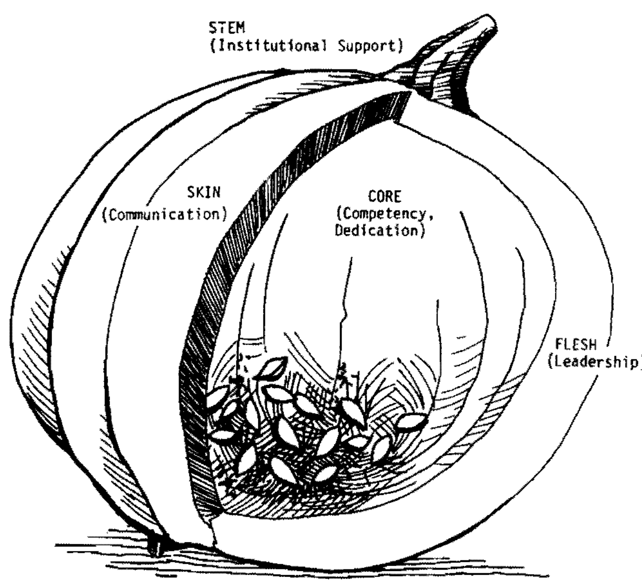
*Adapted from W. W. Shaner, P. F. Philipp, and W. R. Schmehl, (1982) Farming Systems Research and Development: Guidelines for Developing Countries, Boulder, Co.: Westview Press, and included here with permission of the publisher.

Figure 1. A model for interdisciplinarity.



A MODEL FOR INTERDISCIPLINARITY

The squash drawn in Figure 1 illustrates our model for interdisciplinary teamwork. The essential components of this model are:

1) a core of competent and dedicated workers;

2) fleshed out by adaptive and balanced leadership;

3) held together and enhanced by frequent and open communication; and

4) supported by an institutional framework that understand and rewards the extra time, effort, and funding appropriate to the costs associated with successful interdisciplinarity.

Competent and Dedicated Workers -- The "Core"

Quality interdisciplinarity demands quality in the component disciplines, regardless of the disciplines of the team members. Those who have a successful disciplinary record command the respect of others. Furthermore, a competent individual with a proven record is usually self-reliant and feels less threatened by interdisciplinarity, which often generates insecurity because it follows new courses of action or pioneers new techniques. Finally, the professional who is well-grounded in a discipline is in a good position to understand others' paradigms. But whether the professional chooses to do so or not is partly a matter of personality. This respect for the knowledge that each team member brings to a joint effort is critical to the success of interdisciplinary teams. This point is further discussed under "leadership".

The second major ingredient of the core is dedication. This dedication must be focused on the goals of the interdisciplinary effort. Members of the team must have enough commitment to the project or program goals that they are willing to endure the difficulties of collaboration to achieve these goals. Thus, the team leader and those who share the responsibility of achieving the interdisciplinary goals should have a major role in selecting team members.

Although it may be desirable, an agreeable personality is not a necessary attribute for an interdisciplinary team member. High motivation and competency are important and they are not always present in highly personable individuals. Assigning individuals who are personable but not fully committed to team goals or who lack competency in their own discipline almost ensures failure of an interdisciplinary team.

Leadership -- The "Flesh"

Given staff who are competent in their disciplines and are strongly committed to interdisciplinarity, the leader must develop a collaborative style that enables each member to fully contribute to the team's goals. Although many leadership characteristics are important, certain characteristics are critical to successful interdisciplinarity.

As with other team members, the leader must be competent in one of the disciplines relevant to the team's assignment and be committed to interdisciplinarity. In addition, the leader must be sensitive to differences between the members' disciplines. Each professional has, to some degree, a paradigmatic reference for viewing and solving problems. This paradigm is part of the professional's training and involves not only a mental framework for approaching problems but also the use of terms with specialized meanings. These two characteristics combine to form a set of mental "tools" common to that profession. An interdisciplinary leader should not only understand these professional differences, but should also seek to promote an appreciation for the differences among the team's members.

Kuhn (1970) presents a thorough discussion of paradigms, their relevance to science, and how they relate to changes in scientific thought. Among the important implications for interdisciplinary research are these:

- A paradigm is both determined by and determines those who share it.
- Because a paradigm tends to produce a strongly focused view by those who subscribe to it, communication across paradigms is difficult and may cause conflicts.
- Typically the more able scientist belongs to several paradigmatic groups -- usually closely related -- and is more open to the paradigms of others.
- Understanding a paradigm involves knowledge of the symbolic generalizations, shared commitment to certain beliefs, common values, and similar frames of reference for problem solving inherent in a specific paradigm.

In large measure the leader's success depends on the ability to synergistically bring the unique perspectives and competencies of each member together toward common goals. Closely related to achievement of this task is the leader's ability to balance flexibility and authority in dealing with team members. The leader must be firm in bringing the team to a decision on the what and when of component tasks but should leave the how to the individual and collective ingenuity of the team. This requires a delicate balance of decisiveness and patience on the leader's part.

A good summary of the theories underlying and the characteristics associated with balancing task-oriented with people-oriented styles --

i.e., concern about accomplishing tasks versus concern about people's feelings, motivations, etc. -- can be found in Reddin (1970). Among the indicators Reddin gives for the "integrated" manager are:

1) capability to harmonize institutional and individual goals;

2) reliance on common aims and ideals as the source of authority;

3) ability to use a variety of participatory techniques;

4) capacity to depersonalize authority; and

5) ability to develop a highly cooperative approach toward achievement of organizational goals.

Another important trait of a successful interdisciplinary leader is adaptiveness. The leader must use a variety of styles and techniques depending on the issues before the team and the stage of the team's effort. When a leader is not capable of such adaptiveness, then alternative strategies are needed, such as rotation of leadership among team members or strong delegation of leadership tasks. These approaches, however, cost the team time and effort.

Team effectiveness can be increased through certain exercises. Examples of useful exercises are described in a handbook by Merry and Allerhand (1977). The basic steps they recommend in team development involve the following:

1) brief problem-sensing
2) examining effects of differences in perception
3) listening and clarifying
4) giving and receiving feedback
5) looking at process and content
6) developing interactive skills
7) personal contracting with team members
8) follow-up procedures.

Another excellent reference on team building that includes a description of 46 team building activities is the manual by Francis and Young (1979). While useful, these exercises should be used judiciously since most are derived from Western experiences.

One of the most direct exercises in team building is to continuously refine objectives and tasks within the generally agreed upon goals for the team. For simplicity, we use a hierarchical arrangement of terms:

1) "goals" are the general purposes of the team effort;

2) "objectives" are the special targets to be achieved in reaching the goals; and

3) "tasks" are the jobs undertaken to achieve the objectives.

Definition of goals can generally be achieved readily. Team members who subscribe to these goals should be chosen. Experience has shown that teams sharing common goals may still have difficulty defining and agreeing upon objectives. Thus, one of the major roles of the interdisciplinary leader is to bring about agreement on team objectives and to translate them into tasks. Delineating and assigning tasks and implementing work is also difficult. Consequently, a great deal of open discussion is needed during the initial stages. This process of objective and task refinement needs continual feedback throughout the team effort and requires skilled leadership in problem solving, decision making, and implementing the results. At the same time, this process provides an ideal opportunity for the leader to build and maintain a strong collaborative team.

An additional value of refining the objectives is the continual evaluation of individual and team progress. Careful identification of the measures of accomplishment for each objective provides criteria for measuring individual and team performance. Because a team functions best when all members contribute, tasks and responsibilities should be clearly assigned, monitored, and corrected, when necessary.

<u>Frequent and Open Communication -- The "Skin"</u>

Communication has been briefly discussed under "leadership". Obviously much time must be spent in open give-and-take discussion to clarify team objectives. Communication in interdisciplinary teams must be open and continual. The first and foremost responsibility of the team leader is to ensure that this occurs, but each team member must make the extra effort to help. No clear choice as to the best way to achieve effective communication can be prescribed. However, one almost universal complaint from interdisciplinary teams is that the team members did not spend enough time together during the initial stages of their work. When available, a specialist with experience and graduate degree training in group dynamics and interpersonal communications should help the team develop these skills.

If sufficient effort has been invested initially, then the preferred model is to keep communications on an informal, need-to-know basis. Periodically, the leader should arrange a group review of progress toward objectives. Other actions by the leader to facilitate team interaction include common task assignments and having members work at the same location. The leader must provide the opportunity -- i.e., time and space -- for interaction.

Serious problems often develop when team members are at separate locations. When separate locations are necessary, the leader must exert effort and ingenuity to promote interactions among the team. Techniques include shared work plans, conference calls, site visits, delegation of leadership tasks, and social events.

Institutional Support -- The "Stem"

Interdisciplinary efforts frequently cut across established institutional boundaries. In trying to foster interdisciplinarity, institutions should:

1) assign a capable team leader;
2) delegate authority to the team leader in selecting team members;
3) allow sufficient time for teams to learn how to work productively before judging the results;
4) ensure adequate financial support; and
5) give a clearly defined reward for the team, as well as for individual performance.

Selecting a good leader, from individuals with limited or no experience leading interdisciplinary teams, is not always possible. Those in authority must be prepared to change leadership whenever the leader does not perform satisfactorily. The team will provide plenty of indications when such action is warranted. If difficulties in leadership arise, an outside trouble-shooter may be needed to aid the team leader in resolving the difficulties. When the problem cannot be solved promptly, leadership may have to be changed.

A frequent mistake when establishing interdisciplinary teams is for higher authorities to force some of their staff to become members of the team. The team leader should resist such attempts and insist on having a major role in selecting team members. Because staff selection is so important, higher authorities should allow team leaders freedom in selecting their staff. Otherwise, these authorities cannot logically hold the leaders responsible for accomplishing their objectives.

Decision makers need to realize that while interdisciplinary approaches may take more time and resources than disciplinary approaches, the interdisciplinary approach may be the only effective way to produce satisfactory results. In this sense, the interdisciplinary teams can be truly cost-effective. Unless management believes in the approach and is willing to make a full commitment, interdisciplinary teams should not be formed. Although some of the complexities may be anticipated, too often the full range of complexities are not. Without an understanding of the time required to reach fully effective teamwork, interdisciplinary activities may be halted before they have had a fair chance of succeeding.

Finally, rewards should be provided for interdisciplinary team members. This is difficult because separating an individual's contribution from the team's is often complex. Several management practices can be used to offset these difficulties. Each team member should receive a reward based on the team's performance. Further, the team leader should rate each individual's contribution to the team's output.

When appropriate, team members may also be asked to rate fellow members' contributions. When interdisciplinary teams are temporary and members come from different organizations, the team leader and higher management should inform these other organizations of the team members' contributions to the team. Furthermore, provisions are needed to ensure that these other organizations do not penalize team members because they are not contributing directly to the parent organization's activities.

APPLICATION OF INTERDISCIPLINARITY TO FSR&D

Application of the foregoing concepts to FSR&D relates to a number of general and specific considerations. These considerations are presented in the following two sections.

General Considerations

FSR&D is an interdisciplinary effort requiring at least one and usually more interdisciplinary teams. Therefore, FSR&D management needs to consider the implications at the different levels of operation and provide suitable conditions for interdisciplinary teamwork.

Leadership in FSR&D may be a critical factor in many developing countries because of the overload on existing staff. When qualified national leaders are not available, a country's decision makers will have to decide whether to entrust the leadership of a new FSR&D project or program to foreigners or to wait until the local staff can be properly trained. In view of the desirability of local leadership, such responsibilities should not be given to expatriates for more than the first one or two years of an FSR&D activity. By that time, the local staff should have received accelerated training in FSR&D and have learned FSR&D processes and methods under the expatriates.

Because FSR&D requires a long-term commitment by a variety of disciplines, the core teams in the research areas and in regional and national headquarters should be assigned to their positions for at least one and possibly several years. Rotations among these locations are useful in acquainting team members with a range of situations and activities. When bringing new members into these groups, assignments should be on a trial basis. The judgment of the team leader, along with those of team members, should determine if the new member is to be given a regular position.

A helpful device in deciding on assignments requires members of the FSR&D team to write their job descriptions according to their individual interpretation of team goals and objectives. Team interaction then leads to a workable set of descriptions and to criteria for evaluating performance and, consequently, for setting responsibilities. Moreover, this device encourages better understanding of each team member's contribution to team goals and objectives. Any serious disagreement among the members should be resolved before finalizing the job descriptions. This same general process can also be used for:

1) preparing lists of equipment, supplies, and related needs; and

2) adjusting team objectives and tasks in light of new or unexpected circumstances.

A work plan, which is the "glue" that holds the team together, needs to be developed. This work plan should contain a summary description of the above activities, a schedule for their completion, and a set of times or conditions for staff meetings. Finally, the entire team needs to review and approve the work plan.

In some developing countries, problems among the disciplines are less acute than in the industrialized economies because of the staff's education. For instance, many of the professionals engaged in FSR&D in Guatemala have a similar agronomic training. Thus, having to learn the idiosyncrasies of other disciplines is not as frequent or urgent. Other approaches that help keep disciplines from dividing into self-interested groups include focusing on solving farmers' problems and measuring team success through farmers' acceptance of new technologies. With such approaches, individuals from separate disciplines are able to identify the nature and value of their contributions to the team effort.

On the other hand, problems can arise. One type of problem may occur when interdisciplinary teams are formed by assigning staff from more than one ministry. When this occurs, the team leader needs to be sure some members are not discriminated against because their parent ministry does not place as high a value as the other ministries on the team's activities. Another type of problem arises when: (1) a discipline's members feel superior or inferior to the other members, or (2) members do not value small farmers or agriculture highly. These feelings can block effective team interactions and are especially critical when the discipline is key to the team's success. When these instances arise, FSR&D's leaders should take corrective action at the outset. Alternatives include moving the project to another ministry or changing team membership.

Other problems can arise within older and more established interdisciplinary teams (Pelz and Andrews, 1966). These authors suggest two concerns relevant to FSR&D. One is that teams do not remain open and flexible to new ideas and approaches. This problem may be alleviated by adding new members from time to time and by presenting the team with new challenges. The nature of FSR&D is such that the latter will usually happen of its own accord. Another concern is that over time the team may become less action oriented when its leader becomes more facilitative and less directive. When this occurs, the leader's contribution as an orchestrator of group processses should probably be augmented by more active leadership from those with strong technical capabilities.

Specific Considerations

Each of the FSR&D activities offers a variety of opportunities for developing effective interdisciplinary teamwork. Following are some suggestions and examples from actual practice.

Selection of the target and research areas gives the FSR&D team an early opportunity to develop an interdisciplinary approach. Establishing criteria for selection, as well as consensus on final choices, is an important team building opportunity.

During problem identification, members with different disciplines work together on a single task. Each draws on the experiences of the others in finding out what the farmers' most pressing problems really are.

In Guatemala, social scientists are paired with agronomists during the reconnaissance surveys (see Shaner, Phillip, and Schmehl, 1982, Appendix 5-Q, for more details).

Opportunities for interactions among the members are enhanced by having the pairings change each day. Researchers are encouraged to view the whole farm rather than the narrow perspective of their own discipline. Hildebrand (personal communication) has characterized the initial stage of these surveys as one in which entomologists look for insects and soil scientists gather soil samples; but by the time their work is finished, each has given up looking at just his or her own specialty and concentrates on identifying the key factors confronting the farmers.

In the above example, team members are encouraged to look beyond their disciplines by having them write about other disciplines. In writing these reports, they have access to specialists' knowledgeable in the various subjects. The idea is to encourage members to view farmers and their environments from other vantage points and to begin to learn the terms and concepts of other disciplines. In this way, for example, agronomists will learn something about calculations of economic profitability and social scientists will learn something about the experimental designs for crop and animal studies.

Planning on-farm research begins with a workshop to ensure interaction of all relevant specialists. The team leader needs to see that some individuals do not exert undue influence on the team because of their status, experience in the region, or similar reasons. All members should look at the whole system and be free to offer suggestions on all components, not just on topics pertaining to their disciplines. Alternative hypotheses should be identified. One method for this is through brainstorming sessions in which ideas are quickly and freely offered without thought as to their eventual practicality. After an ample list of possibilities has been identified, further study will show which possibilities to consider more carefully. Such an approach broadens the range of approaches and helps to avoid settling too quickly on an inferior plan of action.

On-farm experiments offer further opportunities for team building. Zandstra et al. (1981) suggests:

> "Development of strong (interdisciplinary) ties can also be assisted by the engagement of the whole team in field operations normally under the responsibility of a single team member. For example, the entire team may participate in initial survey activities or selection of plots for pattern trials or design of specific component technology trials. Also, members should visit each other's trials and discuss the implications jointly. For example, the establishment of grain legumes after rice is an area where several disciplines overlap. Standing rice stubble helps suppress early season legume pests. It also changes water losses right after rice harvest and together with minimum tillage planting techniques can save residual soil moisture. Omission of tillage requires the development of special planting techniques and the evaluation of weed control requirements. Where planting techniques require substantial labor or specialized equipment, the opinion of economists about farmers' acceptance or limits to expenditures must be considered."

This example illustrates the need for interdisciplinary teamwork in FSR&D and some of the opportunities for its accomplishment.

Finally, other opportunities will naturally present themselves during this and the remaining activities in the FSR&D process. In some cases, daily interaction is needed between researchers dealing with rangelands, livestock, crops, soil conservation, farmer preferences, societal constraints, and economic matters. Such interactions are especially pertinent when designing experiments, drawing up terms of reference for studies, and evaluating the results. Frequent interaction and cooperation should help break down the too-often-encountered practice of individuals' narrowly guarding their sources of data. These and similar suggestions should improve interdisciplinary teamwork, which is so essential to successful FSR&D activities. We provide the following checklist for successful interdisciplinarity.

CHECKLIST FOR SUCCESSFUL INTERDISCIPLINARITY

These two sets of questions may be used as a checklist for promoting successful interdisciplinary teamwork for FSR&D activities. The first set concerns topics related to the initiation of program (or project) activities, and the second set concerns topics related to program (or project) operations.

CONCERNING INITIATION OF PROGRAM ACTIVITIES

- o Has the appropriate mix of required disciplines been identified?
- o Have the most competent persons been chosen for the core team?
- o Are those chosen fully dedicated to team goals?
- o Is the team leader satisfied with the team?
- o Will the leader be able to balance task-oriented and people-oriented styles in providing creative interaction among team members?
- o Is the leader sympathetic to the different paradigms of team members?

- Is the leader competent in one of the key disciplines for the team?
- Is the leader fully dedicated to team and program goals?
- Has adequate time been provided at the outset of activities for team building and refinement of program objectives?
- Has a communications plan for the team been prepared?
- Has physical space been provided for effective team interaction?
- Have any problems of spatial separation been adequately considered?
- Does the team's institution fully support the team and the program?
- Do the team members understand the basis for the evaluation of both team and of individual accomplishments and the accompanying reward system?
- Have provisions been made for team liaison with others who are needed to support the team technically?
- Have adequate support services been provided for the team?

CONCERNING PROGRAM OPERATIONS

- Are all members contributing as part of the team?
- Is the leader able to adapt to changes in the team's needs?
- Are new team members effectively integrated into the team?
- Is provision made for additional team and program needs as they arise?
- Is the team able to accomplish agreed upon goals by accomplishing stated objectives and tasks?
- Do team members feel adequately informed?
- Are team members receiving adequate feedback on the performances of the team and themselves and are the rewards in keeping with performance?
- Are communications among team members open?
- Do team members have sufficient freedom to be creative?

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Interdisciplinary Research for Multiresource Forest Management: An Example

Lawrence D. Garrett

The interdisciplinary team approach has been critical to forest management in research planning and selection of study areas, developing multiresource inventory methodology, and in analyzing and interpreting research findings. This paper describes ECOSIM, an interdisciplinary integration of techniques for predicting multiresource outputs from forests under alternative forest management practices and strategies. Benefits of the program include identifying, as well as evaluating, the complex trade-offs involved in both the individual and the combined impacts of changes in forest management.

INTEGRATING RESEARCH AND MANAGEMENT

The United States' public looks to the National Forest System, its forest managers, and researchers for an ever-increasing array of resource outputs.

The concept of managing the nation's forests in order to derive multiple benefits for the public was initially captured in the statement of the first Chief of the United States Forest Service, Gifford Pinchot, in 1905: "The great forests of our country will be administered to yield an array of benefits to the most people in the shortest period of time". In 1960, the Multiple Use Act identified the multiple resource uses served by the National Forests. The problem of determining efficient methods for allocating resources such as timber and recreation among competing interests was not, however, resolved in this act.

Several succeeding acts made this responsibility more explicit. The Renewable Resources Planning Act (RPA) of 1974 assigned responsibility to the Chief of the Forest Service to periodically assess the supply and demand for forest and range resources (USDA, 1976). In 1976, an amendment to the RPA, titled the National Forest Management Act (NFMA), outlined systematic methodology for planning the use and future productivity of the National Forest System. The amendment addresses the need to develop standards and methodologies for improving multiresource management planning and monitoring of the National Forest System.

The National Forest System, administered by the USDA Forest Service, is comprised of 154 individual National Forests, located in nine U.S. geographic regions. Each region is administered by a regional forester; each forest is managed by a forest supervisor and skilled forest managers. Integrated with this forest management system are eight USDA Forest Service Experiment Stations. Each Experiment Station is administered by a director and staff and covers approximately a 5-9 state area. Forest research specialists, located at special laboratories and at universities within the various regions, conduct research programs in support of forest management. The responsibility for multiresource management, planning and monitoring has created a need to integrate the various professional forestry disciplines of both research and management so that effective guidelines for multiresource management can be developed and implemented. Forest managers have responded with intensive efforts devoted to land management planning (LMP). Forest managers and researchers working together strive to understand how these increasing multiple demands can best be served with the existing resource base.

Whereas forests have traditionally been managed by a number of functional resource plans (i.e., timber plan, recreation plan, etc.), the new process requires each National Forest to prepare

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a single, integrated plan developed by an interdisciplinary team referred to as the ID team. The plan required under NFMA contrasts, under differing management approaches, all resource benefits expected to accrue to a given forest and chooses the best combination of these alternatives as the management direction.

An excellent review of the LMP process is presented by Hartgraves (1981). The process is described as a nationally controlled and directed program which attempts to:

1) Determine for each forest the operational and biological feasibility of achieving specified target output levels under given budgetary, operational, and production capacity constraints.

2) Determine the allocation of production targets at regional and forest levels which minimize the total cost of production (economic, social, and environmental costs are considered).

To insure consistency and effectiveness in this planning process, a set of regulations has been developed (USDA Forest Service 1979) to guide resource management planning at all National Forests. These regulations provide conceptual foundations, as well as specific standards and methods, for implementing evaluations. To support multiresource management, the USDA Forest Service has implemented multiresource research programs which evaluate the impacts of differing management alternatives on an array of forest benefits. These programs have been multifunctional in direction and utilize interdisciplinary research teams to evaluate the forest resource base as an ecosystem which has many resource outputs.

The use of interdisciplinary teams, both in research and management, has been important in resolving many forest-related problems. Many examples exist. Teams at the Forest Products Laboratory (FPL) in Madison, Wisconsin have addressed many problems of converting our nation's raw timber into finished products for public consumption. The programs have been important in the design and evaluation of new processsing technology for lumber, plywood, and paper products.

Interdisciplinary research and management teams are also used in resolving problems associated with insect infestations in the United States and Canada. In this decade, the gypsy moth in the eastern United States threatened thousands of acres and millions of dollars of resource. Interdisciplinary research teams, working in close cooperation with management, have developed and applied new methods to combat the problem.

Still another example is represented in the USDA Forest Service's Beaver Creek Research Program in southwestern Arizona. Of the program's several research objectives, one is to provide research on new analytical methodology for assisting multiresource management of National Forests in the Southwest. The program is unique in that it has been continuously managed as a multiresource program dedicated to understanding the integrated effects of man and/or nature on the many resources that are derived from the forest (Carder, 1977).

THE BEAVER CREEK PROGRAM

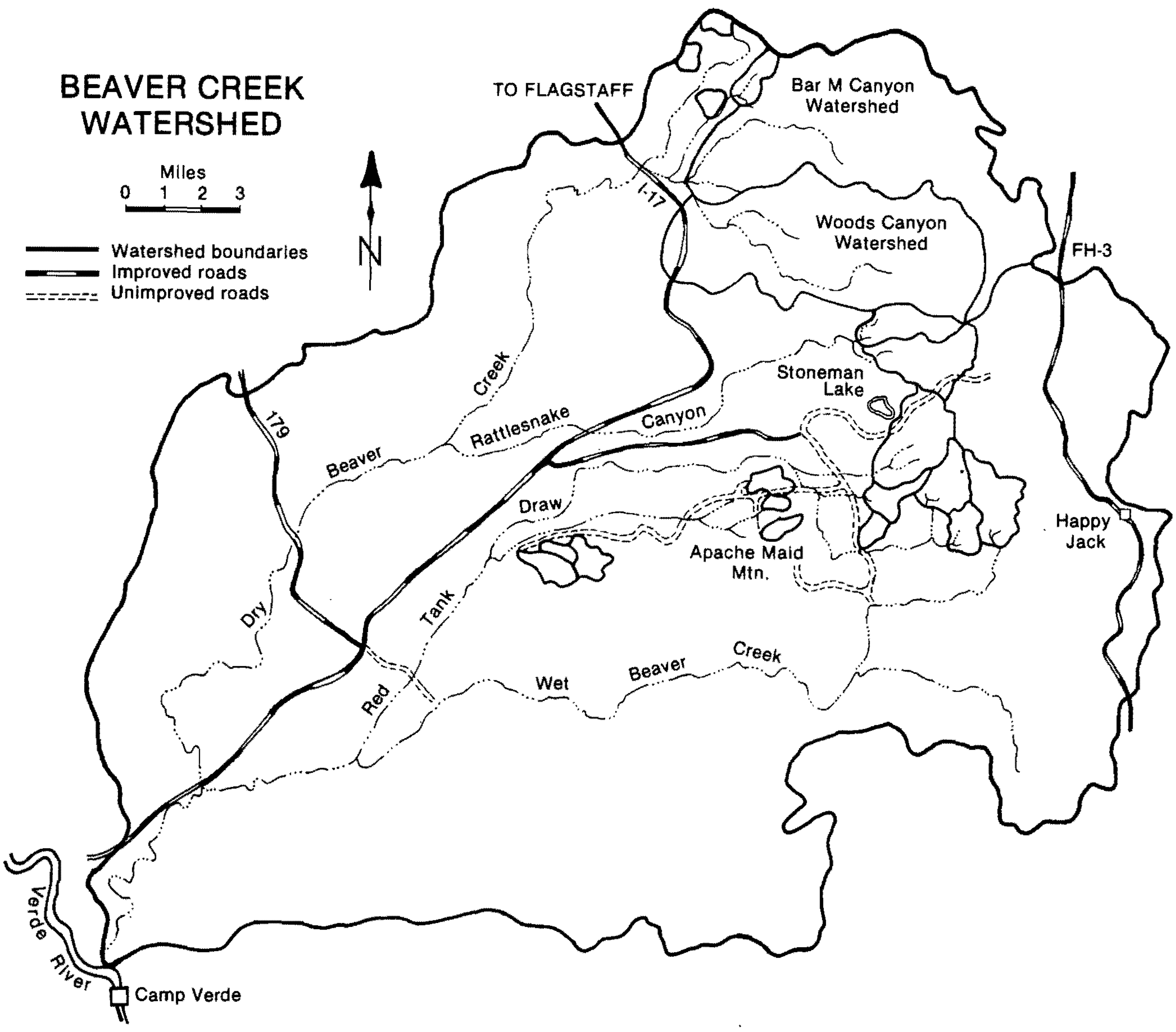
The multiresource management research project is designed to characterize the biological and economic tradeoffs in multiresource outcomes that could result from implementing different forest management alternatives. The characteristics of these outcomes are then captured in management guidelines and/or analytic methodologies for use by forest managers. The program, established in the late fifties, is informally referred to as the "Beaver Creek Program", referencing the Beaver Creek Experimental Watersheds and Biosphere Reserve on which much of the research has taken place.

The study of multiresource interactions relating to flora and fauna dynamics, water movement, wildlife, scenic beauty, etc., requires in-depth pretreatment and post-treatment studies of large research areas. To accomplish this, many small and large watersheds in the Beaver Creek Watershed research area have been studied since 1958 (Figure 1).

To characterize a range of management alternatives, differing treatments have been applied to differing watersheds. In total, 24 small watersheds ranging in size from 10 to 100 acres, 18 medium-size watersheds ranging from 60 to over 2,000 acres in size, and two large watersheds of 12,000 and 16,000 acres in size have been studied. The watersheds have had experimental treatments applied, ranging from complete removal of the overstory to increase water yields, through differing types and levels of thinning to accent timber growth and scenic beauty, to small patch overstory removal to enhance wildlife.

Treatments have been conducted in both pinyon-juniper woodlands and ponderosa pine forests -- two major vegetation types of the southwestern United States. These types, located on volcanic-derived soils, are similar to extensive forest and woodland areas of the Southwest.

Figure 1. Beaver Creek Research Watershed Area.



The Interdisciplinary Team Approach

Project scientists include economists, engineers, systems analysts, timber, wildlife, range, and hydrology specialists, as well as landscape architects and sociologists. In addition to Forest Service personnel, university personnel participate in the program, broadening the range of the interdisciplinary teams.

The interdisciplinary team research approach has been critical in:

- o Research planning and selection of study areas;
- o Developing multiresource inventory methodology; and
- o Analysis and interpretation of research findings.

It is possible for research areas to be established and research programs to be implemented on wildlife, timber, range, etc., without the specific specialists being involved. However, this can and does lead to many shortfalls. For example, selection of study areas dominated by young-growth timber precludes:

1) important multiresource research implications for wildlife resources, such as cavity-nesting birds and squirrels;
2) timber implications, such as declining growth rates and increased mortality rates from overmature trees; and
3) range implications of increased yield, due to reduced crown cover in older stands.

On the other hand, the interaction of several disciplines in determining the research design and in selecting the research sites can mitigate these problems.

Likewise, although it is possible for one individual to develop effective multiresource inventory methodology, a research team representing several disciplines may develop a more accurate and productive study. The design resulting from an interdisciplinary effort is likely to reflect a compromise in the set of parameters and in the observation methodology that, taken together, better represents existing multiresources.

Interpretation and analysis of a particular resource observation require the inputs of specific disciplines. But, an assessment of the dynamics of several interacting resources is best guided by an interdisciplinary team. The complexity of interactions and the knowledge required to interpret these diverse resources as singular and collective entities require assessments broader than those provided by an individual specialist.

Multiresource Inventory and Data Management

Inventory techniques designed for and implemented at permanent recording stations (intermittent sampling schemes, and singular time event and site-specific studies) have resulted in extensive multiresource data bases. These inventories and sample schemes have included observations, the parameters of which permit the assessment of treatment effects on various characteristics of flora, fauna, water quality and yield, sediment, nutrient cycling, site productivity, aesthetics, economics, etc. In addition, these studies have provided information on the inputs for various resources on all watersheds, for both pretreatment and post-treatment periods.

A data base management system (DBMS) is used to provide efficiency in tabulating, collating, retrieving, and processing the extensive multiresource data. The DBMS used by the Beaver Creek Program includes all resource data bases and manages these data bases for analysis and interpretation (Fong, 1975).

USING RESEARCH IN FOREST MANAGEMENT

The current planning process under NFMA requires forest managers to assess public concerns and to blend these with potential opportunities for the forest. Alternative strategies are developed by evaluating all the physical, biological, social, and economic tradeoffs of one alternative over another. That alternative which minimizes the costs of providing public benefits and opportunities is implemented and monitored over time. To define the interacting responses of resources to management treatment, and to effectively support multiresource management, forest management research uses interdisciplinary research teams and multiresource evaluations. One method through which this is accomplished is in the development of multiresource tradeoff models.

Evaluation techniques which specify alternative tradeoffs and are also reproducible and trackable over time are critical to the success of the land management planning effort. ECOSIM, a system currently under development by the Beaver Creek Program, attempts to accomplish this end. ECOSIM is basically an integration of state-of-the-art techniques for predicting multiresource outputs from forests under alternative forest management practices or strategies (Rogers et al., 1980). The system, specifically designed for use in forest planning, also provides additional optional outputs which can be used by managers for "on-the-ground" management.

The ECOSIM planning system simulates effects of forest management alternatives at the forest analysis area or sub-forest level. It predicts the long-term response of many resources, including forest overstory, understory, forest floor, sediment, scenic beauty, water, and wildlife. This capability is critical to managers who must monitor many resources simultaneously and manage their increases or decreases over time. The management tool has been developed cooperatively by USDA Forest Service research scientists and National Forest System land managers. Land managers have described their needs for predictive tools in forest planning; and researchers have responded with the best models, information, and data currently available.

ECOSIM consists of a main or executive program and four sets of components for input initialization, resource projection, activity simulation, and output and summary displays. Figure 2 shows the general relationship of these components and present areas of capability.

The main program controls the simulation and calls other components in proper sequence. It provides capability for simulating up to 10 management alternatives during a single program execution.

The input and initialization components read user instructions and a description of the current forest stand. An interactive question-answer dialogue enables the user to select different tree species, choose among optional models for some species, and define variables which describe overstory, understory, forest floor, and environment.

The resource projection components simulate resource outputs on an annual time step. This simulation is currently available for the areas of:

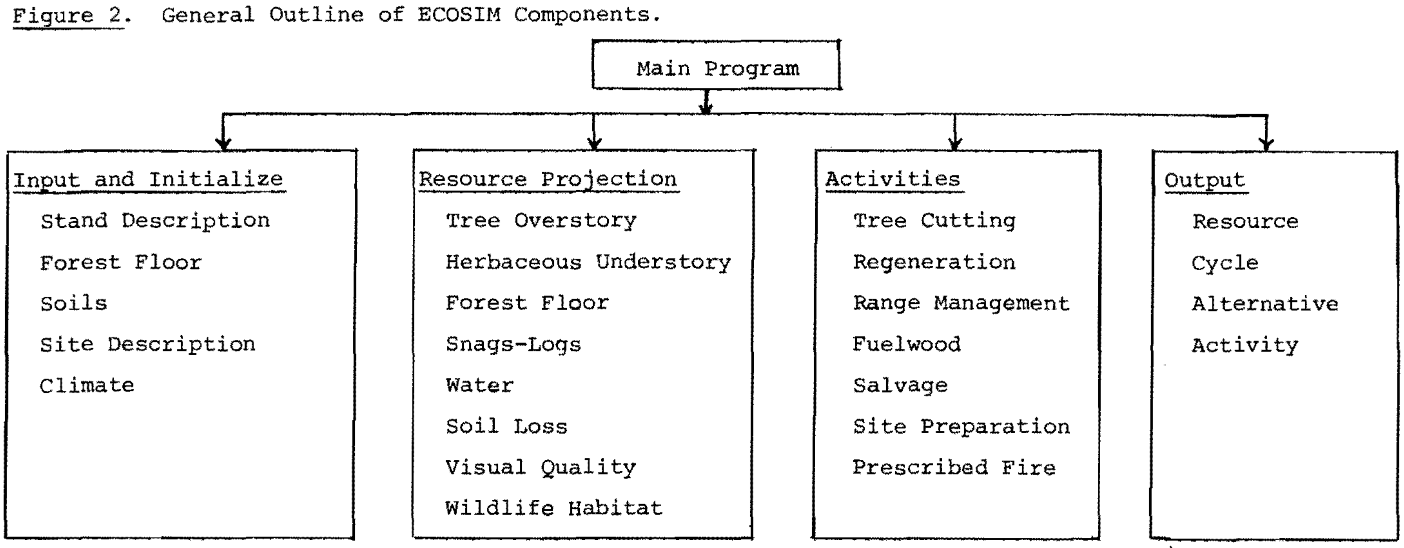
1) forest growth and yeilds;
2) herbage yields and carrying capacity;
3) forest floor, snag, log, and debris accumulation and decomposition;
4) water yields;
5) soil loss;
6) wildlife habitat; and
7) near-view scenic quality.

Activity simulators provide the manager with methods of simulating the implementation of various forest treatments under differing management alternatives. Areas of simulation capability include various methods of tree cutting, regeneration, salvage cutting, range improvement, prescribed burning, site preparation, and fuelwood removal.

Output and summary component simulators provide summaries of the effects of treatments on resources. Standard output is information required in forest planning. Optional outputs are detailed summaries of individual resource and activity impacts.

Issues and concerns often differ among forests which have significant differences in vegetation, soil types, population pressures, etc. Hence, the ability to transport ECOSIM and to localize it to the specific needs of a forest is important. This need has influenced the design and development of the ECOSIM system. Structured programming and modularity have been used to

Figure 2. General Outline of ECOSIM Components.



produce a system in which components can be easily modified, replaced, deleted, or added depending on the needs of the application. This modularity should permit forest managers in differing locales to utilize models that are specific for the conditions of that area.

DISCUSSION

Forest research and management have traditionally dealt with resource management strategies as single entities. Now, researchers and managers must look at the implications of management treatments and/or alternatives on the wide array of resources, their interrelationships, and the resulting tradeoffs. The thrust of the 1976 National Forest Management Act makes this effort an integral function of the planning process.

Just as resource interactions and tradeoffs are complex, so is the task of forest multiresource research and management. The Beaver Creek Program and the system ECOSIM represent a step toward the development of management tools to assist in this complex process. This initial step points to a promising avenue which will be further defined through more intensive research and development in this area.

The benefits which multiresource research offers to forest management are many. Most important is the ability to evaluate the extensive and complex tradeoffs that occur in a forest area when management actions are taken. Since discerning publics are scrutinizing the impacts of changes and hold the National Forest System accountable for these, it is necessary to identify both the individual and the combined impacts of changes and to evaluate management actions in light of these. Interdisciplinary teams are indispensible for effective research on multiresources management.

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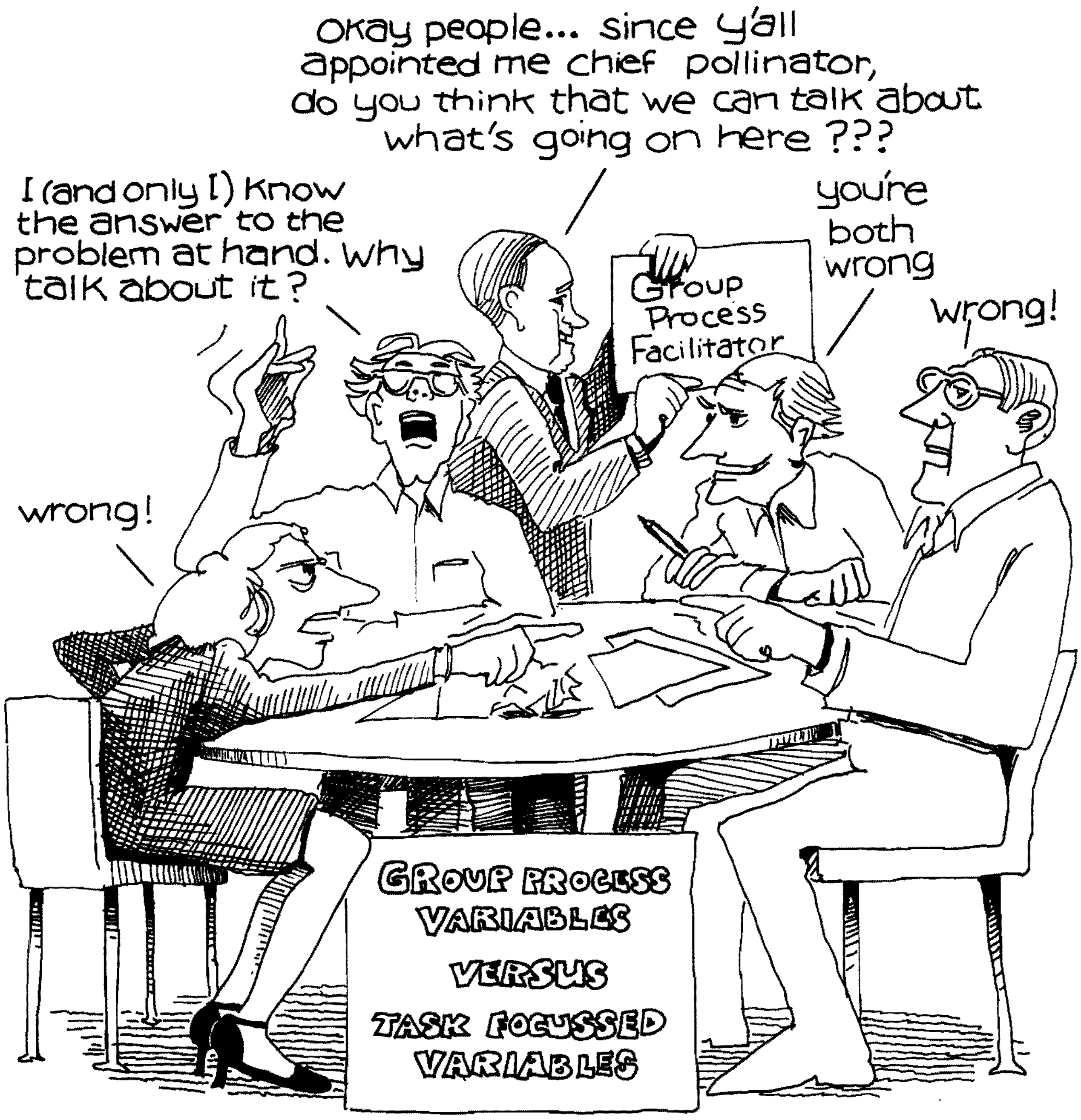
Potentials



OKay people... since y'all appointed me chief pollinator, do you think that we can talk about what's going on here ???
I (and only I) Know the answer to the problem at hand. Why talk about it?
you're both wrong
Wrong!
Group Process Facilitator
wrong!
GROUP PROCESS VARIABLES
VERSUS
TASK FOCUSSED VARIABLES

Systems Research Provides New Knowledge About Agriculture*

*Edward M. Smith***

This article describes how interdisciplinary teams in colleges of agriculture can use systems research and the computational capacity of computers to study complex agricultural systems. Systems research requires administrative sanctions which allow experienced disciplinary scientists to both work as a team and to maintain disciplinary identity and recognition. According to the author, the essential component of systems research consists of a change in attitude among administrators and scientists.

Research is indeed a search for new knowledge. This new knowledge can be manifest in the discovery of some new thing or in additional facts or principles about something which has already been discovered. Strong universities must have viable and productive research programs in order to perform their primary function, which is to teach this knowledge to students who can go out and put it to use for the betterment of mankind. Without research to provide new knowledge, a university cannot serve the needs of mankind through education.

The essential resource for research is a person who is dedicated to the search for new knowledge; who has, through the process of education, mastered previous knowledge; and who accepts, gladly, the responsibility to expand that knowledge -- through research and through preparing future researchers. By the same token, a university's most important resource is a faculty composed of individuals who possess these attributes. With this resource, the university can produce two products, new knowledge and graduates who, through the process of education, have mastered that knowledge.

The colleges of agriculture in universities are at the forefront of the continuing search for new knowledge and have amassed an enviable record. Faculties in these colleges are unequaled in university communities. Their performance in producing new knowledge and graduates who, through the process of education, have mastered the knowledge and are prepared to carry it further, is manifest in the abundance of food that is available to sustain a highly industrialized society in return for a relatively small fraction of society's output.

Colleges of agriculture, as is the case with any other entity in life, can never rest on their laurels, but must continue the search in a world which is continually changing. Agriculture is on the threshold of a new frontier in research, brought on by the tremendous progress in high speed computers. One often hears the high speed computer referred to as miraculous. However, the adjective should modify the scientist whose ingenuity devises logical mathematical programs which utilize the high speed computational capacity of the computer to gain new knowledge. Ingenuity can be applied to harness the capability of these machines, and utilize the voluminous reservoir of knowledge about specific components of agriculture that agricultural scientists have amassed. In a relatively short period of time, such efforts can produce much

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**The author would like those who read this paper to understand that the ideas, concepts and opinions expressed in this paper are those of the author and were derived from his own experiences with systems research. He intentionally refrained from using statements from other authors which would either support or refute his ideas, concepts and opinions. It is the author's hope that each of you will make such an evaluation in the light of your own needs and experiences.

new knowledge about interactions among the specific components. This process of utilizing fundamental knowledge about specific components to obtain new knowledge about interactions among the components is systems research.

A scientist who selects research as a career becomes a strict disciplinarian, by necessity, because mastery of previous knowledge is a prerequisite to any search for new knowledge. Each agricultural scientist researches a specific component of the real world of agriculture. An effective college of agriculture has a faculty of scientists whose collective inquiry yields new knowledge that is responsive to the problems which exist in the college's area of influence. One shortcoming of this disciplined research is that the need for knowledge about the interactions among these many components has not been adequately addressed. Currently, many problems in agriculture require such knowledge for their solutions. Systems research can address these interactions, and the scientists who conduct the systems research should be the same scientists who are conducting the disciplined research.

Disciplined research has resulted in the segregation of scientists into administrative units which tend to compete for facilities and funds. This segregation has not encouraged the interdisciplinary interactions which are absolutely necessary for conducting systems research. Systems research requires an interdisciplinary team of scientists, each of whom maintains a disciplinary identity. It is, in fact, the scientist's disciplined knowledge and wisdom which makes him or her an indispensable member of a systems research team. A college of agriculture does not necessarily need to change its administrative structure or form so-called institutes to carry out systems research. Once administrators and scientists recognize that systems research can provide the new knowledge needed to solve contemporary problems and that such knowledge cannot be acquired through strict disciplinary research, the essential change that is required to initiate systems research consists of a change in attitude among administrators and scientists.

A change in attitude can only result from the recognition that a new program will produce something which is needed and which cannot be produced with existing programs. Administrators need to be convinced that systems research can improve the productivity of administrative units -- as evidenced by scientific publications and quality graduates. Scientists need to be convinced that systems research can help them not only to gain new knowledge about interactions among disciplined research programs but also to strengthen each disciplined scientist in his or her own disciplinary research program. A change in attitude cannot be dictated by higher echelon administrators. However, it can be accomplished by developing among administrators and scientists a continuing dialogue in which discussions, both pro and con, can lead to a consensus concerning the need for systems research. Seminars such as this one can be effective in initiating such a dialogue.

High speed computers are developing the computational logic to deal with systems research. Many universities are training students in computer science and offering degrees in conventional agricultural fields (agricultural engineering, animal science, agronomy, entomology, agricultural economics, etc.) with minors in computer science. Some graduates of these programs are joining the faculties of colleges of agriculture with the expectations that they will be able to develop strong systems research programs. Very often the efforts of these innovative young faculty lead to naught and end in frustration because their educational experiences have not prepared them to be members of interdisciplinary teams which conduct systems research.

Systems research projects usually are not successful, nor the results recognized by the scientific community, unless senior disciplinary scientists are active members of the interdisciplinary team which conducts the research. The senior scientist's experienced judgement in observing phenomena and formulating hypotheses is indispensable when it comes to the development of mathematical relationships which accurately describe real world events. Such mathematical relationships form the basis for systems research. Thus, the knowledge and wisdom of senior scientists must be actively involved in the research from its inception. The knowledge possessed by senior scientists might be retrieved from their publications, but their wisdom can best come from personal involvement.

A system is a combination of entities which interact over time. Systems research allows scientists to gain new knowledge about how these dynamic interactions affect both the performance of the total system and the rates of time change in the selected attributes of each of the entities in the system. One pertinent example is the grazing of grass by beef animals. The principle entities in this system are grass and animals. However, there are many other peripheral entities (soil, climate, insects, diseases, management, economics, energy, etc.) which influence the dynamic behavior of each of the principal entities. One attribute that reflects the performance of a grazing system is the accumulation of weight on the animals over some period of time. This attribute is directly related to the daily rate of intake of grass by the animals and the nutritive value of the grass that is consumed each day. The daily rate of intake is, of course, limited by the quantity of grass that is consumed each day. The quantity of grass that is consumed is, of course, limited by the quantity of grass that is available and this availability is determined by the growth rate of the grass and the rate at which it is being consumed. The rate of consumption affects the growth rate of the grass and the nutritive value of the grass for the animals. The daily rate of intake is also related to certain characteristics of the animals (breed, age, weight, sex, reproductive status, number of animals per unit area of grass, etc.); and a grazing herd is usually heterogeneous with respect to these characteristics. Consequently, when these two entities are combined in a grazing system they interact dynamically to influence the performance of the system and the dynamic behavior of each of the entities. The growth rate of the grass, rate of change in nutritive value of the grass, and the voluntary daily intake by the animals are among the important attributes which are influenced by the interactions.

An exhaustive review of scientific literature attests to the inability of traditional grazing experiments to elucidate these dynamic interactions. Inferences from these experiments are often contradictory because of these interactions and because of the fact that physical measurements cannot be made to elucidate these interactions without interfering with their dynamic progression. Systems research can be used to explore these dynamic interactions and, thus, enable scientists to re-examine the data from these grazing experiments.

The scientific method for conducting research to gain new knowledge about a phenomenon involves the development of a hypothesis, followed by experiments in which observations are made and analyzed to verify the validity of the hypothesis. Early Renaissance individuals using only the simplest tools and measures, applied the scientific method to understanding phenomena such as gravity, combustion, and light. By dint of their great thought and experiment, enough was learned to harness energy for work. The Industrial Revolution resulted. The evolution of scientific thought, driven since then by the method of hypothesis and experiment, has led us to the coordinated analysis of highly complex problems which exceed the capacity of one individual's effort. The modern computer-based simulation has all the characteristics of the traditional experiment. Its results are reproducible, it is quantitative, and it can be tested.

Experiments are, in reality, systems and include entities which interact over time to cause the results which are observed during the experiment. Even though the experiment is designed to observe selected attributes of selected entities, the scientist recognizes that there are other peripheral entities that influence the observations, and that these peripheral influences have to be controlled, or in some way taken into account when inferences are made concerning the validity of a hypothesis. In most instances, the scientist designs each experiment to satisfy a statistical model. The observations made during the experiment feed into the statistical model, and the model output serves as a basis for either accepting or rejecting the hypothesis.

Statistical models are developed from the premise that the uncontrolled peripheral influences are caused by random events. This premise is not always valid; for example, field experiments include sunshine, rainfall, wind and air temperature as peripheral entities which cannot be controlled. Their influences are not caused by random events, so the inferences from these experiments are very specific because the statistical model does not consider the dynamic interactive influences of the peripheral entities. Systems research uses causal models which enable the scientist to make inferences from these same experiments. However, inferences made through systems research are not restricted by the specificity that is inherent in statistical models.

Einstein's theories on relativity were based upon his observations of the orderly progression of natural events. Eisteinian relativity precludes randomness and postulates that the mathematical forms of natural laws are invariant and that time is the only independent variable in the orderly progression of natural events. These postulates provide the basis for systems research. Systems research is unique as a concept because of the fundamental premises that all natural phenomena, in fact most phenomena, are a result of dynamic interactions among entities, each of which is always undergoing change; and that the only independent variable is time.

Statistical models are useful in designing and interpreting the results from experiments in order to study interactions, especially when the interacting entities can be parameterized and controlled at preset levels during the experiment. Simulation models, on the other hand, are needed in designing and interpreting the results from experiments in order to study dynamic interactions when the interacting entities vary during the experiment, i.e., experiments to study systems in the real world.

Simulation models are sets of mathematical relationships which describe the time rate of change in selected attributes of entities in real world systems. These models are dynamic and mimic continuous and discrete real world events. Systems research involves the development of the mathematical relationships, the construction of simulation models to mimic selected systems, and the use of the models to gain new knowledge about dynamic interactions among the entities in selected systems.

An orderly format for scientific thought and action in systems research is as follows:

1. Conceptualize systems and discern new knowledge which is to be gained from systems research.

2. Identify entities which interact with each other to form systems and describe the interactions which need to be elucidated in order to gain the new knowledge which is desired.

3. Identify scientists who understand the invariant natural laws which are basic to the development of mathematical relationships for each of the entities in the conceptualized systems.

4. Develop interests among these scientists in the proposed systems research so that a research team can be assembled to plan, seek support for, and carry out the proposed systems research. Administrative sanction is very important to this phase of the research in order to assure the maintenance of disciplinary identity and recognition of scientists who will participate in the systems research.

5. Identify the attributes of each systems entity which needs to be simulated, develop hypotheses concerning the mathematical relationships which can be used to simulate the time rate of change of each attribute, and conduct experiments or utilize data from previous experiments to verify these hypotheses. This phase of systems research can be the most exciting activity for the scientists, individually and collectively, because the disciplinary interaction of scientific thought can, seemingly, explode with new concepts and revelations about old concepts of how dynamic systems really function in light of peripheral constraints. Each participating scientist comes to the realization, in a relatively short period of time, that this collective exercise is going to be worthwhile in his or her own professional development.

6. Construct mathematical-logical simulation models which incorporate the time rate of change relationships for the various attributes of the systems entities. The disciplinary expertise of computer science is necessary during this phase of systems research; however, this discipline is not sufficient in and of itself because the knowledge of all the pertinent disciplines is needed to guide the construction process. The construction process involves the formulation and verification of hypotheses about the logical arrangement of mathematical relationships to accurately mimic conceptualized systems.

7. Validate the simulation's results by comparison with known data, by statistical methods, and by experiment. The simulation can never be proven, only tested and compared to factual data. The process of validation is enlightening. It requires comparisons with a large body of knowledge and experience and the coordination of many minds to make the comparisons. The participants can make permanent contributions to the model's logic and precision, and begin to use the model as an experimental unit.

8. Use the simulation models to conduct systems research to study dynamic interactions among systems entities and gain new knowledge about agricultural systems. Simulation models, which result from scientific thought and action, and which are scientifically documented and verified, are useful as laboratories to conduct systems research. Experiments can be conducted using the models as laboratories, and valid inferences can be derived from these experiments. The inferences (new knowledge) from these simulated experiments cannot be gained by using statistical models on real agricultural systems because the statistical models cannot accommodate dynamic interactions. Systems research with real agricultural systems is enormously expensive as compared to research with simulated systems. Research with simulated systems, on the other hand, gives more information (new knowledge) about systems performance and how the dynamic interactions among systems entities affect performance.

In summary, the main ideas proposed in this paper are as follows:

1. Research is a search for new knowledge, and new knowledge is the life blood of colleges of agriculture. It provides a basis for growth in education and service.

2. Colleges of agriculture have excellent faculties whose disciplinary research has provided new knowledge that supports an industry which supplies food to an industrial society for a relatively small cost in terms of the output of that society.

3. Systems research offers a new frontier for colleges of agriculture -- to couple interdisciplinary research teams with the computational capacity of high speed computers to gain new knowledge about complex agricultural systems.

4. Systems research requires administrative sanctions which allow a group of experienced disciplinary scientists to work as a team and still maintain disciplinary identity and recognition.

5. Systems research should not be undertaken by employing a recent graduate with a major in computer science with the expectation that this person will initiate such a program. This person and the program will fail unless experienced scientists on the faculty are prepared to work actively in the program.

6. Systems research can provide new knowledge about agriculture. This new knowledge is needed as a basis for finding solutions to complex problems in contemporary agriculture. With this new knowledge, colleges of agriculture can grow in education and service.

Systems Approach to Animal Agriculture*

H. A. Fitzhugh and E. K. Byington

Systems research in animal agriculture uses different types of models. Developing and using these models promotes team work in research. This paper discusses several advantages to using modeling approaches: that decisions produced from the systems approach are based on more rational grounds; that critics of the decision can pinpoint areas of concern; that new information can be more easily incorporated into new models; that the consequences of decisions can be quickly re-evaluated; and that decision makers become more accountable for their actions.

The importance of animal agriculture in the service of man is well documented. In 1970 the total production of food energy from animal products was 574 thousand million Mcal and that of protein 22 million tons (Table 1); these are expected to increase substantially by the year 2000. Apart from producing highly desired food products, animals provide fiber and hides and serve as a major source of work power. Their wastes are also valuable sources of fuel and fertilizer (Fitzhugh, 1978).

Notwithstanding these many contributions, there are many critics of animal agriculture. In support of such criticism, they cite health hazards from saturated animal fats and cholesterol, ecological degradation from overgrazing, inhumane treatment of animals in intensive production systems, and most often, the relative inefficiency of animals as converters of plant materials and other resources.

MEETING DEMAND

Sorting through the claims and counterclaims of both proponents and opponents of animal agriculture, it seems clear that meeting consumer demand for animal products must be realized through more efficient management of natural resources. Important decisions will be made by producers, processors, scientists, planners and politicians which will determine animal management and breeding plans, resource allocations, investment strategies, research priorities and government policies with regard to animal agriculture.

Considerable information and experience are available on which to base these decisions. A special need exists to organize this information, cull extraneous material, and identify what further data are required. The best decisions are likely to be those based on an objective, holistic analysis of animal agriculture systems. Such a management and decision-making strategy is embodied in the systems approach.

COMPLEX APPROACH

Animal agriculture systems are complex; they are composed of and influenced by interacting biological, climatic, social, cultural and economic factors. Biological components include the provision of nutrients through plant agriculture, the inhibition of animal performance by disease organisms, and the animals themselves. Socio-economic variables, including disposable income, taste, preference, and government policies, all affect supply and demand and, ultimately, profitability. Clearly, decisions based on simple analyses involving only a few of these factors are unlikely to be effective in improving the efficiency of the system.

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*Adapted from "Systems Approach to Animal Agriculture," Science and Public Policy, February 1979, with permission of Pergamon Press.

Table 1. Food Energy and Protein Production from World Ruminant, Pig and Poultry Populations. Annual Production for 1970 and Projected for Year 2000.

Product	Livestock Units[1] (Millions)		Food Energy[2] (thousand million Mcal)		Food Protein[3] (Million metric tons)	
	1970	2000	1970	2000	1970	2000
Ruminants	1195	1540	373	638	16	28
Pigs	154	190	144	215	2	3
Poultry	55	68	57	100	4	7
Total	1404	1798	574	953	22	38

Source: Fitzhugh et al. (1978).

Notes:

1. Livestock unit: cattle = 0.8; buffalo = 1.0; sheep and goats - 0.1; poultry = 0.01; swine = 0.5.
2. Physiological fuel values (PFV), expressed as average Mcal/kg carcass weight: cattle and buffalo meat 2.31; sheep and goat meat 2.0; pig meat 4.2; poultry meat 1.4. PFV, Mcal/kg milk: cattle 0.62; buffalo 1.0; sheep 1.12; goats 0.75. PFV in eggs, Mcal/kg 1.5.
3. Net protein vale (NPV), expressed as g/kg carcass weight: cattle and buffalo meat 105; sheep and goat meet 89; pig meat 60; poultry meat 126. NPV, g/kg milk: cattle 28; buffalo 32; sheep 48; goat 28. NPV in eggs, g/kg 115.

Many sophisticated tools -- mathematical programming, computer simulation, network analysis, flow diagrams -- are commonly employed in the systems approach. However, they are only tools. The systems approach itself is a philosophical basis for problem identification, analysis and decision making.

According to Morley (1972), the systems approach attempts to incorporate in the study all the elements which influence a decision, a response, or the understanding of some phenomenon, within defined boundaries. The setting of appropriate boundaries to define the system under study is essential.

SYSTEM DEFINITION

Exclusion of variables or processes which are critical to the operation of the system or subsystem of interest carries the danger of inappropriate or even erroneous conclusions. Inclusion of non-critical variables or processes unduly complicates analysis of the system.

The definition of the system, including the precision with which variables, constraints and processes are described, will vary with the goals of the analysis, these goals being generally incorporated into the objective function. The objective may be to maximize the output of animal products, efficiency or profitability; or it may be to minimize costs or risks of failure. Constraints to system operation (e.g., limitations to available land, labor or capital) must be taken into account in accomplishing the objectives for the system. Optimization of programming procedures in identifying strategies is best achieved if system variables and processes can be described quantitatively (Ladson, 1970).

The systems approach is not new. Traditional subsistence agriculturists tend to follow a systems approach. Their goal of producing the essentials for survival with minimal risk of complete failure has generally led to highly diversified systems in which animals have an integral role. Labor with low opportunity costs partially substitutes for the principal constraints of land and capital. Land and capital growth have subsequently led to more highly diversified systems.

Traditional smallholders are generally conservative and hesitant to make major changes in systems which have evolved under multiple constraints and stresses. Lessons learned the hard way favor small changes with secure fall-back positions. Thus, those seeking to 'improve' traditional systems would do well to seek first the advice and counsel of the smallholders themselves. This step is at least as important as sophisticated experimentation in understanding how and why the system works.

Changes intended to lead to improvements should be introduced to animal systems with care and caution. Airline pilots practice for hours on flight simulators before taking responsibility for the lives of their passengers, and generals test their military strategies with computer-simulated war games. These professionals use models to gain necessary experience without the risk of costly failure. It is no less appropriate that decisions and management strategies for improved animal agriculture be first tested without risk to the lives and livelihood of producers.

Fortunately, the systems approach is increasingly recognized as the appropriate means of improving animal agriculture. Research and development institutions, such as the U.S. Meat Animal Research Center, the UK Grassland Research Institute, the International Livestock Centre for Africa, and Winrock International, specifically recognize the systems approach as central to their mission and philosophy.

ANALYST'S TOOL

Modelling is a powerful tool in the hands of the systems analyst. Some advantages from the use of models include the:

1) Organization of available knowledge about the system for a clearer understanding of the interacting variables and processes which constitute the system.

2) Identification of gaps in knowledge and understanding of the system which must be resolved by further research.

3) Stimulation of multidisciplinary approaches to system improvement, and coordination of teams of specialists in a common effort.

4) Generation of better understanding of system operation and the necessary experience base for managers and other decision makers to make changes with less risk of costly failure.

5) Extrapolation from situations in which the system's operation is well known to new, less understood or future situations.

Models are an attempt to adequately approximate the real world system of interest. The important terms are adequately and approximate. Inadequate models lead to erroneous conclusions about the system. On the other hand, overly detailed models of the system are confusing and may share the analytical interactability of the real world system.

PHYSICAL MODELS

A model airplane may be used to simulate real aircraft in wind-tunnel tests. Animal experiments are, likewise, the physical models of the real world system to which results will be applied.

And, as is the case for any model, experimental results have little value unless the experimental model adequately approximates the target commercial system. Demonstration models are an effective means of extending new technologies.

Graphs, diagrams and charts portraying a real world system are an effective means of identifying system inputs, outputs, constraints, processes and goals. These visual representations assist in organizing available information, setting boundaries, assigning tasks to research teams and describing the system function.

The schematic model indicates the components and functional interrelationships contained within a system, but it does not provide quantitative information about how the system operates and how it will respond under various conditions. The schematic model must be transformed into a mathematical model in order to obtain this type of information.

Increased use of mathematical models has closely paralleled the development of high-speed computers and supportive software. Computations which would previously have been practically, if not physically, impossible can now be made in seconds. It is not even necessary to understand how computers operate or the underlying logic of programming algorism. This ease of accessibility to powerful tools is both a boon and a curse to the decision maker.

Much of the concern and criticism of mathematical modelling results from modellers' promising too much and delivering too little. Ignorance of the underlying biological, social and economic relationships cannot be compensated by more and more sophisticated mathematical methods and computing equipment. Even more dangerous is an inaccurate or inadequate model of the system that can transform perfectly good input data into garbage.

The mathematical model is based on a conceptual model. Both are descriptions of the real world. The mathematical model is written in more precise language. The mathematical model is not more accurate simply because it is more precise. Translation of verbal and graphic models into more precise mathematical models requires a clear and exact description of how the system is perceived. The rigorous analysis reflected in the formulation of a mathematical model makes such a model a valuable tool in systems analysis (Lee, 1973).

Compromise between model comprehensiveness and analytical manageability tends to produce models that are subsystem specific for a particular location. This limitation can be overcome by examining the model's logic and underlying assumptions and by modifying them to reflect the unique features of the new situations to which they may be applied.

MODEL DEVELOPMENT

Model building and application should be part of a coordinated effort to improve the operation of the real world system in accord with the specified objective function. The model development staff and the management staff interact through the model to identify constraints in the system. Management can recommend possible modifications of the system to remove the constraints, and the model can be used to evaluate the recommended actions. Potentially successful recommendations are then field tested, and the results are used to validate or to modify the model so that it reflects more accurately the real world system being examined.

So much time and effort can be put into developing a mathematical model that the model can become the end product -- instead of the means to an end. One way to ensure the effective use of a model as a management tool is to maintain continuous communication feedback between the system analysis team and the decision makers (producers, processors, politicians and consumers).

Mathematical models of animal systems do not replace but, rather, complement experimental research. Research provides the base data and understanding of biological, social and economic functions necessary for adequate mathematical characterization of the system. On the other hand, models of animal systems identify critical knowledge gaps. Finely tuned analyses help determine the precision with which experimental vaiables must be measured.

For example, as pointed out by Cartwright, the biology of weight gain is relatively well understood (Cartwright, 1977). However, considerably more research on weight loss is needed in order for effective decisions to be made about feeding high-producing dairy cows. These cows are often in negative energy balance early in lactation, especially in the tropics where seasonal variation in quantity and quality of forage has a major effect on body weight. Assuming adequate knowledge of the biology of weight loss, models of such production systems would be effective tools in the design of strategic nutrient supplementation programs to increase productivity and efficiency.

EFFECTIVE TEAMWORK

A primary advantage of modelling is the promotion of effective teamwork. The model serves as a common frame of reference, helping to bridge communication barriers which too often hinder the multidisciplinary efforts critical to practical decision making.

One useful technique in modelling animal systems is to partition the system into components or subsystems to allow specialists to concentrate their attention on the components about which they know most. In a beef cattle production model, range scientists and agronomists may concentrate on the feed production models, animal scientists on the cattle production models. Finally, the component models must be combined. This requires the common efforts of all disciplinary specialists.

EXTRAPOLATION POTENTIAL

The potential for extrapolation from known to unknown situations is one of the most valuable opportunities for use of models and also the one most fraught with danger (Joandet and Cartwright, 1975).

Models may be used to test strategies or make predictions when it is not feasible to observe or measure the real world system. For example, models were used to estimate feed energy requirements of the world ruminant population -- both current and projected -- for the year 2000 (Fitzhugh, et al., 1977). Comparisons of feed requirements and feed availability in 15 regions of the world provided a basis for suggesting strategies to improve the efficiency of ruminant populations.

Animal systems are influenced by so many factors that it is rarely feasible to conduct controlled experiments under all possible situations. Production and economic conditions can and do change rapidly, and decision makers need to be able to respond to these changes just as rapidly.

A major advantage of well-designed, accurate models is that they can be used to test the sensitivity of the system to change. Test results are available in minutes, not in the months or years which would be required to observe response in the real world system. Models of animal systems based on data and documentation from the developed world may serve as initial planning aids in the development of improved strategies for animal systems in the developing world. The obvious caveat about all such exercises is that both modeller and decision maker must be exceedingly critical of all extrapolations until they have been proven in practice.

Political decisions affecting animal agriculture systems are often made on a 'trial-and-error' basis. Unfortunately, the errors may have disastrous consequences for producers and consumers alike. For example, the controversial question of livestock feed additives (diethystilbestrol, antibiotics, etc.) has entered the political arena in the United States.

The U.S. Congress is considering legislation to ban some or all feed additives because of their alleged human health hazard. Proponents of feed additives question the wisdom of doing away with practices which increase the productivity and efficiency of animal systems at a time when many people of the world are hungry. Headley, using comprehensive bioeconomic simulation models of cattle, swine and poultry systems, examined the projected consequences in the United States of eliminating several important feed additives (Headley, 1978). Among the conclusions were that intensive poultry and swine management systems would be most changed by the removal of antibiotics, and that the consumer, not the producer, would bear the cost of removing growth promoters from animal rations. The results of this model simulation study will be most helpful to politicians as they weigh the economic impact of their actions on both producers and consumers of animal products.

TRADITIONAL APPROACH

The traditional approach to decision making generally involves a model of the system and prediction of expected consequences of the decision. The trouble is that the model is often a poorly specified mental image of the system largely derived from the decision maker's past experiences. Important variables and processes may be unconsciously omitted from this 'mental model' with neither the decision maker nor anyone else being aware of the omissions. Decisions are made on less rational, objective grounds using the traditional approach. They tend to depend on linear extrapolations of the effects of changing single variables. However, most systems are composed of variables and processes which may behave non-linearly with multiple feedbacks and interactions among variables. These variables and processes should be considered simultaneously.

Even when mental models of the system, developed through long experience, function effectively, the decision maker may have difficulty communicating the basis for decisions to others who do not share the same mental image. Poor communication leads to confusion and controversy. These impede the acceptance and implementation of decisions. Even worse, mental models are just as mortal as the person; death can terminate a model based on 40-50 years of experience.

The systems approach avoids many of these problems by utilizing quantitative models of the target system. Certainly, experience generated from both research and first-hand association with the real world system should supplement the quantitative model. Many important factors -- especially socio-cultural variables and processes -- are poorly quantifiable at best. However, a clearly specified model makes apparent which values are based on well-documented results, which are best guesses, and which are strictly subjective opinions.

The systems approach does not guarantee that decisions will be popular, easily implemented or even correct. They will, however, be based on more objective, rational grounds. Critics of the decision (or the model on which it was based) can pinpoint their areas of concern. New information can be more easily incorporated in models, and the consequences of decisions can be quickly re-valuated. And, for better or worse, decision makers many become more accountable for their actions.

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Evaluating Interdisciplinary Research*

Martha Garrett Russell

The evaluation of interdisciplinary research is maximized when both disciplinary and interdisciplinary affiliations and communication are strong for reviewers and researchers, when screening and strengthening criteria move research activities in the same direction, and when planning and evaluation occur in tandem. This paper addresses the evaluation process for IDR in terms of its rationale, its timing, its criteria, and its agents. It explores the need to employ multiple assessment criteria so as to maintain both disciplinary rigor and interdisciplinary relevance.

BACKGROUND

Interdisciplinarity has been one of the continuing forces in the history of knowledge. Even the earliest communities of scholars recognized the importance of collaborative efforts among academicians in developing the philosophy of science (Gusdorf, 1977). Their collaboration served as a basis for professional development (Beaver & Rosen, 1978). Later, the technological revolution encouraged the acceleration and expansion of knowledge production.

Both the expansion and the utilization of knowledge have produced a need for greater diversity in scientific communities and also in the accountability mechanisms whereby these disciplined groups monitor their paradigms and progress. These needs for specialization have provided impetus for formalizing various types of collaboration into subdisciplines and disciplines.

This dynamism of differentiation and integration among scientific disciplines exists in an environment in which one supplements the other and in which each is a precondition to the other (Darvas & Haraszthy, 1979). It has been at the points of differentiation among the disciplinary specializations that standards for academic respectability have evolved (Peston, 1978). These standards establish, for each discipline, the criteria for admission into that community of scholars, the range of problems considered important, the approaches considered appropriate, and the criteria for legitimizing findings as new knowledge through publication. This evaluation is characteristically performed by scientific peers.

These disciplinary communities and their standards have been essential in the transfer of scientific knowledge into educational programs, into cognitive and social processes, and in training researchers' perspectives. At the same time, interdisciplinary efforts have and continue to characterize not only the research frontier (Blauberg & Mirsky, 1979) but also the utilization of knowledge.

Two broad classes of criteria motivate and regulate research activities. Firstly, the profession of learning stimulates research which advances knowledge by expanding the pool of knowledge. This research is traditionally disciplinary, but is also interdisciplinary in "the interrelatedness of basic science phenomena" (Huston, 1981). Secondly, pressures for problem solving stimulate research efforts which contribute to solutions.

Criteria for evaluating disciplinary research focus on the assessment of its scientific

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soundness and depend on the criteria established by the contributing disciplines (Cotterrell, 1979). The standards for interdisciplinary research assessment involve these criteria, and additionally include the criteria of mission or policy relevance and the utilization of findings.

As an interdisciplinary research effort continues beyond an exploratory phase, the continued external referral to disciplines for validation and assessment sometimes grows cumbersome. In some cases, this inconvenience leads the interdisciplinary researchers to concentrate on certain aspects of the interdisciplinary analysis on which there is internal agreement.

Increased agreement on internal criteria develops as the specific interdisciplinary community establishes its own internal standards and may deemphasize the disciplinary assessments which gave it validity at the onset. This increased concensus which results permits the interdisciplinary field to shed some of its reliance on criteria established by the parent discipline.

Sometimes, the infant interdisciplinary effort becomes, itself, a parent discipline. Parent disciplines that continue to produce new interdisciplinary efforts or that maintain sufficient control over their boundaries survive to the next generation. Others serve their function then disband when no longer needed.

The growth of knowledge and the need for new perspectives to deal with changing complexities have converged in the development of new specializations within the scientific community. Each new phase of specialization has limited the concerns, the perspectives, the paradigms, the methods and the theoretical frameworks of the succeeding subdisciplines (Price, 1980). In order to establish standards of academic respectability or negligence, the paradigms for these new subdisciplines must be articulated. At the same time, the issues identified by sponsors and by scientists continue to be broader than any one specialization. These issues call forth research activities which lie outside of the traditional turfs of disciplines and subdisciplines.

It is in this context that the interest in, concern for, and importance of interdisciplinary research, brought about through collaboration, continues to grow (Russell, 1982). The capacity of interdisciplinary research to address very difficult research questions, especially when the procedures and outcomes of the research activities are uncertain, has been repeatedly acknowledged. Yet these very complex problems and responsive procedures resist easy application of the peer review process. The needs and missions out of which such problems are identified, the variety in combinations of disciplinary approaches, and the outcome uncertainty of such research make evaluation difficult and raise several critical questions:

> Why should IDR be evaluated?
> When is it appropriate to perform the evaluation?
> What aspects of IDR can or should be evaluated?
> Who should perform the evaluation?

WHY SHOULD IDR BE EVALUATED?

Evaluation, of which peer review is one type, is at the heart of quality control in the scientific community. Research results are not considered new knowledge until they undergo critical review and acceptance by the scientific community. This validation of scientific merit is an assessment of quality -- of rigor in theory and methodology and of consistency in interpretation.

Such quality control not only decides which works merit publication in professional and scholarly journals, but also controls the entry of new members into the community and identifies which members deserve special recognition (Bowers, 1975). This review process typically occurs after the research has been completed and written up. The publication of research confirms that it has been performed.[1] The evaluation of research confirms its compliance with standards of respectability.

Funded research, which accounts for much of the large scale interdisciplinary research, carries with it several additional requirements because fund administrators must account for the money allocated to research activities. How has the money been spent? What did it buy? These are issues that surface throughout the life of the sponsored research project in reporting systems. The missions and goals under which money is awarded to research are, in some environments, broadly defined and in others targeted to specific needs. In either case, evaluation for accountability to the sponsor is required (NCR, 1980).

Evaluation provides information for determining how resources will be used (ex ante risk assessment) and how they were used (ex post accountability). Through such feedback it permits scientists and administrators to benefit from accumulated experience. The evaluation of interdisciplinary research is, in this context, useful in both screening the most promising research proposals and in strengthening contributions to the foundation of knowledge.

But although responsibility for the screening and strengthening of interdisciplinary research resides with the sponsors, administrators and editors, inputs for their decisions come from scientists. Several surveys of administrator attitudes toward the critical input received through the peer review of proposals have documented administrator reliance on the critiques by specialists who are informed about the area of study proposed (Vandette, 1977; Cole, Rubin & Cole, 1978). The review by knowledgeable peers is a vital input into sponsor's as well as editors' (Gordon, 1980) decisions.

This input is also valuable in addressing administrators' concerns for the organizational processes which permit the conduct of research with the fewest impediments, with the harmonious confluence of research results with agency mission and institutional reputation, and with the responsiveness to constituencies. Prestige in the public sector institutions and productivity in the private sector institutions are critical to the administrator's ability to assemble and maintain a capable research staff (Ruttan, 1981).

From the sponsor/administrators' perspective, the evaluation of scientific merit and research quality is directly tied to the institution's overall capability to conduct research and to the administration's role in maintaining consistency in research funding. This stability is important in avoiding the disruption of productive programs. Critique by peers can estimate the likelihood that proposed research will result in prestige and productivity.

But the opportunity to review research also offers benefits to the reviewers. Written reports of proposed, as well as completed, research inform reviewers of new directions in scientific thought, of new applications for existing techniques, and of new techniques. Citations or summaries of related literature provide reviewers with reviews of literature on the proposed issues and thereby serve to maintain reviewers' awareness of the research front.

In this way, the reviewer/investigator relationship is reciprocal. Because most reviewers are also investigators, the reciprocal benefits of the review are extended into subsequent research efforts. Thus, peer review serves as a continuing education process for the reviewers and as a feedback device for investigators. The potential for interdisciplinary research evaluation to expand and refine scientific disciplines lies in the responsiveness of reviewers and scientists to this process.

WHEN IS IT APPROPRIATE TO PERFORM THE EVALUATION?

In the case of federally-sponsored research in the U.S., it has been mandated that the potential value of research be evaluated before it is funded. However, among even the most capable researchers, and especially among the most innovative research projects, the outcomes of research are usually known only after the research has been conducted. The research front, like other frontiers, consists largely of unknowns. Interdisciplinary research is highly speculative research and therefore frequently involves higher risk. In order to justify the continued flow of resources into research activities, the administrators of those funds must justify that the money is well spent and that the risks chosen are those most likely to pay off.[2]

This justification includes several types of accountability. Financial and administrative accountability is implemented through reporting and through reviews of expenditures and operations. Scientific accountability involves the recommendations of peer scientists to sponsors and editors. Therefore, the involvement of peer scientists plays a major role in deciding what work will be supported, who shall carry out the work, and what inquiry is significant.[3]

Thus the evaluation criteria imposed by peer scientists guide research activities before (ex ante) and after (ex post) they are conducted. Ex ante review of proposed research is directed by the sponsor via the selection of reviewers. In some cases, ex post review may be performed by the sponsor to determine continuation of funds. However, in most cases, ex post review is largely self-enforced in the research community and is conducted independent of sponsor intervention. This occurs first as editorial boards of discipline-affiliated journals review research results submitted for publication (Gordon, 1980). Through publication, a group of specialists acknowledges an addition to the pool of knowledge affiliated with the discipline they represent.

This review by a group of peers is followed by the evaluation and approval evidenced in citing a publication. A citation acknowledges that the contribution to the pool of knowledge was worthy of becoming part of the foundation. Indicators of ex post review (publications and citations) subsequently become part of the information used to estimate likelihood of success in ex ante reviews. In this respect, the absence of evaluation criteria and the absence of appropriately focused scholarly journals are both problematic in the ex post review of interdisciplinary research.

Interdisciplinary research presents additional challenges to reviewers, administrators and sponsors because collaborative efforts require the coordination of personnel and organizational units (Ikenberry & Friedman, 1972). It is in this context of research management that in-process performance evaluation of ongoing research is significant.

In-process evaluation of the leadership and management of the research project provides information about the congruence of intended and realized goals. In-process review is also helpful in guiding the adjustment of objectives and management strategies. Especially at the frontiers of knowledge, monitoring the progress and process of research activities is essential in recognizing major breakthroughs, avoiding dead-ends, and maintaining appropriate direction.

The in-process review of research performance is also important in allocating institutional rewards to scientists. Evaluation of scientists for tenure and promotion decisions, merit reviews and special recognitions frequently does not coincide with project completion or ex post review. These evaluations are apt to occur at all stages of research activities.

Typically, in such assessments, scientific peers apply disciplinary criteria - usually to a single individual scientist. "The departmental affiliation is the basis for dispersing rewards, considering resource needs, conducting graduate education, achieving professional integrity and allowing others to understand that disciplinary specialty" (Mitchell, 1977). However, the value of interdisciplinary research efforts may be unrecognized by applying disciplinary criteria. In addition, collaborative accomplishments may be undervalued or considered less meritorious.

To this point, the why and when of interdisciplinary research evaluation have been discussed. Ex ante evaluation provides an assessment of risk in allocating funds and reflects the potential contribution of the proposed research to science and also to the identified research issue. In-process evaluation can strengthen research by monitoring its process and progress and providing impetus for redirection when necessary. Ex post evaluation is appropriate in validating the scientific merit of completed research, as it is submitted as new knowledge to the scientific community and as its results contribute to solutions.

Two questions remain. What can or should be evaluated in interdisciplinary research? Who are the appropriate agents to evaluate interdisciplinary research?

WHAT ASPECTS OF IDR CAN OR SHOULD BE EVALUATED?

The evaluation of research has challenged scientists, administrators and sponsors. It includes evaluation of the outcomes, processes and inputs.

Research outcomes are varied, and they are not mutually exclusive. The same result can be viewed differently, depending on the evaluator. Research outcomes have been typified in many different ways:

- as those which have extrinsic vs. intrinsic value (Price, 1977);
- as representative of scientific activity, scientific production or scientific progress (Birnbaum, 1981);
- as fulfilling basic vs. applied research goals (Whitney & Frost, 1981); and
- by considering separately researcher productivity and research productivity (Andrews, 1979).

<u>Outputs</u>

Studies of researcher productivity have used measures of publications, citation counts, patents, ability to attract funding and production of graduate degrees (Birnbaum, 1981; Busch et al., 1980; Evenson and Wright, 1980; Liebert, 1976; Neumann, 1979; Pelz & Andrews, 1966; Price and Gursey, 1976; Salisbury, 1970). Some studies have noted the congruence of ratings of researcher productivity with ratings of research productivity (Andrews, 1979). However, indicators of research performance have been less systematically studied than indicators of researcher performance.

Several critical factors in research productivity have been identified:

- the need to distinguish successful from unsuccessful innovations (Mosteller, 1981);
- the need to develop sponsor agreement on program goals and priorities against which research performance can be evaluated (NCR, 1980); and
- the need to differentiate between "scientific merit" -- quality of research -- and "scientific contribution" -- applicability to the discipline (Cutler, 1974).

Differences in standards for assessing the economic quality of research and the scientific quality of research have also been noted (Evenson and Wright, 1980). Standards of economic quality involve the assessment of multiple spillover effects (Evenson & Kislev, 1975), as well as the impact of technological change (Price, 1977). These research performance criteria are especially important to sponsor decisions.

Team Process

The evaluation of interdisciplinary research involves these criteria, but additionally it requires criteria which acknowledge the unique qualities of interdisciplinary research. One such added dimension is synergy and the orientation to group output, as opposed to the traditionally used orientation to individual output. In-process review of research is considered an unnecessary burden by many scientists. For the interdisciplinary research project, however, the conduct of research is complicated by the need to maintain cooperation and communication toward a common goal. In-process review can provide indicators about the health of interdisciplinary synergy and teamwork.

Inputs

Five questions have been proposed for the ex ante evaluation of interdisciplinary research (Peston, 1978).

- Does the project formulated in interdisciplinary terms show a recognition of the existing contribution made by the separate disciplines?
- Is the interdisciplinarity genuine in the sense that the problems are formulated in terms which enable the different disciplines to get together rather than compete with one another?
- Is the method of data acquisition likely to be helpful to all the relevant disciplines or is it biased in a particular direction?
- Does the interdisciplinarity enhance the possibility of hypothesis testing or does it obscure it?
- What difference will the results of the research make to the policy decisions that will eventually be implemented?

WHO SHOULD PERFORM THE EVALUATION?

The next question which follows from these is then how can all of these perspectives be brought to bear on any specific program? Regardless of the institutional or project basis for allocating research support (Bredahl, Bryant & Ruttan, 1980), sponsor, administrator, peer and client inputs are important in building research activities.

Sponsor evaluation is most appropriate before the research is begun and after it has been completed, in order to provide an assessment of risk and to account for the allocation of funds. Administrator evaluation can strengthen the conduct of research through in-process monitoring. Evaluation by peer scientists is appropriate in assessing the potential contributions of proposed research, as part of a screening process, and in validating the scientific contribution to new knowledge - again screening but also strengthening the discipline and its knowledge base.

Because of the potential for interdisciplinary research outcomes to contribute solutions to identified problems, client or user interest may be appropriate inputs at both ex ante and ex post phases. Indeed, the documented returns-on-investment in agricultural research have been partially attributed to clientele involvement in research at both phases and also through extension programs (Dalrymple, Ruttan & Evenson, 1980).

To apply the review criteria identified here, peer reviewers must not only have an intimate familiarity with the disciplinary backgrounds involved but also understand the complex environment toward which the research is focused. Peer review has evolved as an accountability mechanism for disciplines within the scientific community, and it tends to be a conservative influence. "Peer groups have not rewarded members who apply their training to problems that extend beyond disciplinary confines" (Baram, 1981).

It has also been argued that peer review teams represent political, rather than theoretical, structures (Morphet, 1981) and that concern for rules and boundaries by the scientific community will heighten when paradigms or models are felt to be insecure or threatened (Kuhn, 1980). Peer review is consequently limited by the restricted vision which experts who are drawn from the same, self-enclosed pool of scientists tend to maintain. These disciplinary criteria provide the rigor of the disciplines and, at the same time, the barriers to acknowledging the relevance and merit of interdisciplinary efforts.

"Problem oriented research is no respector of disciplines" (Cotterrell, 1977). Yet, for the most part, sponsoring agencies and institutions continue to award funds and incentives along disciplinary structures (Currie, 1976). Disciplinary standards of efficiency and effectiveness are not necessarily consistent with efficiency and effectiveness standards of the scientific community as a whole.

The determination as to which of these standards will be applied to interdisciplinary research rests partially in the scientific backgrounds of the reviewers selected and partially in the agency and/or discipline's parameters for review. It is, therefore, in the flexibility of selecting such reviewers that the optimization and the relevance of the peer review in interdisciplinary research resides.

It has been suggested that the essential condition for interdisciplinary knowledge is competence in all fields called upon to contribute (Sinaceur, 1977). Additionally, the need for convergence of science-oriented criteria and mission-oriented criteria within reviewers' assessments has been noted (Cutler, 1974). The dynamism of the scientific community's specialization and integration modes calls for reviewers who are specialized in many areas. The continued coupling and recoupling of new and existing specializations require flexibility in the composition of such peer review teams.

And yet the maintenance of direction and standards requires stability in the process and criteria. Modification of university reward structures (Currie, 1976), making the rewards for interdisciplinary research more visible (Gordon & Marquis, 1966), and facilitating the use of a common language among reviewers (Gusdorf, 1977) have all been suggested as ways to improve the review process and accommodate the needs for peer review in interdisciplinary research.

SUMMARY

"The interdisciplinary spirit is first and foremost a need for communication" (Gusdorf, 1977). The organization and communication links between interdisciplinary groups and parent disciplines need to remain strong -- as do the links within the disciplines. In order for disciplines to continually reformulate themselves and contribute to the advance of knowledge, a balance has to be maintained between the support of well-established, high-quality endeavors and the support of new, untried investigators and institutions.

"Interdisciplinary work poses among its many challenges that of obtaining the breadth of vision and range of knowledge necessary to move with confidence beyond orthodox disciplinary boundaries" (Cotterrell, 1977). Especially in viewing the importance of freedom of inquiry in academic environments, interdisciplinary efforts play an important role in maintaining intellectual vitality through encouraging flexibility and responsiveness. Interdisciplinary efforts encourage these changes to be made in moderate increments. They also allow faculty to initiate shifts in direction and to anticipate changing organizational goals. Involvement in interdisciplinary research permits scientists to not only pioneer the explorations but to also choose the frontiers.

It has been alledged that peer review is the most important single determinant of funding decisions made by representatives of sponsors (Cole, Rubin, Cole, 1977) and is a significant factor in editorial acceptance (Gordon, 1980). Yet peer review is rarely the single determinant. Some critics maintain that sponsor/administrator and editor decisions are influenced by political as well as social considerations (Bowers, 1975; Carter, 1979; Liebert, 1976; Symington & Dramer, 1977).

The strength of the peer review system resides in maintaining standards of quality which have been previously agreed upon. This strength is vital to interdisciplinary research at two points. The first is in maintaining the strength of disciplinary backgrounds which researchers take with them into interdisciplinary teams. Strong backgrounds form a strong base. The second is in the development of consensus on new quality standards for the interdisciplinary effort - whether that effort serves the purpose for which it was created and then dissolves or whether it proves sufficiently useful that it is refined and reformulated into a new disciplinary structure.

To the extent that the component disciplines are strong, reviewers can assist sponsors and administrators in assessing the stability of the foundations upon which the interdisciplinary effort proposes to build.

To the extent that reviewers have a holistic perspective of knowledge creation and use, they can offer their best guesses as to the success of proposed research and thereby assist in screening projects which are likely to be productive.

To the extent that reviewers have the intellectual courage to entertain the unknown, they can help in recommending the best "next steps" to attempt to take in advancing knowledge. This they do through recommendations for project funding and through approval for publication.

In both the screening and strengthening objectives and at initial, mid-point and final stages of research, the potential in evaluating interdisciplinary research resides in the creative tension between the parent disciplines and the infant interdisciplines. In an environment of exploration and of inquiry, this potential is realized in its fullest sense when planning and evaluation occur in tandem, when screening and strengthening criteria move the research activities in the same direction, and when both disciplinary and interdisciplinary affiliations and communications are strong for reviewers and for researchers.

FOOTNOTES

[1] *Publication provides the corrective process, the evaluation, and the assent of the relevant scientific community (Price, 1980). There is some evidence that this peer review of scientific inquiry and personnel operates most effectively in disciplines in which there is agreement on criteria and in which the visibility of consequences is high. However, this evidence also supports the view that freedom from the constraints of academic departments which are organized around disciplinary perspectives is associated with more innovative research activities (Gordon & Marquis, 1966).*

[2] *It has been noted that the emphasis on more effective monitoring of research outputs tends to be greatest in those environments in which there is strong clientele pressure (Ruttan, 1980).*

[3] *Sponsors and editors are responsible for selecting scientists who are qualified to make these judgements, and differences in selection qualifications have been noted (Bowers, 1975; Gordon, 1980; Peston, 1978).*

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Selected Annotated Bibliography

Martha Garrett Russell and John Cornwell

Allen, Thomas J., Denis M. S. Lee, and Michael L. Tushman. (1980) "R&D Performance as a Function of Internal Communication, Project Management, and the Nature of the Work." IEEE Transactions on Engineering Management, 27:1, February, 2-11.

This study indicates that many research and development functions operate differently. Research projects, development projects, and technical service projects were analyzed in terms of the technical information communicated within and outside the organization. Previous studies have shown that communication in R&D organizations is important. This paper suggests that the amount and value of internal communication varies, and therefore different managerial strategies are necessary, depending on the work being done. Research projects seem to show higher performance when they are not dominated by any individual, while technical service projects seem to perform better when the manager is more in control of internal communication. This study showed that product and process development projects benefit from internal communication far more than research or technical service projects.

Russell is Administrative Fellow, Minnesota Agricultural Experiment Station and Cornwell is Editor, Adapt Software Systems. Cornwell was Research Assistant, Minnesota Agricultural Experiment Station, at the time this was prepared.

Armstrong, David L., Charles W. Laughlin, and George S. Ayers. (1977) "Administration of Interdisciplinary Activities." HortScience. 12:1, February, 35-36.

This paper contends that when interdisciplinary programs are recognized as legitimate activities by the disciplinary guild, the concepts developed through these interdisciplinary activities will become the foundations for the future. Considerations for administrators in meeting the challenges to interdisciplinary research are identified. These considerations include a unique, positive attitude by administrators, maturity and recognition of individual researchers, and a long-term commitment at the college and departmental levels to make interdisciplinary programs work.

Barth, Richard J. and Rudy Steck, eds. (1979) Interdisciplinary Research Groups: Their Management and Organization. Proceedings of the First International Conference on Interdisciplinary Research Groups, Schloss Reisenburg, Federal Republic of Germany, April 22-28, 1979.

These proceedings contain the invited papers presented at the First International Conference on Interdisciplinary Research Groups. Papers address topics concerning: the state of the art and actual problems in research on interdisciplinary groups; organizational approaches and management concepts for interdisciplinary research; management of interdisciplinary R&D in industry; improving communication and cooperation in international and interdisciplinary research; and interdisciplinary research in the university setting.

Birnbaum, Philip H. (1982) "Progress Report on the Organization and Management of Interdisciplinary Research." Journal of the Society of Research Administrators, 13:4, Spring 11-23.

Research on interdisciplinary research (IDR) in 11 nations on two continents is reviewed in terms of the inputs, processes, products, and scientific progress which have so far been identified. The results of this review indicate that IDR is more appropriate for certain types of research problems and for different stages in the research process. Specific features of organization, leadership, and team interaction have been associated with different research products. Areas for both descriptive and normative research are identified for further investigation.

Blackwell, Gordon W. (1955) "Multidisciplinary Team Research". Social Forces, 33, 367-374.

The features which distinguish multidisciplinary team research from other kinds of research undertakings are elucidated. Problems to be anticipated in multidisciplinary research and how they can be solved are discussed. Three dimensions of a research undertaking relevant to multidisciplinary team research are described: the number of people doing the research; the kind of action involved in the research process; and the number of disciplines involved in the research.

Blaschke, Dieter. (1980) "Management Problems of Interdisciplinary Basic Research in the Social Sciences". Interstudy Bulletin, 1: August.

A special research promotion program in Germany is reported. The importance of cooperative research which passes beyond the traditional boundaries between departments and disciplines is stressed, and options for improving research management are explored. An overall evaluation after 10 years showed that the social sciences faced serious management problems in trying to fulfill the sponsor's objectives. These problems are discussed in terms of the sociology of science and of organizational sociology.

Bouvare, P. (1977) "Multidisciplinary Forestry Research: Different Views". Management of Forestry Research for Results, Proceedings of the Third Meeting of the Subject Group S6.06, September 1977.

Multidisciplinary research is being conducted in forestry research institutes, according to this paper, since most are autonomous organizations with research teams studying different areas of forest management and exploitation. Problems in multidisciplinary research are discussed with reference to research teams, programs, and the training of individual researchers. Suggestions are given for effectively encouraging multidisciplinary research in forestry research institutions.

Busch, Lawrence. (1980) "Structure and Negotiation in the Agricultural Sciences". Rural Sociology. 45:1, Spring 26-48.

The contemporary structure of the American agricultural sciences is described, and certain negotiations carried on within that structure are elaborated. Several examples are used to illustrate how contemporary structure can be traced back to earlier negotiations, and some variations among agricultural disciplines are noted. It is concluded that an applied sociology of the agricultural sciences could contribute much to the resolution of both disciplinary and social problems.

Carrese, Louis M. and Carl G. Baker. (1967) "The Convergence Technique: A Method for the Planning and Programming of Research Efforts". Management Science. 13:8, April, B-420-B-438.

The difficulties encountered in attempts to directly apply some of the standard network analysis techniques to the planning of research programs are discussed. The particularized requirements for a planning system suitable for research efforts are identified, and a technique developed specifically for the planning and programming of research efforts is described. The application of this technique to two biomedical research programs of the National Cancer Institute is discussed.

Chubin, Daryl E., Frederick A. Rossini, Alan L. Porter, and Ian I. Mitroff. (1979) "Experimental Technology Assessment: Explorations in Processes of Interdisciplinary Team Research". Technological Forecasting and Social Change. 15, 87-94.

Technology Assessment (TA) is team research that entails the cooperative effort of professionals from diverse disciplines. Whether this effort can be truly integrated into an interdisciplinary assessment is problematic, based on analyses of 24 actual TA's. To probe the situational and process factors that impinge on this interdisci-

plinary research process, the authors performed laboratory simulations on TA-like problems. By controlling several key factors, these sessions have yielded insights into small group interactions and offer suggestive evidence for the conduct of future TA's. These exploratory TA simulations suggest merit in further efforts toward the controlled study of complex interdisciplinary processes.

Dailey, Robert C. (1978) "The Role of Team and Task Characteristics in R&D Team Collaborative Problem Solving and Productivity". Management Science. 24:15, November, 1579-1588.

This paper describes a study conducted on project groups of scientists and analyzes task characteristics and their relationships with team properties, collaborative problem solving, and team productivity. According to this study, productivity may suffer when scientists and engineers perceive their teams as being highly cohesive; task characteristics affect the relationship between group process and productivity; and it becomes difficult for large groups to avoid losses in problem solving behavior due to increasing demands for communication.

Davies, George B., and Alan W. Pearson. (1980) "The Application of Some Group Problem-Solving Approaches to Project Selection in Research and Development." IEEE Transactions on Engineering Management. EM-27:3, August 66-73.

This paper describes how different methods of project selection can be put together to form one flexible, systematic method and illustrates the approach through the use of an example. Most proposals are developed in the context of a need, and their characteristics modified as they are tested against the objectives which must be met. For such a process to be successful, according to this article, it must involve many people, and it has been found in practice that a number of the group problem-solving approaches described in the literature can be of considerable value for bringing out ideas, generating alternatives, identifying parameters, and encouraging implementation.

Denny, Fred I. (1980) "Management of an R&D Planning Process Utilizing an Advisory Group." IEEE Transactions on Engineering Management. EM-27:2, May, 34-36.

This paper emphasizes the practical aspects of selecting projects or programs for funding by utilizing an experience-based resource group or team of experts. A process is proposed for identifying research and development (R&D) needs in a systematic manner which attempts to minimize impulsiveness, while maximizing careful review and decision making. The methodology discussed has proven effective in clarifying problem areas and in producing group consensus. Common pitfalls and means to circumvent inefficiencies are noted.

Dewhirst, L. W. (1981) "Cooperative Regional Research, A Discussion of the Program." Arizona Agricultural Experiment Station. (Mimeographed) November, 1981.

This paper is a critical analysis of what the author considers a highly successful program. It discusses the history, objectives, and accomplishments of regional research projects. A statement of concern given is that this program was set up at a time when scientists approached problems on a single discipline basis. The complexity of scientific problems today requires the attention of several scientific disciplines; however, regional research is silent on this need. This requirement to deal with complexity defies many of the established administrative and management systems. According to this article, regional research provides the framework for accomplishing interdisciplinary research. Possible modifications of the program are stated, such as to place the emphasis on interdisciplinary research rather than on regional research and to eliminate the four regions by conbining them into one.

Eaton, Joseph W. (1951) "Social Processes of Professional Teamwork." American Sociological Review. 16, October, 707-713.

Advantages and difficulties of team research, with special emphasis on the structure and processes of the interpersonal relationships of participating scientists, are discussed. The author warns that individual genius and good personal relationships among collaborators are essential for productive multi-professional research.

Gulf Universities Research Consortium. (1981) "Methods to Support Multi-University Interdisciplinary Research Program." Interstudy Bulletin. 2:4, February.

This document describes the process by which the complete correlative analysis and interpretation of large data bases which contain highly diversified data can be carried out rapidly and efficiently. Each step in the process is described in terms of its objective, the nature of the

procedures involved, and the requirements that its accomplishment imposes on the software system used to build and operate the data base. The capabilities of the process and software are demonstrated and are applicable to a broad range of scientific and management problems, according to this paper.

Hagstrom, Warren O. (1964) "Traditional and Modern Forms of Scientific Teamwork." Administrative Science Quarterly. 9:3, December, 241-263.

Traditional and modern forms of scientific teamwork are described, and primary problems of each are discussed. According to this paper, traditional forms consist of freely collaborating professional peers and of teachers and their students. At the same time, economic and technological changes in research have stimulated the development of new forms of teamwork involving greater dependence of scientists on external authorities, greater centralization of authority in research organizations, and a complex division of labor involving professionals from various disciplines.

Halpin, James E. (1978) "Cooperation Within and Among State Agricultural Experiment Stations - Implications for Entomologists and Engineers." Entomology Society of America Bulletin. 24:1, 68-70.

It is argued that the State Agricultural Experiment Stations provide the best organizational structures for the initiation and development of interdisciplinary research. As components of universities which house numerous disciplines in close proximity, State Agricultural Experiment Stations have a history of, and a future stake in, providing the opportunity for cooperative research. Using entomology and agricultural engineering as examples of departments that can benefit from working together, a strong case for interdisciplinary research within and between stations is presented.

Hanway, D. G. (1981) "The Future of Experiment Station Varieties." Department of Agronomy, University of Nebraska (Mimeographed).

Experiment stations must expand their crop variety development programs by assembling and supporting the comprehensive variety improvement teams needed to integrate the many scientific disciplines that relate to improving each major crop commodity, according to this paper. The basis for this paper was a questionnaire, dealing mainly with field and forage crops, which was sent to the department heads of agronomy or appropriate plant and crop science departments across the nation. The department heads were asked to estimate the percentage of commercial companies' or experiment stations' involvement in breeding the varieties or hybrids of each field and forage crop that farmers in their state were using. The results from this, and other questions in the survey, led the author to conclude that there is an urgent need to establish interdisciplinary crop variety development teams in universities.

Horsfall, James G., Director Emeritus. (1981) "Memorandum to Director Waggoner." Memo to P. E. Waggoner, The Connecticut Agricultural Experiment Station, December 8, 1981.

This document presents the author's reflections on interdisciplinary researches at The Connecticut Agricultural Experiment Station. According to this paper, interdepartmental cooperation has been assured over the years by the policy of no departmental budgets. The history of corn genetics research is described. The administrative dimensions of personnel, discipline, administrative unit, and time are discussed. Appendices are included to illustrate points brought out in the memorandum.

Huston, Keith. (1980) "Priority Setting Processes in the State Agricultural Experiment Stations." A report to the Office of Technology Assessment Congress of the United States, September 1, 1980.

According to this report, priority setting guides the allocation of scarce resources in the development of research programs to meet local, state, regional, national, and international needs and concerns. The report points out that most State Agricultural Experiment Stations have been setting priorities to develop research programs for 60 to 100 years or more, and that in Stations with operational research programs composed of high priority investigations priority setting means reassessing current programs in the light of changing needs and emerging new needs. Providing a thorough description of the priority setting processes, it suggests that interdisciplinary research may become increasingly important.

Ketcham, David E., and Keith B. Shea. (1977) "USDA Combined Forest Pest Research and Development Program." Journal of Forestry. July, 404-407.

In 1974 the U.S. Department of Agriculture activated the Combined Forest Pest Research and Development Program to provide technology for minimizing intolerable losses from the southern pine beetle, douglas-fir tussock moth, and gypsy moth. Participating were departmental agencies, state agricultural experiment stations and forestry organizations, universities, and industry. This was the first time an interagency research program had been administered directly from the office of the Secretary of Agriculture. According to this paper, significant progress was made during the first three years.

Klassen, Waldemar. (1978) "Interdisciplinary Programs in Insect Mass Rearing and in Insect Population Management." Entomology Society of America Bulletin. 24:1, 63-65.

The need is expressed for more research engineers with expertise in such engineering sciences as fluid dynamics, optics, electronics, aerodynamics, and systems science, to work with entomological research units. This article also proposes the need for engineers who can lead the way in applying basic advances in these engineering sciences to entomological research.

MacDonald, William R. (1982) "The Management of Interdisciplinary Research Teams: A Literature Review." A Report Prepared on Behalf of the Department of the Environment and the Department of Agriculture, Government of Alberta, Canada.

This report reviews the literature on the management and organization of scientific interdisciplinary research. Studies and theoretical perspectives on team leadership, factors which affect team performance, and task characteristics of the research program are summarized. Suggestions for guiding working relationships in teams are given.

Mapp, Harry P., Jr. (1982) "Engineering Needs of Other Disciplines: Agricultural Economics." Professional Paper No. p-1155. Presented at the Southeast Region of the American Society of Agricultural Engineer's annual meeting, Orlando, Florida, February 7-10, 1982.

The need for interdisciplinary cooperative efforts by all people in the university setting in order to make meaningful contributions toward solving the future problems in agriculture is stressed. Taking into consideration the optimistic and pessimistic views of the future of the agricultural industry, this paper outlines future research issues. The search for low cost fuels and alternative energy sources, low pressure irrigation systems and more efficient application systems, more productive and efficient machinery, and engineering data on a probabilistic basis for decision making under risky conditions are among the research topics addressed.

Maryland Agricultural Experiment Station. (1981) "Proposed Master Plan for the Maryland Agricultural Experiment Station, Section on Research Programs." (Mimeographed).

Interdisciplinary research (IDR) programs at the Maryland AES in cooperation with the Wye Institute are described. Beef production systems and comparable forage systems to note the interaction of managed grazers and prominent wildlife species are discussed, as well as cooperative research between agriculturalists and wildlife specialists. IDR programs in the areas of integrated pest management, plant genetics and breeding, energy development and conservation, quality of life, and the interaction of land and water agriculture-aquaculture are also described.

Meadows, A. J. (1974) "Scientific Collaboration and Status." Communication Science. 172-206.

This paper describes a study of scientific publications. The results indicate that collaboration in research can be measured adequately from multiple authorship of papers. This phenomenon has been increasing in all major branches of science during the present century, according to this paper. Quality of work seems to be more important than quantity if a scientist is to gain the recognition of peers. Nevertheless, quantity of publications and scientific eminence show considerable correlation.

Missouri Agricultural Experiment Station. (1974) ""An Administrative Structure for Interdisciplinary Research Involving Animal Nutrition, Physiology, and Metabolism." (Mimeographed) May 24, 1974.

A system which ensures smooth operation of a research center and encourages participation in interdisciplinary research programs, without adding administrative loads to individual staff members is described. This system defines a focal point for the research and provides for a coordinator who is an active participant in the research. According to this report, the coordinator structure has implications for individual staff members, department heads, station directors, each department, and extension.

Missouri Agricultural Experiment Station. (1974) "An Administrative Structure for Interdisciplinary Research in Forage Breeding, Management, and Utilization." (Mimeographed) September 10, 1974.

This report describes a program at the Missouri AES which provides a structure for administering interdisciplinary research (IDR). A project coordinator is assigned to serve as a contact with the director of the experiment station, department heads, extension leaders, funding agencies, and staff members. The coordinator administers a research center, and participates in the IDR program in his or her own area of expertise. This coordinated program is designed to facilitate the operation of IDR, strengthen attempts to obtain funds, and allow for greater flexibility in college and departmental planning.

Morphet, C. (1981) "Positivist and Political Approaches to Interdisciplinarity." Science and Public Policy. February, 18-22.

This article elaborates on the nature and definition of interdisciplinary research. According to this article there is no unique basis for interdisciplinary research, and theoretical structures which coordinate disciplines will be more or less specific to problem areas. Like disciplines, they will demand creative generation and will direct analysis. Policy is rarely made on the basis of interdisciplinary analysis, but is made as a result of an interdisciplinary process.

Newell, William T., Borje O. Saxberg, Brian W. Mar, and Sheila A. Acams. (1980) "Guidelines for Applied Interdisciplinary Research in the University: How to Manage the University Role in Solving Society's Problems." Interstudy Bulletin, 1:2, August.

To assist project directors, principal investigators, center or institute directors, and other university research administrators, this paper is a summary report of information gleaned from a nationwide study of university applied interdisciplinary research (IDR). It explores potential pitfalls and concludes with suggestions on how to facilitate IDR. An extensive and comprehensive essay, this report addresses managerial problems, challenges, and solutions in the central university administration, research centers and institutes, and in research projects and teams.

Nissan, Alfred H. (1981) "Academic Research -- Can it be 'Managed by Objectives?'" Tappi. 64:10, October 39-42.

Management of research by objectives has desirable features, but it contains inherent dangers, especially when applied in academic settings, according to this article. By guarding against some common obstacles, management of interdisciplinary research by objectives can provide the desirable features of establishing goals, and estimating time and cost. Research success is often measured by the usefulness of an answer or discovery within a certain time period. However, modern science and research do not necessarily have a time schedule and outcomes are essentially unpredictable. Rules for avoiding dangers are given: the involvement of persons responsible for the research in setting of objectives; freedom of researchers to alter the course of their research; and requirements that reasons for changing the course of research be stated for each time period.

Pionke, H. B. and R. N. Weaver. "The Mahantango Watershed -- An Interdisciplinary Watershed Research Program in Pennsylvania." Northeast Watershed Research Center, Agricultural Research Service, USDA, University Park, Pennsylvania.

This program is an example of a cooperative effort between two regional research centers, the USDA, and the University of Pennsylvania and the Pennsylvania Agricultural Experiment Station. This paper describes the physical characteristics of the watershed and the interdisciplinary scope of the watershed research program. The watershed research programs of the Agricultural Research Service, the Northeast Watershed Research Center, and the Mahantango Creek Watershed are addressed separately. A conceptually based hydrologic simulation provides a research planning and design tool.

Presser, Stanley. (1980) "Collaboration and the Quality of Research." Social Studies of Science. 10, 95-101.

This article addresses the question of whether or not collaboration improves the quality of scientific research. Editorial decisions on papers submitted to a leading social psychology journal were cross-classified by a number of authors. A small relationship in the predicted direction was obtained, and it persisted in the face of two relevant controls. According to this study, collaboration tends to lead less to producing very good papers than to avoiding bad ones, but authors who work with others are more likely to write higher quality papers, regardless of discipline.

Raun, Earle S. (1978) "Engineer-Entomologist Cooperative Research Programs Needed to Solve Problems in Pest Management." Entomology Society of America Bulletin. 24:1, 70-71.

The necessity of the team approach in which the team is comprised of all those disciplines which could address insect control research is stressed. Suggestions are given for breaking down discipline-oriented barriers especially in the case of engineers and entomologists conducting collaborative research.

Ronningen, T. S. (1968) "Systems Research in Agriculture." USDA Annual Staff Conference, Alabama Agricultural Experiment Station, Auburn, Alabama, December 12.

Increasing specialization in the sciences and complexity of modern problems are noted as reasons for the need for systems research. The problems of coordinating specialists from many disciplines are identified. This paper suggests that systems research has all the characteristics of systems analysis plus the element of developing a new system or implementing new operations. In this way, systems research permits a holistic view of a problem, thus providing the most logical solutions. Different kinds of systems research and ideas for improving systems research are discussed.

Rossini, Frederick A., and Alan L. Porter. (1981) "Interdisciplinary Research: Performance and Policy Issues." Interstudy Bulletin. 2:5.

Interest in interdisciplinary research (IDR), based on societal need and intellectual concern seems to be growing. Successful IDR performance depends on such structural and process factors as leadership, team characteristics, study bounding, iteration, communication patterns, and epistemological factors, according to this paper. The necessity for appropriate frameworks for socially organizing the development of knowledge, such as common group learning, modeling, negotiation among experts, and integration by the leader, is addressed. On an institutional level, it is proposed that attention be given to successfully resolving issues such as appropriate organizational arrangements for IDR, rewards for its participants, and its effective utilization.

Roy, Rustam. (1977) "Interdisciplinary Science on Campus -- The Elusive Dream." Chem. and Eng. News. August 29, 28-40.

This essay defines and contrasts disciplinary and interdisciplinary research in the U.S. university setting. Materials science and engineering (which includes a variety of disciplines such as mathematics, physics, chemistry, geological engineering) are used as examples of interdisciplinary research areas. Trends in organizational structure are discussed with emphasis on the value of interdisciplinary research in addressing complex societal problems. A model and rationale for interdisciplinary units on campus shows how proper management, effective leadership, daily interaction, adequate funding, and satisfactory reward structures are necessary for successful interdisciplinary research.

Ruzic, Neil P. (1978) "Interdisciplinary Innovation is a Prominent Feature on the Island for Science." Industrial Research. February, 124-130.

This paper argues that interdisciplinary research and innovation are desirable because the more areas of experience that are accessible to thought, the better are the prospects for creativity and thus, more innovation. A semitropical island in the Bahamas provides an excellent location for interdisciplinary research in combination with development, prototype, testbed facilities, and proven techniques of others through the essentially free transfer of technology and the production of certain products such as shrimp, seaweed, and drugs from the sea. Innovations in marine pharmaceuticals, mariculture, phycology, solar desalination, wind power, and animal breeding are among the discoveries of the island project.

Schaller, Neill W. (1974) "An Appraisal of Potentials for Multidisciplinary Research Programs." Presented at annual meeting of Rural Sociological Society, Montreal, Canada, August 24, 1974.

Premises for justifying multidisciplinary research, including orientation of research to societal problems and a broader base of knowledge, are given in this paper. Improving the climate for multidisciplinary research to increase the benefits of participating in ventures, and then capitalizing on the improved climate to foster the effectiveness of multidisciplinary research, is emphasized. Proper funding, project management, administrative support and control, and other operational problems are addressed.

Lund, Roald. (1976) "Several Approaches to Management of Research Projects in Agriculture at North Dakota State University." Cooperative States Research Service (CSRS) Research Management Workshop, Washington D.C., May 5-6.

This booklet is a collection of letters, memorandums, and proposals from four interdisciplinary research (IDR) programs conducted by the North Dakota Agricultural Experiment Station. Several approaches to management of IDR are documented. According to this report, the approach to the development of each IDR project and to the final arrangement has evolved over time to produce the best approach to the problems addressed through IDR at the station.

Threadgill, E. Dale. (1981) "Interdisciplinary Research at the Coastal Plain Experiment Station, University of Georgia." Department of Agricultural Engineering, University of Georgia. (Mimeographed)

A successful interdisciplinary research program is described in terms of its administration, management, funding, leadership, team members, major disciplines involved, and change over time. Many researchers from several different disciplines are participating. This four-year-old project titled "Irrigated Multiple-Cropping Production Systems" owes its success to the willingness of the researchers to participate in the program, to compromise philosophies and to collectively manage themselves, according to this report.

Tweeten, Luther. (1978) "Multi-Disciplinary Research: An Economist's Perspective." Presented at the meeting of the Southern Association of Rural Sociologists, Houston, Texas, February 5-8.

This essay submits that institutional changes and greater interdisciplinary activity are precursors of successful multi-disciplinary research. Impediments to multi-disciplinary research such as institutional structure of departments and communication between researchers in physically separated disciplines are described. Using the social sciences as an example, areas for multi-disciplinary research from small farms to national development are suggested.

Vlachy, Jan. (1981) "The Measures of Interdisciplinarity in Research." Czechoslovak Academy of Sciences, Prague, June.

This paper uses quantifiable means to study the notion that the interdisciplinary approach has become an integral part of today's research and development. Growth rate, position in R&D budgets, manpower resources, distribution of scientists, publications, and impact on scientific productivity, are used to make a statistical comparison between interdisciplinary approaches and disciplinary research and development.

Williams, Anne S., G. A. Nielsen, H. F. Schovic, D. G. Stuart, and J. W. Reuss. (1978) "Guidelines for Conducting Interdisciplinary Applied Research in a University Setting." Public Data Use. 6:2, March, 3-13.

Recommendations are presented concerning the initiation, management, and integration of large-scale, interdisciplinary, applied research projects. A project to examine the environmental and social consequences of the proposed year-round resort development at Big Sky, Montana, was used as an example. A summary of the guidelines is included. By following the guidelines, it is argued that many of the universal failings of applied, interdisciplinary efforts can be avoided or alleviated.

Wilson, R. R. (1970) "My Fight Against Team Research." Daedalus. Fall 1076-1087.

This nuclear physicist describes his experiences with individual and team research from childhood to his present position as the director of a large government laboratory. The influences of World War II and the project to develop nuclear power, as well as the author's career in research at many universities are discussed. An opponent of team research for several years, the author ironically became the leader of one of the forefront research teams in high energy physics.

Wolman, M. Gordon. (1977) "Interdisciplinary Education: A Continuing Experiment." Science. 198, November 25, 800-804.

This paper argues that the rationale for interdisciplinary studies is based on the complexity of real world problems. Despite this rationale, interdisciplinary education faces difficulties involving philosophy, faculty, graduate students, curriculum, research, funding, and evaluation. The author draws some tentative conclusions from his experience: a vigorous program with an outstanding faculty can compete with specialized disciplines to attract quality students and faculty; a optimum number of faculty for an interdisciplinary effort is between ten and twenty; a proper reward structure for recognition, job advancement, and tenure is critical; a successful effort cannot be guaranteed, but failure can be caused by either an administration or faculty; and despite the difficulties, society's needs merit continued interdisciplinary educational efforts.

University of California. (1981) "Statewide IPM Project, Annual Report, 1980-81."

During its first two years of operation, the Statewide Integrated Pest Management Project has achieved significant progress in three key areas: development of "Decision Criteria" -- the information which growers need for determining sound pest management actions; development of information delivery systems; the actual delivery of useful information to growers and others. Through commodity work groups, researchers from the University of California, and California Department of Food and Agriculture were able to obtain funding for multidisciplinary research and accelerate IPM implementation.