United States
Department of
Agriculture

Forest Service

Northeastern
Station

NE-GTR-111

1986

Proceedings

IUFRO

Evaluation and Planning of Forestry Research

S6.06 - S6.06.01

Colorado State University
Fort Collins, Colorado 80523
July 25-26, 1985

IUFRO PROCEEDINGS

EVALUATION AND PLANNING OF FORESTRY RESEARCH

S6.06 - S6.06.01

Compiled by Denver P. Burns

Colorado State University

Fort Collins, Colorado 80523 USA

July 25-26, 1985

Sponsored by

INTERNATIONAL UNION OF FORESTRY RESEARCH ORGANIZATIONS

and

NORTHEASTERN FOREST EXPERIMENT STATION, USDA FOREST SERVICE

Papers are published in this proceedings in camera-ready form as submitted by the authors. The authors are responsible for the content of their papers.

PROGRAM

July 25, 1985

Introduction: Denver P. Burns, Director, Northeastern Forest Experiment Station, USDA Forest Service, Broomall, PA

SESSION I - RESEARCH FRONTIERS

Moderator: Dr. Eldon Ross

Research Frontiers.
W. Franklin Harris, Deputy Division Director, Biotic Systems and Resources, National Science Foundation, Washington, D.C.

SESSION II - RESEARCH FRONTIERS FOR FORESTRY

Applying Frontier Biotechnologies to Tree Improvement: Opportunities and Limitations.
F. Thomas Ledig, Project Leader, Institute of Forest Genetics, Pacific Southwest Forest and Range Experiment Station, USDA Forest Service, Berkeley, CA

Biotechnologies - The Potential Role of Somaclonal Variation in Forestry.
Darroll D. Skilling and Michael E. Ostry, Principal Plant Pathologists, North Central Forest Experiment Station, USDA Forest Service, St. Paul, MN

Frontiers in Wood Utilization Research in the United States.
John R. Erickson, Director, Forest Products Laboratory, USDA Forest Service, Madison, WI

Frontiers in Handling Wood.
C. R. Silversides, Forestry Consultant, Prescott, Ontario

SESSION III - IDENTIFYING RESEARCH NEEDS

Moderator: Dr. Denver P. Burns

Research Priorities and Advisory Committees in Canada.
Carl H. Winget, Director General, Canadian Forestry Service, Ottawa, Ontario

Comparison of Forestry Research Planning and Management in IUFRO Member Countries.
Hans Gregersen and David Bengston, Department of Forest Resources, University of Minnesota, St. Paul MN

SESSION IV - STRATEGIES TO MEET FORESTRY RESEARCH NEEDS

Industry-Federal-University Cooperation: Designing and Engineering Stringer Pallets.
William C. Baldwin, Executive Vice President, National Wooden Pallet and Container Association, Washington, D.C.

University-Forest Landowners Cooperation: The Regional Forest Nutrition Research Project.
S. P. Gessel and H. N. Chappell, Professor Emeritus and Research Associate, College of Forest Resources, University of Washington, Seattle, WA

The Fish/Forestry Interaction Program: A Case History of Cooperation in Canada.
V. A. Poulin, Principle, V. A. Poulin and Associates Ltd., Vancouver, B.C.; and D. R. Macdonald, Director General, Pacific and Yukon Region, Canadian Forestry Service, Victoria, B.C.

July 26, 1985

SESSION V - EVALUATING RESEARCH

Moderator: Dr. Roger Bay

Evaluating Forestry Research: An Overview.
Robert Z. Callaham, Program Coordinator, Wildland Resources Center, University of California, Berkeley, CA

A Brief History of Forestry Research Evaluation in the United States.
Allen L. Lundgren, Consultant, New Brighton, MN

SESSION VI - EVALUATING RESEARCH AT THE ORGANIZATIONAL LEVEL

Self Evaluation of Research Programs.
Arnett C. Mace, Jr., Director, School of Forest Resources and Conservation, University of Florida, Gainesville, FL

The Southern Industrial Forestry Research Council (SIFRC).
D. M. Crutchfield, Technical Assistant to the Forest Research Director, Westvaco Corporation, Summerville, SC

Publicly Supported Forestry Research an Overview of Review Mechanisms.
Robert E. Buckman and H. Gus Wahlgren, retired Deputy Chief for Research and Staff Assistant for Forest Products and Harvesting Research, USDA Forest Service, Washington, D.C.

SESSION VII - EVALUATING THE INDIVIDUAL'S RESEARCH AND PRODUCTIVITY

Moderator: Dr. Chris Risbrudt

Co-Authorship Patterns of USDA Forest Service Research Scientists at Two Regional Experiment Stations, 1981 - 1984.
Pamela J. Jakes, Research Forester, North Central Forest Experiment Station, St. Paul, MN, and Anne S. Fege, Staff Assistant, Timber Management Reseach, USDA Forest Service, Washington, D.C.

Evaluating Scientists in Canada.
J. H. Cayford and C. R. Sullivan, Regional Director and Program Director, Research and Technical Services, Canadian Forestry Service, Sault Ste. Marie, Ont.

SESSION VIII - EVALUATING PROGRAMS - THE CASE FOR RESEARCH

Evaluation of the Canada-United States Spruce Budworms Program--Organizational and Administrative Effectiveness.
Jack E. Coster, Director, Division of Forestry, West Virginia University, Morgantown, WV and Denis Lachance, Head, Forest Insect and Disease Survey, Canadian Forestry Service, St. Foy, Quebec

Forestry Research and Income Redistribution.
David N. Bengston and Hans M. Gregersen, Graduate Research Assistant and Professor, Department of Forest Resources, University of Minnesota, St. Paul, MN

Economic and Political Factors Influencing the Development of the Short Rotation Forestry Technology.
Anne S. Fege, Staff Assistant, Timber Management Research, USDA Forest Service, Washington, D.C.

Government Research and Economic Welfare Gains: The Case of Softwood Plywood.
Barry J. Seldon, Assistant Professor, Department of Economics, Ohio University, Athens, OH

July 26, 1985 (Continued)

Benefits and Costs of Containerized Forest Tree Seedling Research in the United States.

Robert A. Westgate, Regional Economist, Army Corps of Engineers, St. Paul, MN

Empirical Estimates to Gains from Public Forestry Research.

William F. Hyde, Associate Professor of Resource Economics, School of Forestry and Environmental Studies, Duke University, Durham, NC

DENVER P. BURNS

Introduction

Moving research management from an art to a science is a long slow process with limited progress but tremendous interest. Historically, forestry research, not having the dollars, glamour or intuitive appeal of medical or space research, has not been a part of the ferment about research management that has been a dominant issue in other disciplinary areas. Thus in the United States, amid falling budgets in the early 1980s when forestry research management was called upon to justify forestry research, not just in the context of forestry but rather within the context of the entire spectrum of research, many management presentations were not well received.

The 1970s saw the creation of a critical mass of scientists and especially economists interested in the development of analytical tools to evaluate Research. This work focused on the benefits of agricultural research and it stimulated interest in similar evaluations in the forestry sector. This forestry interest coincided with the sudden need of forestry research administrators to have or at least show they were very sensitive to the ultimate value of the research to the public good. Public funds were approved to conduct research on research evaluation. In addition to the application of quantitative techniques renewed effort was put into subjective forms of evaluation.

Forestry research management and research evaluation are addressed at the international level by S6.06 and 6.06.01 of the International Union of Forestry Research Organizations. Dr. John Sullivan and Professor Pierre Bouvarel, the leaders of the Group S6.06 (Research Management) polled their worldwide membership and confirmed research management and research evaluation to be two of the most important topics to the S6.06 membership. They then proposed a series of international meetings to present research results on management and to provide a forum for the exchange of experience and ideas. I was given the responsibility to plan and carry out such a meeting for the Western Hemisphere. Because of the mutual interests between forestry research management and forestry research evaluation I invited the membership of S6.06.01 through its leader Dr. R. Z. Callaham to participate in the meeting. The members of S6.06.01 (Research Evaluation) joined S6.06 to sponsor a two day meeting in Ft. Collins, Colorado, USA, on July 25-26, 1985, immediately prior to the annual meeting of the Society of American Foresters.

These Proceedings are the tangible result of the Ft. Collins meeting. The first day focused on research management with the first session devoted to speakers representing research organizations and topics outside of traditional forestry. The second session provided information on scientific frontiers that affect forestry research. The opportunity was to expose forestry research personnel to nontraditional views and sources of information and to emerging technologies affecting forestry research.

Sessions three and four completed the first day. These sessions included methods to identify new research needs and strategies to meet forestry research needs.

The second day was devoted to research evaluation in the forestry sector. The program covered four levels: the what and why of research evaluation; evaluation at the organizational level; evaluation of the individual researcher and finally evaluating programs and presenting case studies.

Approximately 60 people attended the sessions with interest and enthusiasm displayed throughout the meeting. Participants were from Canada, Mexico and the United States and observers from other countries were in attendance.

The participants and their organizations have my heartfelt thanks. They responded on short notice, kept the audience intently interested in their presentations and are responsible for the success of the meeting. Thanks are also due to the Rocky Mountain Forest Experiment Station and the Office of Conference Services of the Colorado State University. Ellen Balch was responsible for the meeting arrangements that assured all participants could concentrate on the topics.

RESEARCH FRONTIERS

W. Franklin Harris

Deputy Division Director
Biotic Systems and Resources
National Science Foundation
Washington, DC 20550

Abstract

The third quarter of this century has been referred to as the "electronics revolution." Many now see the remainder of the 20th century as a "biotechnological revolution" of similar magnitude and impact on economics and society. The bases for this forecast are the rapid advancement in molecular biology, and our understanding of genetics. These events certainly have significant ramifications for science administrators and research managers. Being sensitive to and nurturing new ideas in biotechnology is one aspect of our administrative challenges. Identifying true limits to research progress is another challenge. Perhaps the most vexing challenge to research administrators is guiding the broad frontiers of a scientific discipline to respond to, and take full advantage of, sharply accelerated progress in a subfield, such as biotechnology/molecular biology. Forestry is no exception to these challenges. For ecology, physiology, and evolutionary biology -- all important allied sciences to forestry -- the opportunities for significant advances are ripe if we, as administrators, marshall research resources creatively and seek a balanced approach to the biological revolution upon which we are embarking. These needs are discussed from the perspective and activities of the National Science Foundation.

Introduction

I am pleased to speak to you this morning as we begin this Conference on evaluation and planning of forestry research. My interests are two-fold. As a forest biologist, I look forward to hearing of the exciting opportunities in our science. As a research administrator, I am pleased -- yes, relieved -- to see us come together to talk about the problems and solutions -- perhaps akin to an artform in our technological milieu -- of research management. On the whole, there are few forums for this type of discussion, and the organizing committee and IUFRO are to be congratulated for providing this opportunity to share experiences and problems, and hopefully some solutions.

What I would like to do for the next little while is pose some questions about the conditions for scientific progress, raise some issues concerning how research breakthroughs occur, and share with you some directions we at NSF see in the future course of ecology, ecosystems studies, population biology and physiological ecology, and systematic biology. While not foresty *per se*, I see these subdisciplines as important foundations to the field of forestry.

Necessary Conditions For Progress

Scientific progress occurs in two ways. By far the more prevalent course is the slow, methodical, step-by-step gain in knowledge that comes with each well focused study. Scientific progress infrequently occurs by quantum leaps. Such leaps being presaged by a key research result, e.g., Watson and Crick's elucidation of the structure of DNA, development of sophisticated intrumentation, or response to societal pressure, such as the study of malaria. Predicting breakthroughs is impossible, so funding agencies and private interests must, instead, ensure that there is a research environment sufficient to support scientific progress. This environment has three essential qualities: 1) real funds with which to capitalize and operate a research enterprise; 2) a pool of diverse scientific talent; and 3) scientific questions ripe for explanation.

A bountiful supply of the above has to be a research administrator's dream, probably has never occurred and certainly has never been admitted to -- at least not to NSF or your dean. When we stir in the reality of resource limitations, research managers really begin to earn their keep. Perhaps only a squirrel foraging for nuts at the end of a second successive mast failure knows a truer meaning of "resource limitations".

This conference will explore frontiers in biology (biotechnology), processing wood fiber and uses of wood fiber. Life on the frontier always has been tough -- and the late twentieth century is no exception. Let me return to my three conditions for an environment conductive to successful research: 1) money, 2) people, and 3) ideas, and discuss these in the context of the biological revolution of which we are a part.

Money

The fiscal resources available to science are driven by factors other than the real and perceived needs of scientists. Overall economics and political priorities are central to these externalities. If there is one thing I can say with certainty it is that until the administration and Congress deal with the national deficit, there is little likelihood for real growth for scientific research. Witness Congress' move to freeze all domestic spending in FY 1986. Furthermore, planning environments are particularly flat. So while there will be funds for research, the limitations to them will become sharper and more a point of conflict for research managers.

One possible source of relief would be support from private sources. This certainly is the case for biotechnology where the chemical processes, pharmaceutical and food agriculture industries are major funding players. While the forestry industry is playing a role, I would suspect that their full role will not be realized until they fully recover from their own economic trials and tribulations. The private forestry sector will have to continue to play a significant role in support of forestry research -- it would be folly to expect otherwise.

In addition to a world of flat budgets, the research manager is facing sharply increased costs of research -- particularly with regard to cost of new, sophisticated equipment and its operation and maintenance. Participation in the molecular biosciences is truly an expensive proposition. Furthermore, many studies have pointed to the age of equipment in inventories of research institutions, NSF and other federal agencies have taken steps to begin its replacement. We expect to continue these efforts through purchase of new equipment as part of research project awards, awards for multi-user equipment, and special initiatives to refurbish and re-equip field labs such as we have done for marine labs and are planning to do for terrestrial and freshwater labs beginning in FY 1986. Other measures may have to be explored -- such as national instrumentation centers. This is not a move to be taken lightly, however. Purchase or loans of equipment clearly are a place where industry can help immediately, and I would envision new partnerships in the future. While I'm sure you've found my remarks about money more problematic than helpful, let me now turn to my second environmental factor, intellectual content of our research questions, because our challenges here are not unrelated to the money question.

Research Questions

Our intellectual environment is not limiting. However, this fortune is not without its problems. I can quickly classify three types of problems: 1) moving in new directions; 2) avoiding stagnation, and 3) the bandwagon syndrome.

New directions

Taking a group in a new direction is no easy task. It is the exception to see new groups develop overnight or change their modus operandi quickly. There usually is a period of transition. How one adds staff with the new expertise is one issue. However, any transition must be preceded by strategic and long range planning. Such planning often is the Achilles heel of research units. Our peers in the engineering and physical sciences can give us lessons on the value of strategic and long range planning -- probably they learned these lessons more easily because of their dependence on complex facilities, the construction of which is tied to planning. There clearly is a value to planning but like anything else, you can only get out of it what you put into it. Planning is most effective when it has a strong, bottom-up component. This assures the group is a full partner in the tough decisions that effective planning carries with it, and allows all (good) ideas to be heard. There are a variety of successful mechanisms to help move to new areas. These would include seed money funds or other uses of director's discretionary funds that allow development of preliminary data, purchase, rental, or other access to advanced equipment, extra travel, consultation, etc. These mechanisms work only when employed. Moving in new directions is not a time to become ultra-conservative when approached for such requests.

Avoiding stagnation

If I had the answer to how to avoid stagnation, I'd really be something! This is perhaps the biggest pitfall to the research manager. Face it, you can't beat success, and if I've been successful along a particular line of investigation, then there is a lot of resistance to change. The best and most talented groups fall into this trap -- two varieties of problems are the "third decimal place syndrome" and the "let's do it over on just a slightly different system." While I know of no easy solutions, I can safely say that it is easier to see this developing from the outside rather than from within. Time and resources invested in carefully structured and charged advisory committees are efforts well spent. We often make this suggestion to our grantees embarking on long-term complex projects.

The "bandwagon syndrome"

There is a legitimate role in science for duplication of research. Such legitimate duplication is not what I mean by the "bandwagon syndrome." Rather, I refer to the mad, blind rush to follow a fad. Of course, there is not a clearcut distinction where the dividing line occurs. There are certain symptoms that are disturbing. One that is currently prevalent is the abolishment of positions in traditional aspects of our science of biology -- for example, systematic biology and plant morphology and anatomy, similarly so for the zoological sciences. This is a short-sighted, totally self-serving point of view that we must not condone. I've actually had chairs of departments and search committees call to ask what are the "hot areas" -- where should recruiting efforts be directed. Of course, to these folks there really is no sense mentioning long range or strategic planning.

Disregard for the traditional disciplines is particularly shortsighted in the context of molecular biosciences and biotechnology. Reduced to its simplest terms, molecular biosciences and biotechnology simply provide a means to direct genetic processes toward precise endpoints -- to cause plants to do useful work.

Using plant systematics as an example, expertise here of the biological diversity of the plant kingdom is expertise on the raw material resource supporting new products for biotechnology. Similar arguments can be made for other traditional areas -- e.g., developmental morphology and anatomy.

Another problem with the "bandwagon syndrome" is that, as a whole, there is a tendency to reduce diversity of research approaches across labs and, in so doing, reduce the likelihood of finding new, equally fruitful approaches or breakthroughs that will significantly move the science. As a whole, does the research enterprise become short-sighted? I think it does, or at least it becomes much less efficient. We at NSF, both as program officers and our outside peer review community, frequently ask just how many groups need to be studying this or that phenomenon, or if awarded does this group or this principal investigator have a likelihood of contributing significantly to this problem? In our continually resource limited world, these questions will continue and probably become more prominent.

People

Research is people -- people exploring ideas. Ultimately, despite the most glamorous of labs, the research enterprise boils down to people. You say "OK if I shouldn't shift disciplinary makeup of a group without a lot of forethought, how do you propose I ensure that I have access to the appropriate scientific expertise to be competitive in today's (or tomorrow's) research environment?" Large groups, particularly in the engineering and physical sciences, can recruit and organize themselves in ways that facilitate bringing sufficent expertise to bear on a particular problem -- which might be of short duration. This approach is called a matrix organization, where projects and people comprise two axis of an organizational matrix. Projects buy people, people work on multiple projects. Such a system works, but is hard on managers and scientists alike, and few of us are dealing with research units of a size that lends itself to this approach.

There are two questions about people that we must address. First, are we training new scientists with the necessary skills for tomorrow's research? I say, not really. My concerns are the degree to which we are achieving the appropriate cross disciplinary training, and whether the promise and glamour, perhaps even intellectual promise, have led to certain disciplines being undersubscribed. With regard to cross disciplinary training, I question whether we are doing a good job and have a sufficient number of scientists in organismic biology at least conversant in, for example, molecular biosciences and tree physiology. The NSF has taken a positive and popular step with its Plant Sciences Postdoctoral Program. This program is designed to facilitate work by recent graduates in traditional plant physiology, for example, in labs strong in the molecular biosciences, or vice versa. In our Division, a new postdoctoral program affords a similar opportunity, and we are likely to strengthen this program toward this end in the future. Certainly, the technology of the molecular biosciences opens broad new vistas in areas of population biology, physiology, ecology and systematic biology, especially systematic relationships among microbe species. And yes, this does relate directly to the cost of doing research at the frontier.

Another concern is less easy to solve -- lack of sufficient topflight graduates in particular fields. I'll use one example that bears directly on forestry and ecology -- microbial ecology. The more I study ecology, the more I am impressed with the truly central role of microbial ecology. Yet, try to hire a microbial ecologist that meets our criteria for training. It's a competitive proposition. I think this problem will be self correcting once a market is apparent and a research funding environment looks promising.

Retraining established scientists can be a problem. Certainly, the cream of the crop forge their own opportunities. However, the traditional academic sabbatical may not be sufficient, or could even be more than needed, depending on the situation. Use of short-term visits, consultants and postdocs all are mechanisms that our grantees employ to move in a new direction -- especially for learning new methodologies. At this point, we at NSF do not have specific programs for established scientists to learn new techniques, but this clearly is an area we will continue to monitor.

Scientific Frontiers

Let me turn now from the research environment and problems confronting the research manager to the substance of the scientific frontier in forestry biology.

The frontier that we speak of is possible because of the advances in our understanding of the structure and function of genes and the technologies surrounding recombinant DNA. The pharmaceutical, biomedical and chemical process fields are well embarked on the biological revolution. The great undeveloped (economically) areas are agriculture, forestry, and other environmental applications. Based on our assessment of the field and its needs, we see particular requirements in the study of microorganisms. Although not an exclusive focus, enhanced support for microbial ecology is of particular importance because microbes are likely first applications for genetic engineering, and because microbes govern so many ecological and ecosystem-level processes. From the standpoint of assessing the risks of releasing genetically engineered organisms and predicting their ecological consequences, our knowledge of basic microbial ecology is critically lacking.

Several crucial areas requiring a concentration of effort are:

1) Processes of survival, growth, establishment and dispersal of microorganisms. Support is needed for characterization of nucleic acid sequences in nature, systematic classification of microorganisms, and detection of specific RNA and DNA in nature. This will be an expensive undertaking.

2) Population level processes pertaining to the interactions of modified microorganisms with native microflora. Current progress in processes such as nitrogen fixation, decomposition, etc., provide important background against which to make assessments of change and the consequences of introductions to native populations.

3) Potential vectoring of new genetic information to non-target microorganisms, and consequences of ecesis and interactions with native microflora -- the ecology of plasmids.

4) Development of monitoring protocols and management controls (e.g., self destruct sequences of nucleic acids).

5) System-level consequences of environmental introductions -- e.g., changes in essential ecosystems processes.

All of this likely will occur in a multidisciplinary environment. Certainly, my earlier concern for the availability of microbial ecologists is reemphasized by what I see as a central role for environmental microbiology in the future as regards both product development and product safety, as biotechnology moves to field and forest.

Biotechnology is just that -- a technology or suite of technologies by which we can manipulate plant, animal and biological processes generally to provide a useful endpoint or product. My premise here is that how far we push the biological frontier using these modern and continually evolving technologies depends on our fundamental knowledge of trees and their response to environment. It is appropriate that we ask just what we know about basic forest biology, and what we need to know.

A tenure at NSF is interesting for a lot of reasons. One that stands out is the opportunity to see the future knowledge unfold in the course of reviewing and administering research proposals and projects. What follows is my interpretation of some of the necessary (if not exciting) areas of forest biology -- answers to which we will chase well into the next century. I make no claim that I'm all inclusive. In fact, there is a distinct bias toward ecology, or at least a holistic approach to the forest tree system.

As we push back the limits of our understanding of forest trees, what we are going to find is that the tree is more a continuum of activity in its environment than we now understand, especially in the rhizosphere. Already it is impossible to speak of forest photosynthesis without full understanding of the continuum from the cellular architecture of the substomatal cavity, to the architecture of the canopy and the boundary layer above the forest. Right off the top, one factor that we have to be concerned with is the tree's photosynthetic performance. What will the gaseous environment be? The secular increase in atmosphere CO_2 will increase into the next century. Is this important? Has it been important? And to what extent can response to this variable be genetically manipulated? Similarly, the physiological response and genetic control of sensitivity to air pollutants, such as SO_x, NO_x and ozone, will continue to be important, particularly for urban forestry.

Knowledge and manipulation of the raw production of photosynthate is not enough. Some of my own work some years back on the carbon metabolism of a yellow poplar forest impressed me with the fact that net forest production, as bolewood, was no more that 10-15% of net photosynthesis. Modern wood technology using fiber from other tree parts certainly extends this value, but forests still expend a lot of photosynthate for other purposes. One frontier that we have to settle well is our understanding of how, by what rules or priorities, and under what controls -- genetic or environmental -- trees allocate photosynthate. There is a bit of knowledge for annual row crops -- virtually none for perennial crops, much less trees. There are several reasons for stressing this area of exploration: 1) obvious impact on yield if allocation could be manipulated without impact on other functions; 2) need to resolve rules of allocation to understand consequences of modest reductions at specific times of photosynthesis due to damage by oxidants; and 3) understanding allocation as it affects production of secondary chemicals by plants. This latter point bears on chemical products from trees as well as the chemical mechanisms by which trees influence their external environment, particularly its biotic components.

Having introduced secondary plant chemistry, it is appropriate to stress continuing the study of plant-animal (plant-pathogen) interactions for the future, for two purposes: 1) continued elucidation of specific interactions mediated by secondary plant chemistry that inhibit herbivores and pathogen invasion; and 2) understanding the genetic control of these interactions. This latter point is tremendously significant to possible biotech applications as we seek to engineer a tree to cope with a particular pest. I must add the cautionary note that as we add functions to the genone, we have to account for the costs of these functions. We may protect from herbivores, for example, a tree of reduced potential yield because of the imposed allocation.

Returning to phosynthesis, its rate and conversion of photosynthate to fiber or other metabolites is a function of an adequate supply of mineral elements as well as water. Forest soils are not the Class I soils so desired for row crops, and the nutrient use efficency of trees already is high. Mineral nutrition of trees is the area where I feel that the study of biotic interactions will prove significant.

The first such interaction already is well known to us -- the role of mycorrhiza. Much remains to be learned about factors controlling these mycorrhizal interactions. The biology of mycorrhiza is a fertile area for application of biotechnology. All areas of biology -- traditional and molecular -- have something to contribute.

You might find my second class of biotic interactions somewhat surprising -- the complex community of the forest rhizosphere -- invertebrates and microbes (bacteria and fungi). I stress their importance because it is this complex set of associations that regenerates the nutrient capital of forest soils (largely in detritus and soil organic matter) for reuse by forest trees. Soil biology is an area where our progress has slowed. I expect that we will find there are as well structured a set of ecological relationships and importance of secondary chemistry belowground as we are beginning to understand aboveground. In sum, the action is belowground!

These are a few of the exciting frontiers of biology. I don't mean to omit developmental biology, biochemistry, or genetics per se. I expect others at this conference will speak of these and other areas. I hope I convey a true sense of excitement for what the research of tomorrow will reveal.

In addition to the need for a predictable supply of funds for research and equipment I've already mentioned, there are additional needs of the research infrastructure to which we, as research managers. must be sensitive.

Field research is not a short term proposition. Vagaries of climate, natural developmental processes and anthropogenic influences all impact our forests on different time scales. The value of long term research cannot be overstressed. Programs in the U.S. such as those at Hubbard Brook, Fernow, Coweeta, and H. J. Andrews Experimental Forests, all attest to this. We're going to have to protect these investments and begin new ones.

Biological diversity is the raw material of man's future. We hear of the need to protect it in the tropics, certainly, but similar concerns for our temperate germplasm resources are important as well. Biological diversity for higher plants is important, but not more so than for microbes and soil invertebrates.

Finally, as trees do not exist apart from their immediate environment, neither does a forest not bear a relationship to the total landscape. Eventually we are going to need to understand our biosphere as part of a global system, just as we understand a tree to be interactive with its immediate physical and biotic environment. To do this will require new liasions with disparate disciplines and new thinking. The intellectual horizons are limitless, and the need for biologists (foresters) to push on is great.

We, as research managers, have a lot of work to do -- to get the wagons across the frontier. At the risk of appearing maudlin, I sense the excitement that earlier pioneers must have felt, and look forward to seeing the ridge that lies beyond the ridge.

Applying Frontier Biotechnologies to Tree Improvement: Opportunities and Limitations

F. Thomas Ledig

Project Leader, Institute of Forest Genetics, Pacific Southwest Forest and Range Experiment Station, U.S.D.A. Forest Service, Berkeley, CA 94701

Abstract

Genetic engineering will reduce the time required for multi-trait breeding in forest trees and overcome barriers to gene exchange; screening in cell culture will multiply selection pressures many fold; and cloning trees from tissue culture will generate improved stock for research and commerce. The new capabilities to produce directed change will not replace traditional breeding methods; rather, biotechnology must be supported with conventional programs for field testing, cross-breeding, and gene conservation. Present opportunities for genetic engineering are the transfer of genes from bacteria to produce toxins against lepidopteran pests, or osmoprotectants that confer drought resistance and salt tolerance. Future applications depend on detailed knowledge of the biochemical and physiological mechanisms that control growth, pest resistance, stress tolerance, and other traits of practical importance. The new biotechnologies are not well adapted for manipulating traits that are ill-defined or that depend on the action of many genes, scattered throughout the chromosomes. Although genetic engineering has prospered with venture capital in the pharmaceutical and chemical industries, payoffs in forestry are, at present, too uncertain and too distant to attract private enterprise. The task of pioneering the new tools in forest trees will most likely be carried out by government and universities.

Introduction

Research administrators and planners soon will be challenged to develop programs in biotechnology. Inevitably, decisions on research allocation will be difficult, more so because of the highly technical nature of the field. In this paper I have suggested some areas of especial promise, indicated the limitations of forest biotechnology, outlined present research efforts in the field, and provided background for decisions on research support in forest biology.

In the last decade, biology has developed an array of new tools and applications. These "biotechnologies" include the ability to produce highly specific molecular antibodies, to read the genetic code, to synthesize genes, and even to genetically engineer new organisms by the transfer of genes among highly diverse taxa. As a result, biological investigation has been greatly accelerated and entirely new industries created. The earliest payoffs have come from the production of diagnostics and valuable biochemicals. Some hormones, such as insulin and interferon, can be produced more cheaply by genetically transformed microbes than they can be synthesized or extracted from natural sources.

Agricultural applications may not develop as rapidly as pharmaceutical because the entrepreneurial incentives are, in part, lacking. Development of new crop plants or their symbionts, even by genetic engineering, requires years of field-testing before the product can be marketed. Advocate groups will slow the process even further by litigation that prevents the release of "engineered" organisms because of possible environmental consequences. Fledgling companies, founded on venture capital, must target more immediately marketable products. Established agrochemical companies will concentrate on engineering characteristics such as glyphosate resistance in crop plants, to increase sales of one of their major products, herbicides. Other applications to agriculture and forestry will develop more slowly and depend to a large degree on publicly-funded research. Therefore, university and government research planners must become familiar with what the new biotechnologies can do, and what they can not do.

What the New Biotechnologies Can Do in Forestry

Increase Selection Intensity

Crop improvement by selective breeding depends on the intensity of selection; the higher the intensity, the greater the improvement. In annual crops, thousands of individuals can be compared in a single test that takes only a few acres. By contrast, mature trees occupy large areas so that correspondingly fewer individuals can be evaluated for the same cost.

But, the new biotechnologies operate at the molecular and cellular level, and cells of forest trees take no more space than cells of corn. At the cellular level, similar selection intensities can be generated for trees and agricultural crop plants, with no difference in cost.

The types of characteristics that can be selected in cell culture are limited, at present, to stress tolerance or the production of specific chemicals. The stresses may be physical or they may be biologic. Stress and pathogen resistance are simple to select, in principle. The cell culture is challenged by stress levels sufficient to eliminate most of the cells. Those that remain are allowed to multiply and, eventually, caused to regenerate into plants. One example was the selection of herbicide resistance in tobacco (Nicotiana tabacum L.) cell cultures by Chaleff and Ray (1984). Cellular selection for tolerance of heat shock, high salinity, toxic levels of metals, or minimal nutrient requirements are other possibilities. Likewise, resistance to certain diseases, particularly those that produce injury by toxins, may be screened in cell or tissue cultures; major gene resistance of sugar pine (Pinus lambertiana Dougl.) to the white pine blister rust fungus (Cronartium ribicola Fisher) can be detected in callus culture (Diner, Mott, and Amerson 1984). The North Central Forest Experiment Station plans to select for resistance to septoria canker (Septoria musiva Pk.) in poplar (Populus spp. L.) cell cultures (Nelson 1985).

Selection for production of valuable chemicals may be facilitated by cell culture. Many compounds can be easily detected and quantified in cell culture due to the absence of significant amounts of pigments and inhibitors (Balandrin et al. 1985). Furthermore, proper manipulation of culture conditions may enhance production, and, therefore, the ability to detect secondary compounds.

Reduce the Time Required to Breed New Lines

A long pre-reproductive phase and a long crop rotation both conspire against rapid progress in tree breeding, and restrict improvement per unit time. Breeders of annual crops can actually select and cross-breed three generations a year, with the help of greenhouse facilities. Most conifer species would require at least five years to turn over one sexual generation (Righter 1939). Even the time between pollination and seed production can be painfully long in some trees, about 18 months in pines (Pinus spp. L.). And it may require decades to adequately field test progeny for growth, form, or disease resistance.

The ability to transfer genes directly from one tree to another, or from some other organism to a tree species, would result in rapid progress in forestry. Using recombinant DNA technology, the sexual cycle can be bypassed, opening new possibilities for tree improvement (Ledig and Sederoff 1985). For example, in pest resistance, a line incorporating several resistance genes (broad-based resistance) is preferable to a line with only a single type of resistance, but in forestry it may take centuries to breed multi-genic resistance by traditional cross-breeding. With gene transfer, breeding for multiple resistance genes, or multiple-trait breeding in general, becomes a practical possibility.

One deterrent to the use of gene transfer is a paucity of known single-gene traits (Sederoff and Ledig 1985). However, several bacterial genes are available that may have potential in forest trees. Genes that control the production of toxins to insect pests and others that confer drought and salinity tolerance and herbicide resistance have been isolated and cloned.

Overcome Barriers to Hybridization

Traditional breeding is often stymied by reproductive barriers between species. The major gene in sugar pine that provides resistance to white pine blister rust (Kinloch, Parks, and Fowler 1970) can not be transferred to the susceptible eastern or western white pines (Pinus strobus L. and P. monticola Dougl. ex D. Don) because they do not cross with sugar pine. Gene transfer would bypass the reproductive barrier.

A tool that combines the entire genetic complements of two species, is cell fusion. Cells are first stripped of their walls with cellulases, and become naked protoplasts. By proper adjustment of the culture medium with polethylene glycol or by altering the electrical field, protoplasts can be made to fuse. To avoid unbalancing the chromosome numbers, the initial cells must be haploid, like eggs or sperm. One of the first accomplishments of biotechnology was just such a parasexual hybridization of two species of tobacco (Nicotiana glauca Grah. and N. langsdorffii Weinm.; Carlson, Smith, and Dearing 1972). Another way of combining the genes of two species is to create a simple or complex mixture of two cell types, a chimera. An animal with a mixture of goat and sheep cells has been produced in this way (Fehilly, Willadsen, and Tucker 1984, Meinecke-Tillman and Meinecke 1984). Hybridization per se is unlikely to produce an improved tree, but may provide a variable base population for further breeding efforts.

Perhaps more importantly, cell fusion may provide a rapid means of following the inbreeding-hybrid scenario pioneered by maize (Zea mays L.) breeders. Inbreeding weeds out individuals carrying major deleterious genes, and a cross between different inbred lines results in masking the effects of any remaining, weakly deleterious genes by hybrid vigor. The inbreeding-hybrid scenario is currently impractical in forestry because several generations are required to produce a true-breeding line. However, selection of viable haploid cell lines derived from conifer gametes, egg or pollen cells, followed by their fusion, could produce hybrid vigor in a few cell generations -- months instead of years.

Multiply Superior Trees

The production of multiple copies of a single individual by cloning caught the public imagination when it became possible to clone certain animals. The sensationalism of such fictional accounts as "The Boys from Brazil" helped popularize cloning, but gardeners were cloning plants centuries ago by grafting or by rooting cuttings (Marston 1955). Cloning captures all the genetic qualities of the model, not just a portion, as in selective breeding. Techniques for the mass propagation of superior trees for commercial planting could be an economic boon (Libby and Rauter 1984), but not without dangers. Plantings of single clones may be more susceptible to pests and natural catastrophes than genetically diverse plantings (U.S. Committee on Genetic Vulnerability of Major Crops 1972); mixtures of clones will be no great risk but gains will probably be less than the maximum attainable with the single best clone. A protocol for cloning superior trees would be a benefit even if it fell short of a commercial scale, if sufficient material could be produced for testing and research, providing for uniform replicates. In fact, most forest tree species can be propagated vegetatively, but usually at considerably higher cost than through seed propagation.

Cloning from cell culture is necessary to apply all of the biotechnologies mentioned above. Mutations or transformations occur in single cells. Therefore, the application of cell selection, genetic transformation, and protoplast fusion all require regeneration of trees from single cells, which has proven a very difficult hurdle. Regeneration from single cells has been achieved only for a few species of angiosperms, and no reports of success have been published in conifers (Karnosky 1981). Unpublished reports for larch (*Larix decidua* L.; J. Bonga, personal communication) and Norway spruce (*Picea abies* [L.] Karst.; K.-E. Erikkson, personal communication) offer glimmers of hope that the problems are not insoluble.

What the New Biotechnologies Can Not Do

Breed for Multiple-gene Traits

Genetic transformation is not a practical alternative to traditional breeding for traits that are ill-defined, or whose controls are poorly understood. Neither is it feasible when physiological control depends on many genes scattered throughout the chromosomes; isolation of genes is not simple, particularly if their primary products are unknown. However, blind selection of somaclonal variants may be an effective technique even when knowledge of control mechanisms is lacking.

In general, traditional breeding methods will continue to be most important for traits whose control is imperfectly known, those known as "quantitative traits". Gene transformation can be used to insert single genes or small blocks of genes, but it is not practical to think of characteristics under multigenic control; in other words, many of the traits in which breeders have traditionally been interested. For example, growth is influenced by a host of component traits, such as photosynthesis and respiration, water and mineral uptake and use. Each of these is itself a compound trait, its expression dependent on many genes.

Nitrogen fixation was considered a potential candidate for genetic engineering, relatively recently. According to one line of reasoning, if corn or conifers could fix atmospheric nitrogen, like the bacteria associated with legumes, great increases could be achieved in productivity, with a simultaneous reduction in the cost of fertilizers. However, at least 17 genes are necessary for nitrogen fixation in procaryotes and other genes may be necessary to provide the proper cellular environment (Postgate 1982). It may be a difficult task to move the entire complex into a higher plant, and there would be no guarantee that the sequence would function properly when transferred; it has not functioned in yeast, a eucaryote. In addition, it now seems clear that energy requirements for nitrogen fixation are high (Ryle *et al*. 1984). For the present, it is probably cheaper to use fossil fuel in the production and application of nitrogenous fertilizer than to increase the metabolic cost to the plant, which might reduce yields.

Evaluate Field Performance

Genetic engineering by transformation, cell fusion, or cell selection does not eliminate the need for field testing. Even directed change, such as transformation, may have unexpected consequences. Insertion of a foreign gene may interfere with some other function in the host, either by disruption of a gene in the host or by disruption of the host's metabolism at some point removed from the genes. Finally, the engineered plant may simply not perform as anticipated: e.g., an inserted gene may not express, its product may be in too low a concentration, or it may be ineffective for any number of reasons. Genetic engineering must be supported by a traditional testing and breeding program.

Provide for Gene Conservation

The new biotechnologies can not eliminate the need for the traditional sources of genetic variation, gene banks and wild relatives. Any DNA sequence *can* be synthesized, but it is difficult to imagine de novo *what* should be synthesized to fill a particular need. If there had never been any flying insects, birds, or mammals, how long would it have taken to conceive of flight?

Species preservation and gene conservation take on added importance now that recombinant DNA technology can be used to isolate genes from bacteria, fungi, annual plants, or even animals, and insert them in trees. Properly evaluated and conserved gene resource collections are the raw material for genetic engineering.

What the Immediate Opportunities Are for Biotechnology in Forestry

The greatest single opportunity for forestry research is the development of protocols for the regeneration of trees from cell and callus culture. Mass propagation of superior trees would be of immediate economic benefit, as well as the final link in genetic engineering of forest trees (Ledig and Sederoff 1985). Insertion of genes by transformation or selection in cell culture occurs at the level of the single cell; it must be possible to regenerate trees from transformed and selected cells to realize any practical values from biotechnology.

When the problem of regeneration is solved, the greatest opportunities for genetic engineering will be by transformation with bacterial genes. The insecticidal bacterium (*Bacillus thuringiensis*) produces a toxin to lepidoptera, such as the gypsy moth. The main difficulty in using the bacterium for insect control is delivering it in quantity to the target. However, the toxin gene itself has been isolated and cloned, and could be inserted into conifers using the crown gall bacterium (Agrobacterium tumefaciens) developed by Sederoff et al. (in preparation). If produced in trees, the toxin might be useful in the control of gypsy moth, pine tip moths, Douglas-fir tussock moth, spruce budworm, and many other important lepidopteran pests. Other opportunities for genetic engineering are the bacterial genes that control the osmoticum (Le Rudulier et al. 1984) and confer drought resistance and salt resistance. Genes for herbicide resistance are also available from bacteria.

Few single genes are identified in trees, and fewer still have practical significance for genetic engineering (Sederoff and Ledig 1985). Disease resistance genes have the most potential. The major gene in sugar pine for resistance to white pine blister rust was mentioned above. However, the resistance gene would be very difficult to isolate because the gene product is not known, and no tightly linked loci have been identified. Conifers are characterized by a high nuclear DNA content (Dhillon 1980), making it especially difficult to fish-out single genes. Nevertheless, genes for disease resistance offer a real opportunity for genetic engineering; transfer into susceptible species or into susceptible lines selected for growth and form would provide major benefits.

Who Will Do Biotechnology Research in Forestry

Research on forest biotechnology conceivably could be centered in the biotech industry, the forest industry, the universities, or the U.S.D.A. Forest Service. Over 200 companies specializing in some area of biotechnology have been established in the last five years in the United States (Board on Agriculture, National Research Council 1984). Many of these will fail before they ever market a product, according to some predictions. Few of these companies are working on problems directly applicable to forest trees. Those that are, are working under contract from government or the forest industry, primarily in the area of vegetative propagation, or cloning, and cell selection. Native Plants, Inc. was working on clonal propagation of alder (Alnus rubra Bong.) under contract from Crown-Zellerbach, and L. William Teweles and Co. had cooperative research agreements from the Forest Service, North Central Forest Experiment Station. Very few of the biotech companies are working on the basic problems of gene insertion, control, or expression in trees. One of the exceptions is Calgene, which is working with both the North Central Forest Experiment Station and the Institute of Forest Genetics on insertion of an herbicide resistance gene in poplar and in conifers.

The forest industry does support some in-house research in genetic engineering (e.g., International Paper Company is looking at gene transfer systems in conifers) and some contract research (e.g., WestVaCo supports a position at the Institute of Paper Chemistry). Both the National Forest Products Association and the Technical Association of Pulp and Paper Industries (TAPPI) have established subcommittees for reviews of forest biotechnology research, and would readily transfer technology of practical significance.

Neither the biotech industry nor forest industry is in the position to research applications of the new biotechnologies in forestry. Application of any of the new biotechnologies (genetic engineering, cloning, and cell selection) to forest trees is so new, it must be considered a high risk venture. Except for micropropagation of poplars by Native Plants, Inc. and eucalypts (Eucalyptus spp. L'Her.) by Biomass Energy Systems, no commercial successes have been realized in forestry in the United States and, in fact, in many areas there is little research to guide efforts. The closest guides are research programs on horticultural problems, such as propagation of the oil palm (Elaeis guineensis Jacquin). It took a decade of research to develop a commercially applicable protocol for the cloning of oil palm (Branton and Blake 1983). The forest industry has also expended substantial effort on vegetative propagation, because of the potential for rapid commercialization. AFOCEL, the French industrial cooperative, developed successful techniques for mass propagation of eucalypts (Chaperon 1983), and elsewhere in Europe, Norway spruce is propagated by cuttings (e.g., Kleinschmit and Schmidt 1977). But research on conifer regeneration from cell culture is rare. Industry leaders in the United States have been badly discouraged by the seemingly intractable nature of the problem, and the recent business climate has forced cutbacks in their research efforts.

For genetic engineering of forest trees, it is likely that government and universities must take the lead. Some biotech companies may be encouraged to work on defined problems in forest trees under contract arrangements. But, very simply, the basic research on which to build applied programs is lacking, the time to develop and evaluate products is too long in trees, and the value of the product too low to entice private enterprise until there is a clearer indication of success.

Universities have exercised little leadership in forest biotechnology, with the exception of tissue and cell culture. Recently, forestry schools in the United States and Canada have attempted to develop ties with departments of microbiology and molecular genetics. However, dialogue is difficult because most forest biologists are not conversant with recent advances in genetics and few established scientists in molecular genetics are willing to abandon their model systems to work on trees. In recruiting for new positions to fill the void in biotechnology, forestry schools have been forced to hire the relatively untrained and inexperienced, apparently with the hope that they can grow in the field.

For at least once, government has taken the lead, by a small margin. In mid-1983 Congress appropriated funds for a U.S.D.A. Forest Service program in biotechnology. And in the next year the influential National Agricultural Research and Extension Users Advisory Board (1984) recommended further funding in forest biotechnology, reversing a prior recommendation that all biotechnology research be housed in the Agricultural Research Service (ARS), to be supported in small part by levees against the Forest Service research budget. Of course, forestry could expect little research directly applicable to its problems if it was conducted by the ARS; agricultural scientists would work on the best system available to attack basic research problems, and that would not be trees. The precedent is obvious: few scientists in ARS or in university botany departments have ever chosen to work on research with direct application to forestry.

Forest biotechnology in the Forest Service is concentrated in two programs: a multiproject program at the North Central Forest Experiment Station located at Rhinelander and Madison, Wisconsin and St. Paul, Minnesota and as part of the research program of the Institute of Forest Genetics at the Pacific Southwest Forest and Range Experiment Station located at Berkeley and Placerville, California. The two programs have diverged in complementary ways. The Institute of Forest Genetics has focused its program on genetic engineering — applying recombinant DNA technology. Because it is a national program, effort is not restricted to local species or problems, but is focused on major conifers, loblolly pine (*Pinus taeda* L.), Douglas-fir (*Pseudotsuga menziesii* [Mirb.] Franco), and sugar pine. The biotechnology program at the North Central Forest Experiment Station has channeled its efforts into the use of somaclonal variants. Herbicide resistance will be selected in cell and tissue cultures of poplar, and septoria canker resistance will be selected in poplar cultures. Systems for the selection of disease-resistant hard pines are also under investigation. Both programs recognize the need to regenerate trees from microculture and are either conducting in-house research or supporting collaborative projects with universities and with biotech industries.

Recruiting trained scientists in the new biology (e.g., molecular genetics) is a problem for the Forest Service and for forestry schools, because the schools have not been producing forest biotechnologists. Because of the failure to expand forestry research during the last decade, most forest biologists on university faculties, in forest industries, and in government completed their graduate education before the current revolution in genetics. The new research tools, particularly those used in recombinant DNA laboratories, are unfamiliar, and forestry faculty are not in a position to encourage their students to use them. Short courses may help to remedy the situation. To provide training opportunities for forest biologists and to encourage research in forest biotechnology, the Institute of Forest Genetics plans summer courses in recombinant DNA and tissue culture technologies.

The Institute of Forest Genetics was fortunate in finding a molecular geneticist to direct its biotechnology program. The senior scientist is now supported by two professional positions, and will be supported by at least two post-doctoral positions. The program has attracted other faculty on sabbatical, and one is currently in residence. Furthermore, it draws on ongoing biotechnology research on inheritance and linkage of genes that direct the synthesis of soluble enzymes (Conkle 1981), on inheritance of monoterpenes (Strauss and Critchfield 1982), and on disease resistance mechanisms in conifers (Kinloch 1982). The Biotechnology Program at the North Central Forest Experiment Station draws on strength in plant physiology and tissue culture, devoting a total of nearly four scientist-years to biotechnology.

In developing its biotechnology program, the Forest Service can draw on expertise in private industry as well as universities through the use of cooperative agreements. According to a 1981 law, small businesses and universities can secure patents for discoveries made under federal contract, providing an extra incentive to entertain cooperative research agreements. Both Forest Service biotechnology programs have established cooperative agreements for the conduct of research. The Institute of Forest Genetics is currently supporting three separate studies: microinjection as a gene transfer system in conifers at the University of California, Berkeley; cell culture and regeneration of Douglas-fir and sugar pine at the University of California, Davis; and DNA fragment polymorphisms as genetic markers at Washington University, St. Louis. The Biotechnology Program at the North Central Forest Experiment Station supports studies on: shoot and protoplast culture in poplars at the University of Wisconsin; tissue culture and somaclonal selection for resistance to septoria canker of poplars at the University of Minnesota; somaclonal selection for resistance to scleroderris disease (*Gremmeniella abietina* [Lagerb.] Morelet) in a model system, larches (*Larix* spp. Mill.), at Michigan Technological University; cotyledon culture and somaclonal variation in red pine (*Pinus resinosa* Ait.) at the University of New Hampshire; and characterization of poplar DNA at North Carolina State University. Both programs are working with Calgene, Inc. in Davis, California on transformation of tree species with an herbicide resistance gene, using as vectors, microinjection in poplars and the crown gall bacterium in pines.

Because of the cost, research in forest biotechnology should be concentrated in a few centers and use the existing expertise and facilities in agriculture and medicine. Labor intensive procedures and sophisticated laboratory equipment drive up the cost of biotechnology research. As examples of the scale of investment by industry: Monsanto Corporation recently opened a life sciences research center at the cost of $150 million and has a $400 million annual research budget (Freeman 1985); Genentech, the most highly publicized biotechnology company, employs about 650, at least 300 directly in research (Cass, Graf, and Sterling 1985); and in Japan, 14 major firms engage in biotechnology research, each with an annual budget of over $200 million (Board on Agriculture, National Research

Council 1984). Costs are high, however, multi-million dollar budgets are not necessary if other resources can be tapped.

To play a role in biotechnology, forestry schools must build bridges to other departments. Smaller forestry schools and departments, not associated with major medical centers having programs in biotechnology, should consider consortia among universities to ensure educational opportunities, at least, for their students. Models exist in other disciplines, such as the association between universities in the Northeast and the Marine Biological Laboratory at Woods Hole, Massachusetts or the radiation facilities at the Brookhaven National Laboratory, New York. These laboratories provide lecturers for courses on a number of campuses and access to specialized equipment for graduate student and faculty research.

Resources, money, and talent are generally too limited in forestry for each organization to support every worthwhile academic initiative. In the last two decades, many American universities found they could not afford to be multi-versities, and wisely concentrated on their strengths. Likewise, forestry schools should not spread their research budgets too thinly, although they should ensure educational opportunities in the broad spectrum of technical and social disciplines, including the new biology and its applications to forestry. By creatively combining resources within and among universities, federal laboratories, forest industry, and the biotech industry, forestry can profit from the biotechnology revolution without a massive influx of new funding and without gutting existing programs.

Conclusion

The insertion of new genes into forest trees or the selection for stress resistance in cell cultures will not make traditional approaches to tree improvement obsolete. Forest biotechnology must be supported by a program of field testing and cross-breeding. Many characteristics do not lend themselves to improvement by the new biotechnologies, and genetic engineering should be applied to improve the best that conventional breeding strategies have produced. Biotechnology will have its greatest impact when it can draw on an array of tested materials. A program of gene conservation, with its attendant collections of germplasm, provides the necessary raw material for genetic engineering. Collections of conifers, such as those in the arboreta and test plots of the Institute of Forest Genetics, and selected poplar clones, such as those available at Rhinelander, are vital to progress in biotechnology. An active research program in molecular genetics requires substantial funding, and administrators should be wary lest the genetic engineering bandwagon steamroll traditional programs of tree improvement and gene conservation. Forest biology would be the loser.

Because of the high cost combined with perennially limited resources, any new research program in biotechnology should be carefully considered. Progress in biotechnology in agriculture and, especially, in the medical sciences required the annual input of multi-millions of dollars for at least a decade. Fortunately, forest biotechnology will profit by progress in sister sciences; genes isolated from bacteria may have utility in trees. New programs should be tied to existing departments of molecular biology and microbiology. One or two lone research positions, operating in isolation in a forestry school, will be relatively ineffective. Even the most well-endowed schools of forestry should consider consortia, combining resources of several universities and agencies to support centers for research in forest biology. The new biotechnologies offer spectacular opportunities for improving forestry, but they will not fulfill their promise without careful planning.

Acknowledgements

I am grateful to Neil Nelson for providing information on the Biotechnology Program at the North Central Forest Experiment Station and for reviewing a draft of this manuscript.

Literature Cited

Balandrin, M.F., J.A. Klocke, E.S. Wurtele, and W.H. Bollinger. 1985. Natural plant chemicals: sources of industrial and medicinal materials. Science 228: 1154-1160.

Board on Agriculture, National Research Council. 1984. Genetic engineering of plants: agricultural research opportunities and policy concerns. Nat. Acad. Press, Wash., D.C. 83pp.

Branton, R. and J. Blake. 1983. A lovely clone of coconuts. New Sci. 98: 554-557.

Carlson, P.S., H.H. Smith, and R.D. Dearing. 1972. Parasexual interspecific plant hybridization. Proc. Nat. Acad. Sci. 69: 2292-2294.

Cass, M., J.S. Graf, and J. Sterling. 1985. Genentech: leading the way to biotechnology's pot of gold. Genet. Eng. News 5(1): 8-10, 47.

Chaleff, R.S. and T.B. Ray. 1984. Herbicide-resistant mutants from tobacco cell cultures. Science 223: 1148-1151.

Chaperon, H. 1983. Clonal propagation of *Eucalyptus* by cuttings in France. In: R.B. Standiford and F.T. Ledig (tech. coords.), Proc. Workshop on *Eucalyptus* in California, June 14-16, 1983, Sacramento, Calif. U.S.D.A. For. Serv. Gen. Tech. Rep. PSW-69: 108-114.

Conkle, M.T. 1981. Isozyme variation and linkage in six conifer species. In: M.T. Conkle (tech. coord.), Proc. Symp. Isozymes No. Am. For. Trees and For. Insects, July 27, 1979, Berkeley, Calif. U.S.D.A. For. Serv. Gen. Tech. Rep. PSW-48: 11-18.

Dhillon, S.S. 1980. Nuclear volume, chromosome size and DNA content in three species of *Pinus*. Cytologia 45: 555-560.

Diner, A.M., R.L. Mott, and H.V. Amerson. 1984. Cultured cells of white pine show genetic resistance to axenic blister rust hyphae. Science 224: 407-408.

Fehilly, C.B., S.M. Willadsen, and E.M. Tucker. 1984. Interspecific chimaerism between sheep and goat. Nature 307: 634-636.

Freeman, K. 1985. Monsanto opens $150 million life sciences research center. Genet. Eng. News 5(1): 20.

Karnosky, D.F. 1981. Potential for forest tree improvement via tissue culture. BioScience 31: 113-120.

Kinloch, B.B. 1982. Mechanisms and inheritance of rust resistance in conifers. In: H.M. Heybroek, B.R. Stephan, and K. von Weissenberg (eds.), Resistance to diseases and pests in forest trees. Proc. Third Intern. Workshop Genet. Host-Parasite Interaction For., Wageningen, Neth., 14-21 Sept. 1980. Cent. Agric. Publ. Docum., Wageningen, Neth.: 119-129.

Kinloch, B.B., G.K. Parks, and C.W. Fowler. 1970. White pine blister rust: simply inherited resistance in sugar pine. Science 167: 193-195.

Kleinschmit, J. and J. Schmidt. 1977. Experiences with *Picea abies* cuttings propagation in Germany and problems connected with large scale application. Silvae Genet. 26: 197-203.

Ledig, F.T. and R.R. Sederoff. 1985. Genetic engineering in forest trees. In: Proc. Eighteenth Southern For. Tree Improv. Conf., Long Beach, Mississippi: 4-13.

Le Rudulier, D., A.R. Strom, A.M. Dandekar, L.T. Smith, and R.C. Valentine. 1984. Molecular biology of osmoregulation. Science 224: 1064-1068.

Libby, W.J. and R.M. Rauter. 1984. Advantages of clonal forestry. For. Chron. 60: 145-149.

Marston, M.E. 1955. The history of vegetative propagation. Fourteenth Internat. Hort. Congr.-1955: Rep., vol. 2. Veenman and Zonen, Wageningen, Neth.: 1157-1164.

Meinecke-Tillmann, S. and B. Meinecke. 1984. Experimental chimaeras - removal of reproductive barrier between sheep and goat. Nature 307: 637-638.

National Agricultural Research and Extension Users Advisory Board. 1984. New directions for science, education, and agriculture: an appraisal of the administration's proposed budget for 1985. February 17, 1984. Wash., D.C. 66 pp.

Nelson, N.D. 1985. North Central Forest Experiment Station Biotechnology Program -- applications to tree improvement. In: Proc. Eighteenth Southern For. Tree Improv. Conf., Long Beach, Miss.: 14-22.

Postgate, J.R. 1982. Biological nitrogen fixation: fundamentals. Phil. Trans. Roy. Soc. Lond., B 296: 375-385.

Righter, F.I. 1939. Early flower production among the pines. J. For. 37: 935-938.

Ryle, G.J.A., R.A. Arnott, C.E. Powell, and A.J. Gordon. 1984. N_2 fixation and the respiratory costs of nodules, nitrogenase activity, and nodule growth and maintenance in Fiskeby soyabean. J. Exp. Bot. 35: 1156-1165.

Sederoff, R.R. and F.T. Ledig. 1985. Increasing forest productivity and value through biotechnology. In: Forest potentials -- productivity and value. Weyerhaeuser Sci. Symp. 4. Weyerhaeuser Co., Tacoma, Wash.: 253-276.

Sederoff, R.R., A.-M. Stomp, W.S. Chilton, and L.W. Moore. In preparation. Transfer and expression of bacterial genes in *Pinus taeda* cells transformed by *Agrobacterium tumefaciens*. Biotechnology

Strauss, S.H. and W.B. Critchfield. 1982. Inheritance of -pinene in xylem oleoresin of knobcone x Monterey pine hybrids. For. Sci. 28: 687-696.

U.S. Committee on Genetic Vulnerability of Major Crops. 1972. Genetic vulnerability of major crops. Nat. Acad. Sci., Wash., D.C. 307 pp.

BIOTECHNOLOGIES - THE POTENTIAL ROLE OF SOMACLONAL VARIATION IN FORESTRY

Darroll D. Skilling and Michael E. Ostry

Principal Plant Pathologists
North Central Forest Experiment Station
USDA Forest Service
1992 Folwell Avenue, St. Paul, MN 55108

Plant tissue culture techniques are being used to obtain disease resistant forest trees by identifying genetic variation in cultures. This study seeks to obtain resistance to Septoria canker of Populus, caused by Septoria musiva, and scleroderris canker of Larix caused by Gremmeniella abietina. Populus somaclones with varying resistance to Septoria have been tentatively identified.

Recently, plant scientists have identified a useful supplement to traditional breeding programs in a phenomenon called "somaclonal variation." This system could incorporate one or more desirable traits into forest trees without breeding cycles of 20-30 years. The term somaclonal variation was proposed to describe variation exhibited by plantlets obtained from aseptic plant cultures (Larkin and Scowcroft 1981). With aseptic plant culture systems, it is now possible to take many old plant lines and literally turn them into dozens of new lines in a matter of months.

Many plants regenerated from aseptic cultures are not true to type. Rather than being clones of the parent plant, the regenerated plants may show wide variation in certain traits. In some cases this variation may even exceed that of plants produced by standard cross-breeding practices (Larkin and Scowcroft 1981, Shepard et al. 1980).

Although somaclonal variation was not recognized as a true genetic process until recently, natural variation has been utilized in agriculture for many years. Two familiar examples are the "Russet Burbank" potato, a sport or natural somaclone from the Burbank potato, and the various color varieties of Delicious apples - sports from the original striped Delicious apple. Examples of beneficial somaclonal variation are becoming common. One example of this type is from Australia as reported by Larkin and Scowcroft (1981). Working with sugarcane, they were able to produce lines that were resistant to eyespot disease caused by the fungus Heliminthosporium sacchari. This disease has limited the usefulness of the new high-yielding cane types because of their high susceptibility to the Heliminthosporium fungus. Larkin and Scowcroft introduced a toxin produced by the eyespot fungus into the nutrient medium. Young plantlets grown in this toxin supplement medium were then screened for disease resistance. The researchers were amazed at the amount of variation present. Most plants were more resistant than the parent line. Seventy percent of these plants maintained stable resistance through five vegetative generations. When six of these somaclones were given a second 6-month tissue culture cycle, 60 percent had similar or enhanced toxin tolerance relative to the primary somaclone (Scowcroft et al. 1983). Apparently characters can be "stacked", i.e., we can screen for modification of a second characteristic following a second culture cycle and expect that some will retain the first characteristic, (Shepard et al. 1980).

Other examples of directed somaclonal variation in agriculture include work with the aforementioned Russet Burbank potato by Shepard et al. (1980). Although no special effort to induce genetic changes in his parent lines was made, extensive variation among regenerated plants was observed. Variant characteristics included a more compact growth habit, a reduction in the day length required to initiate flowering, increased yield when compared with the parent line, and 5 of 500 regenerated plants were resistant to early blight caused by the fungus Alternaria solani. These characters proved stable through three tuber, i.e., asexual generations.

Oono (1981), working with rice tissue cultures, found extensive variation. In this study, a homozygous parent that was a dihaploid derivative from anther culture was used. The progeny of 1121 somaclones were examined in three successive selfed generations. Among the somaclones analysed for five characters, 72 percent differed from the parent in at least one character; 28 percent differed for two or more characters.

What Causes Somaclonal Variation?

So far, the genetic basis for somaclonal variation is unknown. Larkin and Scowcroft have suggested that it is due to gene amplification, an increase in the number of specific genes, so that their combined product has an enhanced effect. Gene or whole chromosome deletion involving the loss of a gene function may also be involved. A third possibility is transposable elements, special DNA sequences that can move from one position in a cell's chromosome to another or even to a different chromosome, thereby altering the activity of certain genes.

Whatever the causes of somaclonal variation, it apparently is a frequent phenomenon within many aseptic culture systems. The frequency depends on the plant species being grown in tissue culture and the duration of the culture cycle. The longer the plant is in tissue culture the greater the variation.

The confirmed amount of somatic variation appears to be limited primarily by the investigator's ability to devise a system that will identify the variation. Thus, although many somaclones may be resistant to a certain fungus disease, they must be exposed to the fungus so that the variants can be recognized.

Although somatic variation may be a somewhat random process, it is possible to increase the variation by exposure of the tissue to toxic culture filtrates from a disease organism. However, somaclonal variation for disease resistance also preexists in the host tissue or is induced through the culture cycle in the absence of toxins (Larkin and Scowcroft 1981).

The stability of observed variation after the somaclones are removed from tissue culture is an important consideration. Scowcroft et al. (1983), working with Mexican double dwarf wheat, found that, although the initial somaclonal regenerants exhibited some phenotypic differences from the parents, their progeny revealed even greater genetic change. Some of the progeny bred true but others continued to segregate for height with some progeny being twice as tall as either somaclone parent. Evidence from several important agronomic crops shows that this variation is heritable in both sexually and asexually reproducing species (Scowcroft and Larkin 1983).

The Impact of Somaclonal Technology in Forestry

The techniques developed with agronomic crops offer exciting possibilities in forest tree improvement. For the first time we can consider improving forest trees for specific characteristics in about the time it takes for corn and wheat improvement. If somaclonal variation is exciting to the wheat breeder who works with a generation time of 6 months, think what it means to the forest tree breeder who has to deal with generation times of 10, 20, or more years.

The North Central Forest Experiment Station's Biotechnology Program is part of the USDA Forest Service's national research effort for forest tree improvement (Nelson and Haissig 1984). Now we can talk seriously about developing resistance to specific tree pests and other stress agents with the possibility of seeing results in a few years. Some problem areas that Forest Service laboratories are considering are resistance to diseases, insects, nematodes, drought, salt, metal toxicity, frost, and herbicides. Examples already exist in agronomic crops where resistance to most of the above problems have been identified in somaclones. The word "identified" is important because, with the exception of phenotypic variation that is visible, most traits will not be expressed until challenge systems are devised to expose somaclones to a particular pest or environmental stress. Only then can variants be identified.

Concurrent study of all the problems listed above is unlikely. However, in a well organized somaclonal variation research program where all somaclones are maintained, it is possible to test any given somaclone against each stress agent. Thus, even if a somaclone has no resistance to a particular disease, it may still have resistance to another stress agent. By placing a resistant somaclone back into culture and regenerating hundreds of new plants, it may be possible to recover plants expressing resistance to both the original and secondary stress agents.

In addition to the advantage of reduced time with the somaclonal system, there are several other advantages of somaclonal variation programs. First, since all preliminary screening is done in the laboratory, conditions can be controlled with greater precision than in a greenhouse or in the field. Also, since thousands of somaclones can be grown in a single incubator, the cost of developing initial test plantations is postponed until plantlets with expressed resistance are ready for furthur field testing.

This system is not viewed as replacing traditional tree breeding, but rather as a new tool that will help tree breeding move forward more rapidly. In fact, another advantage of somaclonal variation is that it offers a potential source of genetic variation in plants that normally have a narrow genetic base. The idea of introducing genetic variation into forest tree species with little genetic variation such as red pine, *Pinus resinosa*, offers interesting possibilities.

Our research into the development of disease resistance in forest trees utilizing somaclonal variation was begun about 15 months ago. We believe this is the only project involving the use of aseptic culture techniques to promote the identification of useful variation in a forest tree species for disease resistance. The first step was to determine which tree species to begin with. *Populus* was selected as the best candidate because it is of worldwide importance as a source of fiber and energy. In the United States, potential biomass yields from intensively managed hybrid poplar plantations are primarily limited by the foliar and canker pathogen *Septoria musiva*. Conventional tree selection and breeding are costly and too slow to produce genes resistant to *S. musiva* as well as other damaging pathogens of *Populus*. The genus *Populus* has previously been grown in tissue culture, and plantlets from culture have shown variation from the parent plant (Lester and Berbee 1977).

Our *Populus* study is a cooperative venture between the North Central Forest Experiment Station, Forest Disease Project, and the Department of Horticulture at the University of Minnesota. The specific objectives of this project are to: (1) develop aseptic plant culture systems that allow plantlet regeneration at different levels of tissue organization, and (2) develop screening systems that can be used at either the cellular or plant level to identify and recover somatic variants that show increased resistance to S. musiva.

The hybrid poplar clone NE299 (Populus nigra cv. betulifolia X P. trichocarpa) is the main source of material in our research. It was selected because it has good growth potential but its usefulness in plantations is limited because of its high susceptibility to Septoria leaf spot and canker. Other clones somewhat resistant to Septoria are used for comparing with NE 299. Isolates of S. musiva have been collected from poplar plantations throughout the north central United States. Woody Plant Medium (Lloyd and McCown 1980), and various auxin-cytokinin concentrations and combinations are being used to refine methods for increasing adventitious shoot proliferation from leaf discs, callus, and suspension cultures. We are also developing bioassays, using conidia, mycelium, and culture filtrates of S. musiva to rapidly identify and select disease-resistant Populus cells and regenerated plantlets. Although this phase of the project is only a few months old, a leaf disc assay has been developed that can be used to rapidly identify Populus somaclones with resistance to S. musiva. Preliminary screening with this assay has already identified Populus somaclones with varied resistance to the Septoria fungus. Although these results are preliminary and furthur testing will be needed, the ability to produce potentially disease-resistant plantlets in the laboratory within a few months is a strong indication of the possible utility of somaclonal variation.

The second model system selected for study involves Larix and the fungus Gremmeniella abietina, the cause of scleroderris canker. Although conifers are much more difficult to manipulate in vitro, the need to obtain Larix resistant to scleroderris in a short time period dictates that work begin in this area. This is a cooperative study between the North Central Forest Experiment Station's Forest Disease Project and the Michigan Technological University's, BioSource Institute, and is now 6 months old. The primary species being used is European larch, L. decidua. We have already obtained many plantlets from the aseptic culture system and transplanted them into a soilless potting medium under high humidity. Isolates of both the North American and European strain of G. abietina will be used to inoculate these plantlets in our search for resistance to Scleroderris canker.

Although we are optimistic about somaclonal variation, it is important to point out that testing for disease resistance in tissue culture is different from field testing. For example, static defense barriers, such as cuticles, may not be well developed in plantlets coming from the culture system, so these resistant mechanisms can be discounted. This problem can be overcome by growing plantlets in the greenhouse, although this increases the time and expense of the screening system. Also, many events that normally occur in plants may not occur under aseptic culture conditions. If these events are related to disease resistance, only greenhouse plantlets could be successfully screened. One final unknown is whether resistance expressed in vitro will be maintained into maturity. This emphasizes the need for field testing. While none of the foregoing problems have been limiting obstacles in agronomic crops, they do deserve consideration for forest tree species. None of these problems is related to the expression of somatic variation, only to the disease testing process.

W. R. Scowcroft, one of the pioneers in this field sees somaclonal variation as an interim means of improving agricultural species while molecular biologists work toward the ultimate goal of genetically engineered plants. This may also be true in forestry. Given the rate of progress in forest genetics during the last 75 years, we can only applaud any system that will allow us to see useful genetic change that will benefit forestry within our own lifetime.

Literature Cited

Larkin, P. J.; Scowcroft, W. R. Somaclonal variation - a novel source of variability from cell cultures for plant improvement. Theor. Appl. Genet. 60: 197-213; 1981.

Lester, D. T.; Berbee, J. G. Within-clone variation among black poplar trees derived from callus culture. Forest Sci. 23: 122-131; 1977.

Lloyd, G.; McCown, B. Commercially - feasible micropropagation of mountain laurel Kalmia latifolia, by use of shoot-tip culture. Proc. Inter. Soc. Plant Prop. 30: 421-426; 1980.

Nelson, N. D.; Haissig, B. E. Biotechnology in the Forest Service's North Central Forest Experiment Station. In: Proceedings International Symposium of Recent Advances in Forest Biotechnology, 1984 June 10-13; Traverse City, MI. East Lansing, MI: Michigan Biotechnology Institute; 1984: 139-154.

Oono, K. In Vitro methods applied to rice. In: Plant Tissue Culture (T. A. Thorpe, ed.) New York: Academic Press; 1981; 273-298.

Scowcroft, W. R.; Larkin, P. J.; Brettell, R. I. S. Genetic variation from tissue culture. In: Use of tissue culture and protoplasts in plant pathology. (Helgeson, John P.; Deverall, B. J. eds.) Australia: Academic press; 1983; 139-162.

Scowcroft, W. R.; Larkin, P. J. In Comprehensive Biotechnology Vol. 3 (C. W. Robinson and H. J. Howells, eds.) in press. Oxford: Pergamon Press; 1983.

Shepard, J. F.; Bidney, D.; Shahin, E. Potato protoplasts in crop improvement. Science 208: 17-24; 1980.

FRONTIERS IN WOOD UTILIZATION RESEARCH IN THE UNITED STATES[1/]

John R. Erickson

Director, Forest Products Laboratory
USDA Forest Service
One Gifford Pinchot Drive, Madison, WI 53705

Research in forest products has yielded tremendous benefits to the people of the United States and its economy. Safe wood structures, strong corrugated containers from recycled wood fiber, chemicals and energy, and treatments that protect everything from piers and docks to porches and decks are but a few. Research today is needed to accurately define wood properties so that more species can be used; also, we must learn to use more of the harvested tree; and finally, we must keep pace with the changing needs of our society by not only using, but by creating new technologies.

Introduction

For centuries, people have depended on the wood resource for the necessities and comforts of life--shelter, transportation, tools, paper, and chemicals, to name only a few. In the United States, wood was essential to early colonial development and continues to be a major segment of our economy today. As a structural material, it continues to dominate the housing scene. The pulp and paper industry is a giant, and as a fuel, wood provides nearly as much energy as does nuclear power. Artisans and researchers have expanded both the practical and scientific knowledge of wood, making it one of the most valued natural resources on earth.

The degree of wood-use technology varies from country to country--improving the efficiency of wood as a cooking fuel may be a focus in an underdeveloped nation, while sophisticated new technologies developed through microbiological research is currently an aim in several developed countries. For this paper I will concentrate on technologies of wood use in developed countries, specifically the United States.

In the later part of the 1800's, wood scientists began systematic investigations on wood properties at various university laboratories throughout the United States. A primary goal of this research was to find a broad spectrum of species to meet the expanding need for a variety of wood products. In 1910, a team of 45 scientists were drawn together at Madison, Wisconsin, when the Forest Products Laboratory (FPL) was established.

The advancements from this and other wood research laboratories have been significant. At the turn of this century, only a few species were considered commercial. Now we believe that nearly all species could fill commercial market needs. Fifty years ago, about half of the wood delivered to many mills was wasted. Now, over 95 percent of this wood is used.

Although we've come a long way since the 1800's, the cliche' holds true--much remains to be done. First, we still leave too much of the harvested tree in the forest. Second, we lack specific data on wood properties of some species; this hinders their use. Third, our timber resources are constantly changing. And finally, the wood products industry, like all others, is characterized by change. New products and processes are needed to meet the changing needs of society, maintain our industries, and promote international trade.

The body of knowledge supporting the wood-based industries in developed countries is well known around the world. A close look at the research programs of various nations today and in the recent past shows a marked similarity in scientific studies. This is not surprising, since scientists throughout these countries enjoy a long and strong history of collaboration. This collaboration will increase as our tools and methods of communication become even more sophisticated.

With these thoughts as a preface, I'd like to present my views on selected areas of wood utilization research which are forging new technologies. I will draw heavily on the FPL program to illustrate my views of research, since it represents a collective opinion of many scientists, administrators, and user groups on research needs in the United States. This is with due respect to the partnership we share with other countries and many U.S. universities and industries and their many successes. These areas of research are: wood engineering and construction practices, solid wood processing techniques, structural composite products, pulp and paper, wood chemistry, and microbial technology. I will discuss each area in terms of past accomplishments, current efforts, and future research needs.

Wood Engineering and Construction Practices

Accomplishments. Safe wood structures are the result of many years of research quantifying the physical and mechanical properties of wood. Design standards and various codes are built on this fundamental research data base. Also, more efficient designs permit building safe structures with less wood. Many different species are now used for construction, and structural composite wood products are beginning to emerge in the marketplace.

[1/] Presented at IUFRO S6.06 Meeting, Ft. Collins, CO, July 25-26, 1985.

Several light-frame construction methods have recently been developed to conserve the wood resource. Most notable are the factory-built homes now being built in several countries. However, even custom-built homes are using less lumber than in the past. Trusses with members of small cross sections are being used more. An innovative variation of light-frame structural trusses is the truss-framed system. This innovative framing system incorporates an open-web floor truss, an open-web roof truss, and conventional wall studs into a unitized frame. The factory-built frames are delivered to the construction site and easily erected onto the building's foundation. This concept was developed to provide a high-quality engineered frame at reasonable cost. Some U.S. builders are claiming savings of ten dollars per square foot and are cutting construction time in half. Typically, a three- to four-person crew can erect and sheath a house in one day. This building concept, developed in the United States, has been applied in more than 30 States and other countries have published design manuals on this method.

Current research. One major area of research underway is what we call In-Grade testing. The procedures for visual grading of structural lumber were established in the United States about 50 years ago. Although there have been some modifications since then, the basic principles of grading remain essentially the same. Strength values assigned to lumber are based on early tests performed on small clear specimens with adjustments for reduction in strength due to knots and irregular grain. Tests were conducted only on certain species and, as a result, strength values are not currently available for much of our domestic timber. This data has served satisfactorily over the years, however, modern wood structures are being engineered more precisely and better estimates of structural lumber strength are needed. Studies are underway to determine the "mill run" strength of selected grades, sizes, and species. The new values will be used to improve the design of wood structures.

Future emphasis. There are a number of research emphases in processing and use of wood for structural purposes. There is a continued need to improve our knowledge on the basic engineering properties of solid wood and composite wood products--such as trusses, I-beams, particleboard, and glulam timbers. This knowledge will lead to reliability-based design procedures for wood similar to those used for other construction materials. Concurrently, there is a need to continue to develop analytical computer software to incorporate this new data base in design models.

Solid Wood Processing Techniques

Current research. Many years of research and development have provided the primary and secondary wood processing industries with improved product recovery. As indicated earlier, in the first few decades of this century, it has been said that U.S. mills converted less than one-half of the logs to product. By 1962, about 87 percent of the log was used, and by 1976, over 95 percent of the log was used.

While more mill residue is being used for energy, there are also great improvements in the yield of primary and secondary products. Electronic log scanning, new sawing techniques, and thinner saws have increased lumber yield by 15 to 20 percent in recent years. Improved drying techniques reduced the degrade substantially, and new processing techniques converted residues to usable pulp chips and furnish for particleboards.

Recent research. Continued attention to the processing of softwood lumber is providing additional gains in product yields. Other research involves methods to produce quality structural lumber from hardwoods. Most solid wood structural members are now made from softwoods because hardwoods warp and twist when sawed and dried by conventional methods. One such method for making high-quality structural grade lumber from light- and medium-density hardwoods is Saw, Dry, and Rip (SDR). In SDR, green logs are live-sawn into 1-3/4-inch flitches. These are dried, often at temperatures above 212 degrees Fahrenheit, to a moisture content of about 12 percent and then rip-sawed into studs. Structural lumber processed by the SDR method is virtually free of warp with over 90 percent of the lumber, depending on species, meeting stud grade requirements.

Future emphasis. A look to the future indicates a need to develop new processing techniques that will maximize the yield of lumber from logs. For example, laser cutting and new sawblade technology would provide for greater lumber yields, thinner kerfs, and better finishes. Further, the use of microcomputers and robotics will not only be possible, but essential for the lumber industry to remain competitive with other materials.

Structural Composite Products

Accomplishments.--Early research in adhesives and veneering made the United States a major producer of plywood. Recent research has provided new techniques to increase yields of plywood and to improve product performance.

One example leading to increased yield is a device called the powered backup roll which improves veneer peeling from logs. Powered rolls push against the log and help to rotate the log against the cutting knife. It virtually eliminates spin-out caused when the driving chucks cannot match the force of the cutting knife. It will permit peeling of logs down to about 4 inches, rather than 6 to 8 inches, thus greatly increasing yield.

More recently, research efforts led to the increased use of structural flakeboard and oriented strandboard. These new structural board products supplement plywood in construction. It is exciting to think that forest residues as small as your finger can make a structural board that compares to plywood made from large-diameter logs. In 1978 there was only one flakeboard plant in the United States. Now there are over 15 and many more have been announced.

Current research. Advancements continue in the field of composite products. A new process injects saturated steam into a structural flakeboard mat during the press cycle. This reduces the time required to cure the resin by about 60 percent for 1/2-inch boards and by 75 percent for 1-inch boards. This is a significant improvement in processing and will permit commercial operations where thicker flakeboards are needed for heavy structural applications.

Future emphasis. There will be a continuing need to improve the process for taking wood apart and putting it back together. It will involve the tools and processes for cutting wood and a greater understanding of adhesives and the adhesion process. Also needed will be a better understanding of raw material properties and the mechanics and physics of forming a variety of products. This research will result in a new generation of engineered products from flakes and other particles that will provide durability against adverse environments, including fire.

Pulp and Paper

Accomplishments. The success of today's pulp and paper industry is due to the research breakthroughs made by many laboratories. In addition to the development of several pulping processes, research on specific species has changed and improved industrial forestry and management. We are now able to harvest and manage a much greater proportion of our forest lands. A good example of this is the mixed hardwood stands where thinnings used for pulpwood permit the accelerated growth of sawlogs and veneer timber.

We have also found ways to reduce waste. For example pulp chip handling and storage was a major problem because the natural biological reactions in chip piles caused heating and deterioration. The loss of fiber was nearly 1 percent per month of storage in some geographic areas. Research addressed this problem and recommended storage techniques to minimize these large losses.

Another more recent accomplishment promises to make better use of our hardwood resources. Presently the linerboard used to make corrugated containers is made primarily from softwoods. A new process called press drying will produce an excellent linerboard from 100 percent hardwoods. It also works well with recycled fiber.

Current research. A good example of current research is in recycled papers. The United States now recycles about 26 percent of the available wastepaper. While commendable, it falls quite short of the Japanese, who recycle 95 percent, and the Europeans, who also recycle large percentages. A recent advance in this area is the disk separation process which permits removal of adhesives, plastics, and other so-called "stickies." The process takes advantage of the difference in wettability of wood fibers and contaminants to recover good fiber. Reject material from mills using recycled papers can exceed 10-15 tons per day. This is waste that now goes to a landfill. Recent research results with the disk separation process have recovered over 50 percent of the good fiber. These promising results warrant scale-up to commercial trials.

Future emphasis. An emerging technology promises to change our thinking on how paper is used. FPL Spaceboard, a three-dimensional structural board made from wood fibers, can be made thin enough to make strong lightweight corrugated containers or thick enough for wall sections for homes. Treated chemically, FPL Spaceboard can provide the wet strength and dimensional stability to build highly engineered structures. You will see these research results applied in the 1990's.

Wood Chemistry

Accomplishments. The study of wood chemistry has been a growing, changing one. In the early 1900's, research in naval stores improved production processes for turpentine, resins, and rosins. Progress in the 1920's was largely in such fundamental concerns as the components of wood--cellulose, hemicellulose, and lignin. The major effort of World War II in chemical and energy research was toward the persistent problem of threatened fuel shortages.

Current research. Today sophisticated analytical instruments allow researchers to refine their description of wood's components on a precise chemical basis. At the FPL one of these components is receiving special attention--lignin. Researchers are carrying out a series of studies to clarify the chemical nature of lignin and its role in chemical and fiber production. In papermaking, for example, yield and properties of fibers are directly related to the form and amount of lignin remaining in the fiber. As pulp yields are increased through retention of hemicelluloses or lignin, the properties of the pulp and their suitability for various products changes. We know in general that increased lignin content means a stiffer fiber and increased hemicellulose means a more easily beaten, better bonding fiber. If lignin is to be retained with the fiber, as in the case of high-yield chemimechanical pulps, we must have methods of decreasing the stiffness to improve bonding and conformability for grades such as printing papers.

Future emphasis. Underlying all chemical or partially chemical methods of producing fibers from wood is the fundamental chemistry of lignin. It is important that we know more about its nature--its chemical reactivity and its association with other wood polymers--to permit the optimal design of efficient, selective pulping systems.

Another opportunity is to recover chemicals from wood processing and pulping operations. Advanced technologies include the application of specific membranes for separating the wood chemicals produced in pulping. This advanced technology has promise for separating components from the complex mixtures of chemicals from the pulping stream.

The development of new adhesives, both chemical and natural, will also open new techniques for wood products use. The future of composite products such as flakeboard and waferboard will strongly depend on the availability of inexpensive and durable bonding systems. Phenolic resins, currently used in many composite products, are derived from petroleum and are, therefore, subject to price and supply fluctuations. Research to yield economic adhesives from lignocellulosics is needed.

Microbial Technology

Accomplishments. The last area of work that I would like to highlight is microbial technology. Considerable interest and support is now available in this exciting new field. What we in forest products are doing is developing an understanding of the biodeterioration process. On the one hand, we are looking for ways to protect the wood from decay. On the other hand, we are looking for enzymes that will degrade the lignin, thus freeing the cellulose.

About 2 years ago FPL scientists discovered the first lignin-degrading enzyme. This discovery offers potential economic and environmental advantages for wood pulping and papermaking. Also discovered recently was a biological process to remove the dark color from pulp mill wastewater. Called MyCoR for mycelial color removal, the process also reduces or eliminates the BOD, COD, and some chlorinated compounds found in the effluent.

Future emphasis. Another area of research at the FPL is the cultivation of shiitake mushrooms on wood residue. This mushroom is important commercially and a new cottage industry is developing from the initial effort using logs. The cellulose residue from mushroom cultivation will be tested for use as animal feed and paper pulp.

Another area with great potential is in the production of ethanol. Recently scientists have found ways to ferment the 5-carbon sugars remaining after acid hydrolysis. Full success in this area will change the total yield of ethanol from hardwoods, leading to more economical production of alcohol.

There is a wide spectrum of useful products and improved processes which can be explored through biotechnology. Some of the more exciting potentials are biological pulping, production of byproducts and food stocks from wood, chemicals through biochemistry, and ethanol production.

Emerging Opportunities

Research in forest products has yielded tremendous benefits to society and our economy. Current research efforts underway point to a promising future. The examples I've mentioned are but a few of the projects that are underway. Some of the research needs and opportunities for the future follow:

Wood Engineering and Construction Practices

- --A data base to support reliability-based design procedures for wood.
- --Evaluation of new composite wood products.
- --Computer models for three-dimensional design of wood structures.

Solid Wood Processing Techniques

- --Processing techniques to optimize recovery of solid wood products from logs and lumber.
- --Development of improved sawblades and cutting methods.
- --Greater use of microcomputers and robotics.

Structural Composite Products

- --Improved processes to produce a variety of wood particles.
- --Advanced knowledge of adhesives and adhesion phenomenon.
- --Improved methods for forming composite products.

Pulp and Paper

- --Purification of waste effluents.
- --Greater use of hardwoods and wastepaper.
- --Development of structural paperboard products.

Wood Chemistry

- --Improved knowledge of the nature and structure of lignin.
- --Recovery of chemicals from the byproducts of pulping and other processes.
- --Development of economical and effective adhesives from wood and wood byproducts.

Microbial Technology

- --Biopulping.
- --Alcohol production from 5-carbon sugars.
- --Chemical production from wood--new processes.

Conclusion

During the past 75 to 100 years, federal, university, and private industry laboratories have joined in the efforts to produce the kind of research information that I have been speaking about. It is this effort that has made possible the viable wood products industry that exists today.

We must not be content with our position. We must continue to question our conventional wisdom and challenge, rather than congratulate, ourselves. Only then can we move ahead to where our future plans become our past accomplishments. The outlook for wood products and wood products research is indeed promising.

FRONTIERS IN HANDLING WOOD

C.R. Silversides

Forestry Consultant
R.R.#1, Prescott, Ontario, K0E 1T0
Canada

The handling, harvesting and transportation of wood in all its forms and end-uses represent a continuum of activity with the objective of supplying its conversion plants atthe lowest possible cost, taking into account all the constraints, natural and man-made, imposed upon it. All forms of technological advancement will be incorporated into this activity as appropriate.

The word "frontier" has two meanings. One is the furthermost limit of knowledge or achievement with respect to a particular subject. The other is, a new field which offers scope for exploitative or developmental activity. Both these meanings will be considered in the context of this paper, the advanced state of the art and potential new fields for development.

For a 25-year period, post-World War 11, there was a great outburst of activity around the world in the attempt to mechanize timber harvesting. This was particularly true in northern Europe and North America. In Canada during the period 1945-1948 there was an intensive effort made in Quebec to mechanize timber harvesting. It ended in failure. If it proved anything it established that there was little increase in productivity and decrease in cost if logging methods were not changed and human and animal power alone were replaced by mechanical energy.

Animals, predominantly horses, which provided much of the energy used in the woods were displaced by mechanical power. This was the result of the invention and production of the rubber-tired, articulated frame-steered chassis in different configurations as skidders and forwarders. It appeared on the forest scene in 1951. This chassis mimicked the manouverability of the animals and replaced them in their functions without disturbing any other part of the harvesting sequence. Their direct substitution was immediately accepted by woods workers. In reducing tree lengths to shorter logs or bolts, high-capacity mechanized saws with attendent hydraulic loaders and unloaders or conveyors took over at roadside or riverbank landings removing the laborious work involved.

In the above developments there was a good deal of direct manual labor involved and the operations might be considered to be semi-mechanized. Full mechanization has generally been defined as producing wood untouched by human hands, and in some instances with no person working on the forest floor.

Attempts at full mechanization of tree harvesting began in the southern United States about 1953, culminating in the appearance in the woods of the Busch Combine in 1959. This was a rubber-tired, one-man harvesting machine which produced short bolts at the rate of 1.5-2.0 cords/hour. In Canada in 1957 the first attempts were made to harvest full trees. In Ontario it was with Clark Horncastle's Full-Tree System and in Quebec with the Vit Feller Buncher and the Bombardier Processor. In 1960 the so-called Beloit Harvester made its appearance, to produce tree lengths. Tens of millions of dollars were spent in engineering concept, design, prototyping and short series production or preproduction with little success.

The drive for mechanization had two aims, to deal with threatening shortages of woods labor and also to reduce wood costs. This latter aim was never widely achieved although in regional or site-specific instances was claimed.

The prize held out before all those who labored on the development of tree harvesters and processors was the success, both financial and technical, of the chain-saw and the articulated rubber-tired, frame-steered tractor. What was not sufficiently considered was that these two are both basically single-function machines while harvesters and processors by their very nature are complex multi-function machines or systems with a large number of interdependent functions. A compounding factor affecting the potential success of the mechanization of forest operations was that there was not and is not today a consensus as to the form in which the harvesting machines should produce their crop, full trees, tree lengths, short lengths, or chips. This fragmentation of needs has fragmented the market and has created a real dilemma for machine builders.

Wood is a low-density, high-volume, low-value product. For example, in Eastern Canada the average tree size is approximately 19 cm dbh with a cubic volume of $0.20m^3$. With costly machines the trees must be harvested and processed in multiples to obtain the productivity required to justify replacing manually produced wood.

Why are harvesting and processing machines costly? Unlike consumer products such as cars and trucks or agricultural machines, timber-harvesting machines have a relatively restricted market. They can only be produced in short or limited production runs, somewhat similar to those for many types of construction equipment. The economy scale of mass production is not possible. As a result such equipment may cost between $8.80 and $24.00/kg compared to $5.50 to $6.50/kg for mass-produced machines. The Swedish forest machine industry is an example where the capacity to produce a wide variety of machines expanded far beyond the market and the industry literally collapsed and was subsequently sold to Finland to permit its rationalization. The harvested crop, trees, is of a size and weight unknown in agriculture. The operating environment for forest operations is beyond that normally encountered by the construction industry or even by military operations. This is why timber-harvesting and processing machines are costly.

From literally dozens of concepts, most of which appeared in hardware in one form or another, few have continued to develop and thrive. The process has been evolutionary and has varied from country to country. In Scandinavia the forest industry has stayed with shortwood (logs or tree sections), a heritage from earlier sawtimber times. In North America, when forest operations were labor intensive, sawlogs were produced in lengths to be handled with available animal power and pulpwood was produced in lengths capable of being handled by man. However, as mechanization has developed in North America and it was possible to handle larger pieces, tree-length and full-tree systems developed. The rationale for this was that the minimum amount of work should be carried out in the forest under uncontrollable conditions and a maximum of work be carried out with trees concentrated and operating conditions controlled to the degree possible. An example of this trend is seen in the rise and decline of the shortwood harvester and the current rise of the feller-forwarder which fells and carries full trees out of the forest to roadside for further processing.

Table 1: Percentage of limit wood in Eastern Canada produced by different harvesting methods.

Period	Method		
	Shortwood	Tree length	Full tree
1950	95	5	0
1955	90	10	0
1960	80	20	0
1965	45	55	0.2
1970	31	68	1
1975	19	76	5
1980	20	64	16
1984	10	60	30

Twenty-five years after the introduction of machines capable of felling, limbing and topping trees and cutting them to desired lengths, it would appear that developments have reached a plateau. Machines and methods have reached a stage in their evolution where only minor improvements are appearing. As in any evolutionary process presently operated machines have developed from earlier forms, most of which have disappeared. For example, the successful articulated frame-steered chassis which first appeared in the forest in 1951 was similar in concept to a short-lived tractor built by the John Deere Company in 1916.

The introduction in 1960 of harvesting machines such as the Busch Combine in the southern United States and the Beloit Harvester in Canada had a varied reception, from complete scepticism to mild enthusiasm. They were the first and met with the innate resistance offered any innovation, with the fate of those who are first in a world where it is safest to be third or fourth. Alexander Pope, the English poet, was a real student of human nature when he penned the lines "Be not the first by whom the new are tried, nor yet the last to lay the old aside."

Many of the concepts for harvesting trees which have not proven successful did not fail because of their specific characteristics but because the timing of their introduction foredoomed them to failure. The forest industries of the world are cyclic in nature, their market for lumber varying with fluctuating mortgage interest rates and fluctuating GNP for newsprint. Introducing new harvesting equipment and methods on the upside of the cycle does not guarantee success but it does mean that the innovation is in a favorable economic climate. If an innovation is presented at a time when the cycle is on the downside there is great difficulty in having it considered. On the upturn production takes precedent over cost, on the downturn cost over production.

There has been a period of 25 years for the forest-based industry to become accustomed to mechanization, to develop a management that will accept it and to develop a labor force of machine operators and mechanics familiar with and comfortable with complex machines. Woodlands management has become accustomed to, if not reconciled to substantial capital expenditure for equipment. Perhaps now is the time for a thorough reassessment of past equipment and method innovations that did not achieve acceptance because of their timing, or some reason other than the concept itself. The Beloit Harvester is a good example. In 1982, 12 years after their production had ceased, 50 percent of the units manufactured were still in operation, some with over 100,000 hours on them. Is this perhaps another example of the industry giving up on a good idea too soon?

In the 25-year period since logging mechanization began in earnest there have been many new developments which impinge upon and may have a dramatic impact upon this activity. The classic example of the impact of unrelated technologies upon forestry equipment is the case of the chainsaw. This device in much the same configurationas it exists today was first patented in Germany in 1850. However, it was not until post-World War 11 that everything came together to make it a technical and financial success. It was the wartime development of light metal technology and the small air-cooled engine developed for target aircraft that made the chainsaw what it is today.

Wood handling, harvesting and processing are changing and will continue to change as new wood uses and new technologies in materials, in hydraulics, electronics, pneumatics and computer science come to bear on it. The acceptance of energy as a legitimate and important forest product has already had an impact on wood handling. Chipping has always been accepted as the way wood can be disintegrated or broken down into small usable parts. Chemical pulping was the only outlet for such material. When wood is used for energy purposes, in direct combustion or gasification, chips are not the ideal form but shredded or chunk forms have proven more desirable. Bolts of wood are fed through compression rollers similar to rolling out steel or aluminum ingots and they are shattered lengthwise to produce new forms not previously seen. Harvesting residues are collected and baled like hay or old cars to increase ease of handling and transport.

Energy as a forest product will impact strongly on our forest management practices as energy wood has few if any specifications as to quality, species, bark content, size, etc. It can use all of the material constituting our silvicultural slums. It gives a value to forest material which previously had none and whose disposal may have been a cost.

There is a continual change taking place in wood-handling techniques depending upon specific needs. Such changes are made to improve productivity, quality or cost. When mechanical felling with machines was first introduced chainsaws were used. These proved vulnerable to damage and were replaced by large hydraulically operated shears. At first the shears were a single blade acting against an anvil but later consisted of two parallel opposing blades. It was found that shearing trees caused damage to the butt end and degraded the butt log for lumber so shears were replaced by circular saws of various types. Some felling heads contained one saw, others two. Milling heads were introduced and proved successful and of interest to some users. Circular saws, rim driven, with no centre mandrel have appeared and permit a lighter, more compact felling head.

Limbing of trees has been attacked in a number of different ways. One of the early developments was wrapping a flexible belt of chisels around the tree and pulling the tree through it. This in turn was replaced by a set of fixed curved knives designed to follow the taper of the tree. In limbing, as in any operation which involves handling trees longitudinally, the productivity of the operation will vary roughly as the square of the tree's diameter. To limb small trees economically they should be processed in batches. One method developed in Canada was with the use of a chain flail. A number of full trees would be spread on the ground and their limbs would be removed by attrition by means of chain flails mounted on the front end of a rubber-tired tractor - economic but with variable quality. In the southern United States the gate delimber has found wide acceptance. A device similar in configuration to a farm gate, made from heavy pipe is anchored between two trees. A skidder load of pine trees is backed up against it, poking the tree tops through the grid of pipes forming the gate - crude but very effective for the species involved. In Sweden, a recent development is batch delimbing of trees in large drums. The full trees are fed in one end, the limbs are rubbed off in the first section and subsequently the tree boles are also debarked before emerging from the discharge end of the drum.

Bark removal has always been a problem for the pulp industry. Originally barking was carried out in the forest manually in the sap peeling season or with small mobile barking machines. This proved to be too labor intensive and expensive and barking was moved to the mills. Cambium shear barkers were tried but large barking drums proved to be most productive and economic. In this instance there appears to be an economy of scale as drums have grown in size from 3.2 m x 9 m up to 3.8 m x 60 m. With the growth in the use of chipped sawmill residues and of whole-tree chips and with the trend to refiner mechanical pulp, which uses chips, the separation of wood and bark in chip form is finally receiving the attention it deserves. New chip screening technology and pulp stock cleaning techniques combine to make this development most attractive.

Transportation constitutes a third to a half of the cost of delivered wood. The highest unitcost of transport is incurred in the first stage of transportation, from the stump to road. Much effort is being spent to improve and reduce cost here within the increasing constraints regarding environmental damage. New tire technology has produced tires for skidders 1295 mm wide with ground pressures less than 40 kPa. While more costly than conventional tires their use has resulted in reduced wood costs and lessened environmental damage. New materials such as kevlar have resulted in reduced tire damage and increased machine utilization. In forest areas, natural or planted, the choice of road system and its layout is important. The combined cost of road construction and transportation over the road should be minimized. The computer has made possible the development of optimized road layouts to minimize the costs of moving wood from any point in a forest tract to a central collecting point. The highway transport of wood is increasing at the expense of rail transport, particularly in the southern United States. Wood is transported in many forms, each of which has its advantages and disadvantages. Chips may be loaded into vans directly from the chipper spout while tree lengths are loaded one or two at a time. Highway regulations control the physical characteristics of hauling units but much can be done to increase the efficiency of truck transport - lighter tare weights through the use of light metals and plastics. More efficient fuel consumption is possible through the use of air deflectors and trailer design. At highway speeds of 80 km/hr, approximately 20 percent of the engine's horsepower is used in overcoming wind resistance. Trip recorders and vehicle tracking systems can be used to monitor and control wood deliveries.

Wood-handling operations from stump to consuming industry have developed according to Prof. Ivar Samset's theory of discontinuous evolution (Samset 1966). He outlines four stages in such an evolution as follows:

Stage 1: Economic pressure period where costs increase more than productivity when using traditional methods;

Stage 2: Development period when costs become too high in relation to productivity and economic pressure leads to intensive activity to arrive at a new method;

Stage 3: Introduction period when new method after testing is introduced with an increase in cost because of new expensive equipment;

Stage 4: Stabilizing period when new method is accepted and there is some increase in productivity. Costs increase over time and Stage 1 is repeated.

Frontiers in handling wood represent Stage 3 in such a continuum of activity, the stage when new technologies are applied in the forest to meet the challenge of increasing costs or declines in productivity. This stage offers scope for exploitative or developmental activity at a time when the limits of achievement have been reached.

Literature Cited

Samset, Ivar. The influence of mechanization on forest employment problems in developing countries. The law of discontinuous evolution. Technical Committee V, Sixth World Forestry Congress, Madrid, Spain; 1966. 37 p.

RESEARCH PRIORITIES AND ADVISORY COMMITTEES IN CANADA

Carl H. Winget

Director General, Research and Technical Services,
Canadian Forestry Service
Ottawa, Ontario, Canada K1A 1G5

Research advisory committees are increasingly used in Canada to evaluate priorities, reflecting a need for structured consultation among provinces, the private sector, universities and the federal government. While they are reasonably effective, concern is growing that proliferation because of competing interests will negate their usefulness.

In Canada, as in any country with an important forest resource, the simple identification of research needs as such is scarcely a difficult task. Almost any knowledgeable person can rapidly draw up a very long list of valid and interesting research topics in response to real needs, existing or foreseeable in a reasonably near future. It is, in fact, axiomatic that identified needs always far exceed available research resources. This leads to the always fascinating and all too often frustrating task of allocating priorities and resources in a context of competing needs and interests of user-clients. In large countries with a wide range of forest conditions and types, competition among regions further complicates objective analyses and stimulates controversy.

There exist a number of approaches and criteria to deal with the priorization of research needs at various levels of organization from the individual scientist to national programs. These range from the personal conviction of an individual researcher that he will very shortly have the answer to a question he considers crucial through seat-of-the-pants evaluations by research managers to elaborate point-rating systems involving a multitude of factors and to consultation, Delphi exercises and questionnaire approaches. To my knowledge, none of these approaches has proven infallible and none, if applied with a reasonable amount of caution, has proven useless. All have the danger of imposing a mystique and tradition which masks reality and the longer any one system is employed by any one person in any one organization, the greater the deviation from reality is likely to be. A reasonable rate of evolution of systems and/or the individuals applying them is desirable, if only to benefit from contrasting points of view and new insights.

One such approach which has traditionally been used in Canada is that of the forestry research advisory committee (FRAC). In fact, this approach seems to be gaining in popularity and will perhaps become more formalized and structured in the future. It does have the advantage of providing direct input from a variety of clients and collaborators in a federal system where the national government carries out a large part of forestry research. On the other hand, it is somewhat disquieting to recognize that some research organizations have abandoned this approach as being unnecessarily complex, unwieldly and, I gather, erratic. Certainly, in Canada, there are elements of doubt and uncertainty. My intention is to describe our use of this system and its perceived strengths and weaknesses, in the hope that subsequent discussion will clarify the constraints under which such a system should operate.

The Context for Forestry Research in Canada

As background to this discussion, it should be noted that constitutional jurisdiction and the land tenure system in Canada have created a fairly distinct separation of responsibilities and functions in forest management which impacts strongly on forestry research and its application. Because the Canadian constitution vests jurisdiction over forest land in the provinces rather than the federal government, the Canadian Forestry Service (CFS) has traditionally been strongly research oriented with only very minor responsibility for forest management. Even now, following the negotiation of federal-provincial forestry development agreements, the CFS has little expertise or involvement in directly managing forest land and lacks the perspectives and perceptions which that experience would bring. However, the CFS is the most important forestry research agency with university forestry schools as frequent collaborators. Conversely, provincial and territorial governments are responsible for the management of some 94% of Canada's forest land. While all provinces have supported forestry research to some degree, only three (Québec, Ontario and British Columbia) maintain an in-house research service and that of Ontario is currently in a state of flux. Forest industry harvests and, to varying degrees, manages forest land under a variety of leases, licenses, permits and forest management agreements with provinces, but few companies have much research capacity outside of the forest products field. To oversimplify, the land tenure system has created a situation where the federal government and universities carry out most of the forest management research, but do not manage forests, provinces do some research but are primarily responsible for planning and regulating forest management, while companies neither carry out much research nor own much land but are increasingly responsible for carrying out forest management and silvicultural operations through agreements with provinces. Although some of the most productive forest land in Canada is privately owned, this class of land tenure makes up only six percent of forest land, including holdings by large companies. Private landowners are significant clients and producers, especially in the east, but control relatively little land and carry out no research. The result is a rather distinct separation of major functions requiring a somewhat structured and formal consultation mechanism to integrate the various types of expertise relating to these functions into a coherent view of research priorities.

Origin and Structure of Forestry Research Advisory Committees

Given the separation of functions imposed by the forest land tenure system, it is scarcely surprising that research and development agencies, especially the regional research centres and institutes of the CFS, have established research advisory committees over the years. For the CFS, the lack of direct implication in forest management imposed the need for a consultative mechanism. While various incarnations and reincarnations of these FRACs have developed over time, representation on the committees has always included the provinces (and/or territories) as the major owners of forest land and the major regulators of forest management. It has normally included forest industry as well as university forestry schools as major research collaborators. At various times and places, membership has included woodlot owner's associations, forestry unions and professional foresters associations.

As far as the CFS is concerned, its research advisory committees are charged with identifying needs and setting priorities for those research needs. Where appropriate, coordination in the sense of defining complementary roles of collaborating agencies has also been undertaken by these committees. They also commonly comment and make observations concerning the appropriateness of technology transfer activities and on suitability and quality of information.

They do not normally consider questions of research methodology, quality of the science nor effectiveness of scientists. These aspects are usually dealt with by project and program evaluations involving peer review.

At this point in time, various initiatives to develop FRACs as consultative mechanisms have led to a rather bewildering array of such bodies. The CFS has the Forestry Research Advisory Council of Canada, a recently established body mandated to provide a national overview of forestry research policies and resource allocations. In addition, there exists a FRAC for each of the two national institutes of the CFS and a dozen FRACs or similar bodies providing for consultation between the six CFS regional research centres on the one hand and 10 provinces plus two territories on the other. Fifteen such committees seems a bit too much of a good thing, but this is only the beginning.

There are also a number of bodies reporting to other forestry research organizations and whose functions resemble or include those of FRACs. These are advisory to the three provinces which have their own research organizations or to independent research agencies such as the Pulp and Paper Research Institute of Canada, the Forest Engineering Research Institute of Canada and FORINTEK, the major Canadian forest products research agency. The CFS is represented on these committees.

Further, a number of bodies exist which do not report to forestry research organizations, but which offer advice on research priorities in a given discipline to all interested parties. These include the Canadian Tree Improvement Association, an independent body, and the Canadian Committee on Forest Fire Management which is sponsored by the National Research Council, an agency which does no research on forest fires. Again, the CFS is represented.

To further complicate matters, forestry research policies and priorities can be considered from time to time by advisory bodies whose mandate is one of broad forest policy advice, such as Canada's national forestry advisory body, the newly re-constituted Forest Sector Advisory Council or, at the provincial level, the Ontario Forestry Council.

Obviously, there is no lack of advice concerning forestry research policies and priorities. It is available from all interested parties on a national, regional and often disciplinary basis. The question arises as to how to deal with the mass of advice received.

Characteristics of FRACs

From my own experience, dealing with FRACs as a research manager is a rather frustrating exercise in that there are potentially major advantages in having these organizations but which tend to be countered by some inherent difficulties of structure and organization.

A rapid consideration of the major factors follows.

Advantages of FRACs:

1) This system provides a visible and readily identifiable mechanism for consultation with client-users of research results to define research needs and priorities.

2) It also provides for coordination, at least to the point of avoiding duplication, with major collaborators.

3) The system is flexible since membership can be readily adjusted from year to year depending on the interests and activities of clients and collaborators.

4) In addition to the major role of defining research priorities, information exchange and understanding of the research program of the establishment is readily obtained by important clients and collaborators.

5) Direct costs are relatively low, especially where regional FRAC's are concerned, because participating organizations are often willing to donate time and travel expenses for their representative in return for the information exchange and the opportunity to influence decisions.

Disadvantages of FRACs:

1) FRACs tend to proliferate. At this time, the CFS is involved with at least two dozen FRACs. Each jurisdiction and each research organization wants its own independent FRAC and there is considerable reluctance to, for example, share a single FRAC for all agencies in a given region.

2) FRACs can degenerate into routine, rubber-stamp bodies especially if membership remains stable for too long, if the agency being advised has limited flexibility to respond to new research needs or if the FRAC reports at too junior a level to be perceived as influential by its own members.

3) FRACs tend to find it difficult to deal with competing priorities across a broad field of research ie. tree breeding vs. budworm protection vs. impacts of acid rain and may retreat into generalities when faced with such problems, or may simply follow the lead of a particularly vocal member. Conversely, disciplinary FRACs tend to simply ignore competing priority areas and generate advice which is all-too-often an unrealistic demand for a much larger share of limited resources.

4) FRAC advice to different research agencies of a given organization such as the CFS can be of limited value if no coherent effort is made to consider the sum of such advice at a national level, to provide this input at the most senior policy level and to provide feedback to the regional and disciplinary FRACs.

5) Membership on any given FRAC must be restrained in terms of numbers. Large FRACs tend to become spectators rather than participants in research management. Conversely, failure to include key client organizations in FRAC membership can lead to negative reactions and loss of credibility.

Current Status of FRACs in Canada

At this time, the state of FRACs in Canada leaves much to be desired. The elements in place could provide the basis for a reasonably coherent and consistent system of consultation on research needs and priorities, but the participating organizations have yet to reach any concensus on the role, responsibilities, structure or mode of operation which could apply to FRACs as a whole. The following examples may serve to illustrate some of the conundrums.

British Columbia is our most important forestry province with roughly half of Canada's forest industry. The major forest management research agencies are the Pacific Forest Research Centre of the CFS, the Research Branch of the B.C. Ministry of Forests and the Forestry Faculty of the University of British Columbia with forestry-related research also being undertaken by Simon Fraser University and the University of Victoria. There exists a FRAC advisory to the Pacific Forest Research Centre, one to the B.C. Ministry of Forests and a third, the B.C. Forestry Research Council, originally established as a research granting agency but whose role is currently ill-defined. Consolidation seems desirable, if not imperative, but historical precedents and institutional constraints make this a sensitive issue.

In contrast, Québec, the second most important forestry province, has recently completed a major forest policy review and has proposed a single FRAC jointly recognized by all participants and interest groups. This is a most encouraging development.

Indeed, the Quebec proposal already exists in New Brunswick where the N.B. Forestry Research Advisory Committee is jointly recognized by the provincial government, the Maritimes Forestry Research Centre of the CFS, the Faculty of Forestry of the University of New Brunswick and the major forestry companies as the single FRAC in the province. This committee and its well-structured sub-committees have proven effective. However, while forestry is highly important to the economy of New Brunswick, the province contains only a small part of Canada's forest resource. Further, the two other Maritime provinces also depend on the Maritimes Forestry Research Centre and the University of New Brunswick for forestry research. The expansion of the N.B. FRAC to create a single Maritimes FRAC seems highly logical but runs counter to long tradition and provincial sensitivities.

As a final example, let us note the situation in Ontario, a major forestry province which recently created the Ontario Forestry Council, which is jointly recognized as being advisory to the federal and provincial governments. Originally conceived as a FRAC, its mandate was broadened to include policy advice on the full range of forestry issues and membership reflects this mandate. Consequently, its concern with research relates to broad policy and not to research programs and priorities. The Great Lakes Forest Research Centre of the CFS has therefore just established its own FRAC to obtain such advice. Clarification of the policy versus the program mandates of these two bodies seems essential if conflict is to be avoided.

My own view is that a joint policy and approach to FRACs must be developed in conjunction with provinces and industry if the very real benefits of these bodies are to be fully realized. First, the proliferation of FRACs must stop and the numbers be reduced. As far as the CFS is concerned, one FRAC for each of its six regions and one national FRAC would be ideal. In three cases, this would require agreement from provinces and territories to participate in a joint FRAC.

Secondly, FRACs should be developed and recognized as advisory not only to the CFS, but simultaneously to provinces, industry, universities and other clients and collaborators in forestry research within any given region. This situation now exists in the province of New Brunswick and is planned for the province of Québec.

Thirdly, the role of the national FRAC, the Forestry Research Advisory Council of Canada, in relation to regional FRACs must be clearly defined as one of integrating and consolidating regional views, of reporting to the most senior level of the CFS and of providing input to the senior forest policy bodies in Canada, the Forest Sector Advisory Committee and Canadian Council of Forestry Ministers.

Fourthly, the independent, discipline-oriented FRACs which now exist, such as the Canadian Tree Improvement Council and the Canadian Committee on Forest Fire Management should be viewed as advisory to the national FRAC and as providers of input to an integrated national view.

Finally, confusion exists as to the mandate, role and reporting relationships of FRACs in relation to broad forest policy advisory bodies at both national and regional levels. Mechanisms for providing interaction and input, although perhaps not direct reporting relationships, must be developed and definition of the complementary roles of these two types of bodies must be clarified.

CONCLUSION

The effectiveness and usefulness of FRACs seems to decrease as numbers of participants and committees increase. It may well be that Canada has reached or even passed the limit as far as numbers of participating jurisdictions, research agencies and clients are concerned. The current trend to broaden representation to additional interest groups should probably be restrained, at least until the current system is well established. Certainly, I would think that the development of such a system in a country with even more political jurisdictions, research agencies and forest management agencies than Canada should be approached with caution and, at least initially, on a regional basis.

In Canada, the continuing role of FRACs is reaching a decision point. Either an essential minimum of definition, structure and coherence will be attained in the next few years or the system will lose credibility and become ineffective. Unfortunately, the need for some such consultative mechanism would remain because of the structure imposed on the forest sector by constitutional jurisdictions and the land tenure system. A valid alternative to FRACs is not readily identifiable.

COMPARISON OF FORESTRY RESEARCH PLANNING AND MANAGEMENT IN IUFRO MEMBER COUNTRIES

FOR "Forestry Research Planning and Evaluation: New Frontiers and New Assessment Methods," IUFRO S6.06 and S6.06.01

Hans Gregersen and David Bengston

Department of Forest Resources
College of Forestry
University of Minnesota
St. Paul, MN 55108

Similarities and differences in the planning and evaluation of research in 117 forestry research institutes in 58 countries are discussed, based on a questionnaire survey. Results are compared across countries according to the stage of development (developed and less developed) and by the type of research organization (government and university). A follow-up study focusing on the organization and management of forestry research in developing countries is outlined.

The purpose of this paper is i) to summarize the results of an international comparison of forestry research planning and evaluation approaches and ii) to discuss future activity in this area. The survey reported on here is based on a questionnaire sent out to 153 IUFRO member institutions. One hundred and seventeen responses from 58 countries were received and these formed the basis for the present analysis.[1/]

The survey was part of a study aimed at analyzing similarities and differences in the ways in which institutions in different IUFRO countries approach research planning and evaluation. Another aim was to establish contact for more in-depth selective study of certain issues.

Probably the most startling conclusion coming out of the survey results is the fact that countries differ very little in many of their responses. In some cases there are certain significant differences between groups of countries, for example, developing and developed. However, in general, based on responses received, we are dealing with a fairly homogeneous population.

It is evident from the high response rate achieved of 76 percent -- which is very high for this type of international survey -- that there is a great deal of interest world-wide in exchanging information related to research planning and evaluation. This conclusion thus supports the concept of sharing experiences in this area through IUFRO groups, such as the present gathering.

1/ This work was initially reported on in a conference held in Thesaloniki, Greece, dealing with policy analysis for forestry development, August 27-31, 1984.

Summary of Survey Results[2/]

Nine questions were contained in the questionnaire survey. The results of responses for these nine questions are presented in Tables 1-9. Chi square tests for significant difference between responses for different groups were carried out for selected relationships. Some of the highlights for the significant differences are discussed below.

Stage of Development

Developed countries (DC) spent more time seeking outside funds than did less developed countries (LDC) (95%).[3/] Researchers were relatively more important than administrators in DC than in LDC in terms of influencing research program direction (90%) and selecting research projects (95%). In general, LDC administrators answering the questionnaire said that they used evaluations more than did their DC counterparts (from 90% to 99%, depending on the particular comparison made in this area). LDC institutions felt much more strongly than DC institutions that a key need in the future is more effort devoted to developing approaches to disseminate research results (99%).

Type of Organization

Two categories were compared, government (G) and university (U), since these are the main two types in IUFRO. There were only a few significant differences between these two groups. In general, the influence of researchers in decision-making is greater in U than in G. Thus, researchers' interests were more important in U in determining research program direction than in G (99%). And researchers in U made significantly more of the project selection decisions than in G (95%). One interesting difference is in the area of dissemination of results. G tends to use direct demonstration of research results much more than U (99%). U designates a significantly higher percent of its research as "social science" than does G (99%).

Researchers vs. Administrators as Decision-makers

We were particularly interested in finding out the extent to which researchers made their own project selections and in general influenced the direction of research programs. As pointed out above, researcher decisions tend to be relatively more important in DC vs. LDC. We also looked at some other commparisons related to this question. Thus,

-- the greater the percentage of project decisions made by administrators, the greater the use of formal evaluations (90%); while the greater the percentage of decisions made by researchers, the

---

2/ Adapted from Gregersen, 1984.

3/ Percentages in parentheses indicate levels of statistical significance associated with the particular relationship being considered, as calculated for the chi square test.

lower the use of formal evaluations (97%). Administrators apparently institute less monitoring activity when project selection decisions are made by researchers (98%). Also an interesting point is the fact that the greater the percentage of research project selection decisions made by reseachers the lower the importance given (by administrators) to improving the project selection process (98%). In other words, administrators feel comfortable with researcher decision-making on projects.

A number of other relationships between "use of evaluations" and other factors were evaluated. The interesting relationships are as follows:

-- the greater the importance of outside offers of funds and requests for proposals are in determining the direction of research programs, the greater is the perceived need to improve evaluation approaches (91%). This supports the idea that accountability is more stringent when funds are given from the outside on a project by project basis rather than in a lump sum or regular budget allocation.

-- the greater the use of formal evaluations, the greater: i) the percent of administrative time devoted to project planning (95%); and ii) the importance of commercial markets in determining the direction and content of research programs (92%).

Discussion of Results

The survey results reported on here indicate that both administrators from developing and developed countries perceive the researcher as being a main factor in determining both the direction and composition of research programs and research projects to be undertaken. This point has also been supported by a much more detailed study of U.S. research institutions.[4/] Based on this evidence as well as that from other documentation, researcher incentives are put forth as a major factor associated with research effectiveness. It follows that a much greater effort is needed to provide useful evaluation information for researchers themselves, in other words, information that can be used by researchers in carrying out more effectively their role in guiding research program and project directions. A key function of administrators is that of hiring researchers. In this sense, efforts need to be directed towards improving evaluation systems for administrators that focus on providing useful information on researchers when the time comes for hiring.

Research effectiveness and efficiency relate to research in use. In other words, until research has been translated into innovations and the innovations have been adopted by user populations, one can say little about the effectiveness of research or the usefulness of the results of research. In this sense, of direct concern to research planners is the question of dissemination of research results or diffusion of innovations. One outcome of the survey is a conclusion that techniques for disseminating research results are remarkably similar in developed countries and less developed countries (see Table 5). The exceptions are response to inquiries and use of lectures. In both cases, LDCs rate them significantly higher than did developed countries. A much stronger effort is needed to link research planning with the entire innovation diffusion process to ensure a more effective dissemination of research results.[5/] This applies both within national boundaries and across national boundaries in the quest to improve international technology transfer to build up indigenous research capacity in developing countries.

In terms of funding sources, a surprise result was the remarkable consistency in funding sources across regions. About 60 percent of both LDC and DC research institution funding comes from regular budget appropriations, while about 11 percent is from private grants and contracts, and 25-27 percent is from public grants and contracts (see Table 9). The funding situation is closely tied with two of the factors to be considered in the next stage study. One is the question of focus of research programs, and the other is the stability of research funding. Much more work is needed to evaluate the dynamics of budget formation in relation to program and project development over time.

In terms of the main theme of this meeting; namely, planning and evaluation of research, several additional implications emerge. First, with more than one-quarter of all funding being through contracts and grants, it might be useful to explore evaluation approaches used by grantors and contractors in making their decisions and in weighing results of grant and contract research. These approaches could be contrasted or compared with approaches used in allocating funds through regular budget appropriations. As a second point, it would seem useful to explore alternative types of data bases which would be useful to research planners, administrators, and researchers themselves, in developing their programs. In many specialty areas of forestry research, the networks which have been developed over time are not very strong. In other areas, such as pulp and paper, they appear to be quite strong.

The Follow-up Study

The next stage of this work will examine factors associated with effectiveness of forestry research in LDC's. We will not directly attempt to

---

4/ Gregersen, 1985.

5/ See Moeller and Seal, 1983. Also, a study of the innovation-diffusion process in the forest-based sector has been completed and a paper will soon be available from the College of Forestry, University of Minnesota. This work was supported in part by the U.S. Department o Agriculture, CSRS, and the Minnesota Agricultural Experiment Station.

measure efficiency of research and thus will not be able to analyze casual relationships involved. However, based on a synthesis of available documentation, we can identify organizational and managerial factors which are in general consistent with a productive research environment and effective research. Quantitative measures, either direct or using proxy measures, can be developed for many of these factors. Thus, it should be possible to characterize LDC forestry research environments and to assess, at least on a preliminary basis, some of the needs for overcoming constraints and moving towards improved research environments. The final step will be to develop insights into ways in which LDCs can strengthen their ability to be innovative in the forest resources research area and what resources are needed to carry out such strengthening activities. It should be stressed that this type of comparative study produces results which are complementary to country specific efforts and the efforts currently being undertaken by Mr. Oscar Fugali on behalf of IUFRO.

Five groups of factors related to research system effectiveness will be considered: These are as follows:

1. Research linkages including:

   A. Research to research linkages
   B. Research educational linkages
   C. Research to user linkages

2. Researcher incentive structures
3. Responsiveness to local needs
4. Focus of research programs
5. Stability of research funding over time

These factors have been found in previous studies and in the general documentation dealing with research planning to be closely related to the productivity of the research environment and effectiveness of research. By effective research, we mean research that is actually put into use through various innovations diffused throughout the relevant economic system.

Through a follow-up questionnaire survey to the respondents of the first questionnaire, and using results of other studies, the necessary information will be gathered to develop quantitative measures of the five factors mentioned above. At the same time, information will also be generated to be used in analyzing the variability in the five factors among countries, regions and institutions. For example, differences in researcher incentive structure between institutes might be hypothesized to depend on sources of funding, the extent of external influence in research funding, the importance of forestry in the national economy, and alternative research employment opportunities in the country. Independent explanatory variables will be chosen mainly on the basis of relevance to policy makers.

Each of the five factors mentioned above is briefly explained in the following paragraphs. A great deal of support for the importance of these factors is found in the results of past studies but it could not all be included in this brief discussion.

Extent and Nature of Linkages

The development of strong communication linkages has been identified as a key factor affecting the efficacy and productivity of a research system in developing countries (Clark, 1980; Biggs, 1982; many others). Stewart and James remark that "It has often been argued that the structure of research and development in LDCs is unsuited to generating technological capacity: it is argued that it is too formal, with insufficient links with local industry," (1982, p. 12). Some measure of the extent and nature of various linkages is therefore a good candidate for a research effectiveness indicator. Components used in constructing research linkage indexes could include the following:

- formal linkages with national research organizations;
- formal linkages with international research institutions;
- the extent of library facilities and information services;
- level of participation in IUFRO activities;
- formal linkages with national educational institutions;
- formal linkages with international educational institutions;
- percent of budget associated with technology transfer or extension.

Researcher Incentive Structure

Inadequate incentives for scientists have frequently been identified as a major cause of internal and external brain drain and low research productivity in LDCs (Moravcsik, 1976). Odera has observed that "a productive forestry research environment must consider the physical, social, remunerative, organizational, management and intellectual conditions under which the research personnel work," (1983, p. 207). Possible measures of the health of the researcher incentive structure include:

- frequency of use of various incentives, weighted by perceived effectiveness;
- urban amenities -- location in a major urban center or travel time;
- years or service of researchers -- high turnover indicates inadequate incentives.

Responsiveness to Local Needs

Research that is unrelated or unresponsive to local needs has often been identified as a significant constraint on the contribution of R&D to economic development in LDCs (Crane, 1977). The training of many LDC scientists in developed countries has been suggested as a cause for the focus on research topics of interest to the international scientific community rather than local problems. According to some observers, university researchers in developing countries face an "inevitable conflict between the quest for relevance to national goals and the pressure for meeting international standards of scholarship," (Rao, 1979, p. 27). Possible measures of this variable include:

- percent of project selection decisions made by researchers or administrators inside the institute (as opposed to decisions made by outside administrators);
- the importance of user group requests and a desire to solve social, environmental, or economic problems in determining the direction and composition of the research program;
- frequency of use of certain techniques to disseminate research results (e.g., popular magazines, response to inquiries from users, direct demonstration to users).

Relative Program Focus

Some observers have noted that a major weakness in the organization of research in LDCs is the tendency to undertake too many research projects with severely limited resources, rather than concentrating on a few priority projects (Moseman, 1970; Poats, 1972). It is assumed that, given the lack of research resources in many developing countries, a research program that is more focused will be more effective than a dispersed program. The degree of program focus relative to resources available could be measured by:

- the number of research problem areas (or projects) per full-time scientist, adjusted by the educational level or years of experience of scientists;
- a subjective rating by questionnaire respondents of the impact of a more focused program on research effectiveness.

Stability of Research Funding

Research is a long-term investment, and building up the research capacity of a developing country requires a sustained effort over several decades (Eicher, 1982, 1984; Ruttan, 1982). Financial and institutional instability has been identified as a major constraint on the productivity of forestry research in some LDCs (Odera, 1983). Possible measures of stability include:

- the percent of an institute's funding that comes from regular budget appropriations;
- a subjective rating by questionnaire respondents of the impact of instability of support from year to year, uncertainty about future funding levels, and delays in receiving allocated funds on research effectiveness.

These five organizational and managerial factors should serve as rough indicators of the effectiveness of forestry research in LDCs. The output of the analysis will provide some guidance to organizations involved in strengthening indigenous forestry research capacity.

Concluding Comments

We can sum up this brief discussion by suggesting that the results of an initial survey of IUFRO members indicate some striking similarities in the way organizations from quite different countries go about their planning and evaluation functions in forestry research. At the same time, some distinct differences exist between groups of countries as indicated in the text. Much more effort is needed to understand these differences and similarities and to develop background information for use in building more effective programs of support for research development in less developed countries and for support of more effective diffusion of research results, both nationally and internationally. The follow-up study described here will take a step in this direction.

References

Biggs, S. D. Institutions and decision-making in agricultural research. In: The Economics of New Technology in Developing Countries, F. Stewart and J. James (eds.). Boulder, CO: Westview Press; 1982: 209-224.

Clark, N. The economic behavior of research institutions in developing countries -- some methodological points. Social Studies of Science 10(1): 75-93; 1980.

Crane, D. Technological innovation in developing countries: a review of the literature. Research Policy 6(4): 374-395; 1977.

Eicher, C. K. Facing up to Africa's food crisis. Foreign Affairs 61(Fall): 151-174; 1982.

Eicher, C. K. International technology transfer and the African farmer: theory and practice. Working Paper 3/84, Department of Land Management, University of Zimbabwe, Harare, Zimbabwe. 1984. 34 p.

Gregersen, H. Forestry research planning and evaluation: an international comparison. In: Policy Analysis for Forestry Development, Vol. I, Proc. International Conference on Policy Analysis for Forestry Development, Thessaloniki, Greece, August 27-31, 1984. IUFRO; 1984: 157-171.

Gregersen, H. Forestry research planning and evaluation: a comparative study of U.S. institutions. Unpublished manuscript, College of Forestry, University of Minnesota, February, 1985.

Moeller, G. and D. Seal. Technology transfer in forestry: Proc. of a IUFRO Conference, Edinburgh, U.K. Forestry Commission Bulletin 61. London: Her Majesty's Stationary Office; 1983.

Moravcsik, M. J. Science development: The building of science in less developed countries. Bloomington, Indiana: International Development Research Center; 1976. 262 p.

Moseman, A. H. Building agricultural research in the developing countries. New York: Agricultural Development Council; 1970. 137 p.

Odera, J. A. Some major opportunities for strengthening forestry research in Kenya. In: Proc. of the National Workshop on Strengthening Forestry Research in Kenya. Government of Kenya and U.S. Agency for International Development; 1983: 184-211.

Poats, R. M. Technology for developing nations. Washington, D.C.: The Brookings Institution; 1972. 255 p.

Rao, K. N. University-based science and technology for development. In: Integrated Technology Transfer, J. Richardson (ed.). Mt. Airy, MD: Lomond; 1979: 27-35.

Ruttan, V. W. Agricultural research policy. Minneapolis: University of Minnesota Press; 1982. 369 p.

Stewart, F. and J. James (eds.). The economics of new technology in developing countries. Boulder, Colorado: Westview Press; 1982. 282 p.

Table 1. Question #1.
Of the total administrative time of your staff devoted to the following activities, what percent is spent on each activity? (All numbers represent percentages)

	Specifying and Developing Goals	Developing and Working with Budgets	Seeking Funds	Planning Projects	Other	Number of Cases
Government	20.8	26.7	10.8	40.3	0.4	64
University	23.4	19.5	20.0	36.4	0.7	44
Other	16.6	23.8	20.7	38.9	0	7
*Developed	21.7	23.3	17.0	37.7	0.3	68
Developing	21.6	23.9	12.5	41.3	0.7	44
Europe	20.3	23.1	18.9	37.2	0.5	46
Latin America	22.1	20.3	15.1	41.9	0.6	18
Asia (developing)	20.5	30.3	10.1	37.5	1.6	13
Asia (developed)	28.8	21.9	12.2	37.1	0	14
Africa	22.0	22.4	11.1	44.5	0	13
Mideast	16.7	33.3	3.3	46.7	0	3
North America	17.4	26.5	14.6	41.5	0	8

* "Developed" includes: Europe, Asia-developed, Canada, USA
"Less developed" includes: Latin America, Asia-developing, Africa.

Table 2. Question #2.
Please rate the following factors according to how important each factor is in determining the direction and composition of your organization's research program.
(All numbers based on a 1-5 rating scale (see below)).

	Laws and Formal Mandates	Desire to Solve Problems	Commercial Markets	Interests of Researchers	Interests of Administrators	Response to Request or Offers of Funds	Number of Cases
Government	2.7	4.0	2.9	3.3	2.9	3.0	62
University	2.3	4.0	2.7	3.8	2.6	3.4	44
Other	2.9	3.0	3.4	3.6	2.6	2.9	7
*Developed	2.4	3.8	2.6	3.6	2.7	3.1	68
Developing	2.9	4.2	3.3	3.3	2.9	3.2	41
Europe	2.4	3.9	2.5	3.6	2.7	3.2	46
Latin America	3.0	4.4	3.3	3.3	2.6	3.1	16
Asia (developing)	2.9	4.2	3.2	3.3	3.5	3.0	13
Asia (developed)	2.2	3.7	2.6	3.8	2.6	3.0	14
Africa	2.6	3.9	3.3	3.3	2.6	3.7	12
Mideast	2.3	4.0	2.7	4.0	2.3	3.5	3
North America	2.9	3.6	3.0	3.4	2.8	3.1	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

Rating Scale
1) no
2) slight
3) moderate
4) great
5) critical

Table 3. Question #3.
Please specify approximately what percent of research project selection decisions are made by each of the following.
(All numbers represent percents).

	Administrators Inside Organization	Administrators Outside Organization	Researchers	Other	Number of Cases
Government	28.8	22.6	41.1	7.5	64
University	20.6	23.4	55.6	0.4	44
Other	24.5	15.4	58.7	1.4	7
*Developed	22.9	21.1	49.8	6.2	68
Developing	29.9	24.6	43.4	2.1	44
Europe	23.4	23.6	46.1	6.9	46
Latin America	28,4	26.7	43.5	1.4	18
Asia (developing)	41.1	19.3	37.9	1.7	13
Asia (developed)	18.0	18.6	63.4	0	14
Africa	21.0	27.1	48.4	3.5	13
Mideast	13.3	20.0	66.7	0	3
North America	29.1	11.0	47.4	12.5	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

Table 4. Question #4.
Please rate the following uses of evaluations based on the rating scale below.
(All numbers based on a 1-5 rating scale, see below).

	Justification of Expenditures		Funding Support		Project Choice		Monitoring Ongoing Activities		Number of Cases
	Formal	Informal	Formal	Informal	Formal	Informal	Formal	Informal	
Government	2.0	3.2	2.4	3.7	2.6	3.6	2.5	3.5	64
University	2.0	3.2	2.4	3.7	2.3	3.4	2.4	3.2	45
Other	2.0	3.1	3.0	4.0	2.0	3.0	2.4	1.3	7
*Developed	1.8	3.1	2.2	3.8	2.1	3.7	2.1	3.7	69
Developing	2.4	3.4	2.9	3.5	3.1	3.3	3.0	3.0	44
Europe	1.8	3.2	2.3	3.7	2.0	3.7	2.1	3.7	47
Latin America	2.8	3.4	3.2	3.6	3.2	3.8	3.0	3.3	18
Asia (Developing)	2.0	3.5	2.0	3.7	2.7	3.2	3.0	2.9	13
Asia (Developed)	1.8	2.8	1.6	4.2	1.9	3.3	1.9	3.6	14
Africa	2.2	3.2	3.3	3.2	3.2	2.9	2.9	2.8	13
Mideast	1.7	2.7	3.0	3.3	2.7	3.3	1.7	2.7	3
North America	2.0	3.1	2.4	4.1	2.5	4.1	2.5	3.9	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

Rating Scale
1) Never used
2) Seldom used
3) Used about half the time
4) Used frequently
5) Always used

Table 5. Question #5.
Please indicate the frequency with which you use each of the folowing techniques to disseminate research results.
(All numbers based on a 1-5 rating scale, see below).

	Professional Journals and Meetings	Popular Media	Reports to Research Contractors	Response to Inquiries	Direct Demonstration	Lectures	Number of Cases
Government	4.1	2.7	4.1	3.7	3.2	2.8	64
University	4.2	3.0	4.3	3.3	2.6	3.5	44
Other	4.1	2.6	4.9	3.6	3.4	2.4	7
*Developed	4.3	2.6	4.2	3.4	2.9	2.8	70
Developing	4.0	3.0	4.1	3.7	3.1	3.3	44
Europe	4.3	2.4	4.3	3.5	3.0	2.7	48
Latin America	4.3	2.8	3.9	3.5	2.9	3.7	18
Asia (developing)	3.6	3.0	4.1	4.0	2.9	2.0	13
Asia (developed)	4.5	3.1	3.9	3.1	2.6	3.1	14
Africa	3.9	3.1	4.5	3.8	3.4	3.4	13
Mideast	4.7	4.0	5.0	4.0	2.3	5.0	3
North America	4.0	2.9	3.9	3.8	3.3	2.9	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

Rating Scale
1) Never used
2) Seldom used
3) Used about half the time
4) Used frequently
5) Always used

Table 6. Question #6.
Please indicate the importance of the need to improve each of the following activities in your organization.
(All numbers based on a 1-5 rating scale, see below).

	Identifying Objectives	Selection of Projects	Monitoring Progress	Evaluation of Results	Dissemination of Results	Number of Cases
Government	2.9	3.1	3.2	3.5	3.5	64
University	3.0	3.1	3.3	3.4	3.5	44
Other	3.1	3.0	4.0	3.3	2.7	7
*Developed	2.8	3.0	3.1	3.2	3.1	69
Developing	3.1	3.2	3.5	3.8	3.9	43
Europe	3.0	3.0	3.3	3.3	3.1	46
Latin America	3.1	3.1	3.5	3.8	3.9	18
Asia (developing)	3.3	3.3	3.6	3.6	3.9	13
Asia (developed)	2.3	2.9	2.8	3.1	2.9	14
Africa	3.1	3.2	3.5	4.0	3.9	12
Mideast	4.3	4.0	3.3	3.7	4.7	3
North America	2.6	2.8	2.8	2.9	3.1	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa

Rating Scale
1) No
2) Slight
3) Moderate
4) Great
5) Critical

Table 7. Question #7.
Please indicate the approximate percent of your organization's budget associated with each of the following types of activities.
(All numbers represent percentages).

	Basic Research	Applied Research	Technology Transfer	Other	Number of Cases
Government	14.8	66.5	16.8	1.9	65
University	25.1	58.6	16.3	0	45
Other	11.3	55.6	28.8	4.3	7
*Developed	20.6	63.6	15.1	0.7	70
Developing	14.8	62.0	20.8	2.4	44
Europe	16.1	66.5	16.4	1.0	48
Latin America	15.9	63.2	16.2	4.7	18
Asia (developing)	13.2	60.9	25.9	0	13
Asia (developed)	34.4	56.0	9.6	0	14
Africa	14.9	61.6	22.0	1.5	13
Mideast	25.0	56.7	18.3	0	3
North America	23.5	59.1	17.4	1.0	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

Table 8. Question #8.
What percent of your organization's research would you designate as:
(All numbers represent percentages).

	Biological Physical	Social Science	Engineering	Inter-disciplinary	Other	Number of Cases
Government	65.5	7.5	13.4	13.5	0.1	64
University	53.8	14.0	15.5	15.9	0.8	45
Other	45.0	4.1	33.4	16.2	1.3	7
*Developed	60.1	11.2	15.4	12.7	0.6	69
Developing	59.0	8.0	15.5	17.4	0.1	44
Europe	59.1	11.0	15.9	13.2	0.8	47
Latin America	56.8	9.2	21.4	12.6	0	18
Asia (developing)	66.9	10.4	10.0	12.7	0	13
Asia (developed)	62.9	11.7	14.9	10.5	0	14
Africa	54.2	3.8	12.7	28.9	0.4	13
Mideast	61.7	3.3	16.7	18.3	0	3
North America	61.1	11.6	12.9	13.4	1.0	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

Table 9. Question #9.
Indicate the approximate percent of your organization's research funds that come from:
(All numbers represent percentages).

	Regular Budget Appropriations	Private Grants or Contracts	Public Grants or Contracts	Other	Number of Cases
Government	76.0	3.9	16.8	3.3	65
Unviersity	40.1	18.5	40.4	1.0	45
Other	39.0	21.2	23.4	16.4	7
*Developed	59.4	10.7	27.3	2.6	70
Developing	59.3	10.8	25.6	4.4	44
Europe	55.2	9.6	31.5	3.7	48
Latin America	46.5	10.7	36.4	6.4	18
Asia (developing)	79.5	6.1	12.1	2.3	13
Asia (developed)	68.8	15.1	16.1	0	14
Africa	56.5	15.8	23.9	3.8	13
Mideast	86.9	1.8	11.3	0	3
North America	69.0	9.2	21.8	0	8

* "Developed" includes: Europe, Asia-developed, Canada.
"Less developed" includes: Latin America, Asia-developing, Africa.

INDUSTRY-FEDERAL-UNIVERSITY COOPERATION: DESIGNING AND ENGINEERING STRINGER PALLETS

William C. Baldwin

Executive Vice President,
National Wooden Pallet and Container Assoc.
Washington, D.C. 20036

I would like to start by first thanking Denver Burns for inviting me to the IUFRO's 6.06 and 6.06.01 Meeting. I am excited to be here with you because our story has the potential to bring new life to the age old problem of funding and justifying needed research. This problem is most acute in the lumber and wood products business because of the lack of wood-oriented technical knowledge on the part of the vast majority of the 15-20,000 owners of lumber and wood products companies. Most members of the industry are bootstrap entrepreneurs who probably never heard of MOR or MOE, the Budworm, or the short rotation of hybrid poplars.

If they can steal enough moments away from the daily problems of running their businesses to think about research at all, they quickly become bewildered by the titles of wood research published reports. If they are of a cheerful nature, they shrug off such musteries to them as "probably for the good...must be worthwhile to someone or so much effort and such brain power wouldn't have been spent on it." However, if a research report hits them at a time when their problems are about to overwhelm them -- and that's most of the time -- they become critical and think -- "What a waste of money. Who cares about hybrid poplars or the Budworm anyway. Fracture Mechanics, you've got to be kidding."

Let's face it, we wouldn't be sitting here today if a real gap didn't exist between wood research and the wood using industry whom we are supposed to serve. My goal here today is to explain what we in the pallet and container industry are doing to close this gap, to span this chasm that exists between wood research and wood users. This gap is really no one's fault, but is very real. Most of us researchers have as little knowledge of what it takes to be an entrepreneur as have these entrepreneurs of hybrid poplars and fracture mechanics. How many of us know how to look a banker in the eye to get a line of credit to meet next Friday's payroll? How many of us have ever had to explain to our widowed mother-in-law why we want to buy a new band saw because she's got a half interest stake in the company? How many of us have ever sold a load of lumber to someone who hasn't paid for it and now won't? If we traded places with him for a week we'd botch up his projects just as bad as he'd botch up ours. Yes, this gap between the forest user and the wood researcher is very real. However, while he doesn't always understand us or we him, in a real sense we need him more than he needs us.

He is the one who pays the bills. He employs 80% of all the people in the industry. This small businessman logs, saws, and fabricates the vast majority of all the wood sold. The results of his labor are found in every household in the country.

The point here is that we exist because of him since in economic terms, he is the creator of wealth. His genius is the reason for our prosperity so it is in our interest to do the best job we can to help him in solving his problems. If we have failed in the past, if his criticism of us is at all justified, it is not because he has failed to understand what it is we do, it is because we have either failed to solve his problems or have failed to communicate properly to him the solutions we have come up with for his benefit. In others words, the monkey is on our back to make our work relevant to his needs.

Which leads to the NWPCA definition of "research." Until 1977 we had thought of research in terms of a "collection of technical projects on subject of interest to those who are interested in such things." Please don't laugh at such a naive definition. We have always been most serious about research. What this definition really says is that we were operating in the vacuum of special and limited interests.

Almost from the founding of this industry in WWII, FPL had done testing and design work on wooden pallets and when NWPCA was founded in 1953 we took an early interest in Madison's work. Several universities also started testing programs. VPI&SU, U.C. Davis, N.Y. State College of Forestry, and Colorado State come to mind as institutions doing pallet related work in the industry's formative years. By 1964 NWPCA had organized its Research Advisory Committee who wrestled for 13 years trying to coordinate and come up with project ideas for these institutions. In those years, we were just like everybody else involved in research. None of our members understood what it was our Committee was doing, our Committee was trying to understand what the researchers were doing, and despite a lot of great work being done, nothing was working its way down to practical usage by our industry. The taxpayers, private companies, and members were paying for it all. We had even funded the construction of the Pallet & Container Research Laboratory at VPI&SU. However, what enthusiasm there was from our new industry for research was beginning to wear thin as we moved through the 70's because of lack of payback.

And that's the way it was until 1977, when St. Paul was knocked from his horse by a bolt of lightning. In this case the bolt of lightning was cast by Dr. Robert Buckman when, in response to our question of "why doesn't the Forest Service do more for our industry?", he replied, "Well, what is it that your industry needs?"

It was at that point, under the leadership of our Research Chairman, Thomas N. DePew, NWPCA formally adopted a new definition of research which was: "finding solutions to the most pressing problems of the greatest number of our members." With this new definition as a basis, our Research Committee reasoned that if its mission was to solve the most pressing problems of the greatest number of members, rather than to try and guess at what those needs were, we should go straight to the horse's mouth and ask our members what it is they wanted.

The key point here -- to ask those who will be affected what it is they want from research -- was such a simple concept that we had completely missed it until Dr. Buckman asked us the question. We had been guilty of the worst possible sin a researcher can commit -- intellectual arrogance. We had assumed that because most members ignored our research committee they weren't interested and, therefore, we high priests who were active in research should decide what the problems were for "the poor dears. Daddy knows what's best for you." The result of this intellectual arrogance was a mass of uncoordinated ineffectual special interest projects with no clear breakthroughs. The amount of published information was exceeded only by the yawns they evoked from the people we professed to be helping.

So we asked our members what they needed. With the help of Dr. Walter B. Wallin of the Forest Service, and Mr. Thomas N. DePew, Chairman of our Research Committee, we created a 13-page Research Needs Questionnaire. Contrary to the nay sayers who said no one would even bother to read something that long, let alone fill it out, two-thirds of our members replied to the survey. This astounding number of responses laid to rest forever the myth that the businessman is not interested in Research. You bet he is.

From the Survey, our Committee identified four priority needs, one of which -- to develop an easy-to-use design system for wooden pallets -- was selected as the most achievable within a reasonable period of time.

The next step was to develop the necessary funding. First, we went to our membership individually and in small groups. Because we were now taking on a problem they said needed to be solved -- 100 members, large and small, anonymously pledged $250,000 to what we now called PRP, the Pallet Research Project. Simultaneously, Dr. William Lavery, the President of VPI&SU agreed to match this with an additional $250,000 in funding, and Dr. Robert Buckman agreed to match both amounts with scientist man years dedicated to PRP work. While not a new concept, the cooperative PRP Contract that resulted was unique in that for the first time both an industry, a university, and the U.S. Forest Service developed and funded jointly a research project to solve a problem identified through a consensus survey of the industry. Throughout the ensuing four years of PRP, the enthusiasm for the project and its momentum was maintained. Schedules were met and the finished product, what we now call PDS, a Computer Diskette, for designing wooden pallets is now being enthusiastically used by our industry and our customers. Even during the dark days of the 1982-83 recession budget cuts, PRP survived unscathed from David Stockman's OMB audits. In fact, it was used as an example of how other public research projects should be structured and funded. OMB's theory was that if the research was really needed, then the industry would pay for it. PRP was a unique case history of where this was actually happening.

Research accountability and results need not be difficult to measure if independent institutions cooperate, compete and challenge one another on a joint project. Excellence is the inevitable result. There is no way the administration at VPI&SU would ever default on their part of such a joint venture. They wouldn't be caught dead doing anything less than their best with colleagues from the Forest Service and funders from industry working at their side. The scientists with the Forest Service weren't about to fall down when asked for help by an industry of small businesses that uses 18% of all the lumber produced and were putting their money where their mouth was. Nor could the industry default on its funding during the darkest days of the recession when its members were wondering if they could even meet Friday's payroll, let alone a contribution to the Association Research Fund. No one involved was about to admit to the others that things may have been tough internally and that they couldn't do their part, so they continued on, found ways to resolve their respective internal problems and brought the project to a successful conclusion. The three-way partnership set an atmosphere of challenge and an attitude of "can do" that prevailed throughout.

To conclude, then, PRP worked for the following reasons:

1) Research was defined as finding solutions to the most pressing problems of the majority of our members;
2) We asked our members what their problems were; and
3) We used the challenge concept for funding, planning, and implementation of the project.

Unstated up to now, but a fundamental assumption is that top-flight, qualified individuals were responsible for all the thinking and doing that went into this precedent-setting Research Project. Because so many were involved and the purpose of this paper is otherwise, I cannot mention the several hundred individuals by name who made PRP work. However, I can draw a picture of the many qualities that these people possess. The PRP person is successful -- successful in his business, his laboratory, or his managerial role. He is professional in that he does his work competently and thoroughly. He is an expert in his field. The PRP person has an open mind and is dedicated to pushing the frontiers of knowledge ever outward. He is dedicated to excellence and when the chips are down will do that little bit extra it takes to make the impossible probable and the probable a reality. The PRP person knows how to set an objective for himself and others and then drives himself or leads his team onward to that goal. The PRP person is a man of honor and integrity.

If I have just described the world's most perfect person, that is why PRP succeeded. To be more precise, what PRP did because it bridged the gap between the user and the researcher was to draw out all those good qualities that are in each of us and focused this vitality to the objective of solving a real problem for a group of very real people. The reason PRP worked and the reason why other projects can work like it is because each of us has in us a desire to be a part of something greater than ourselves, a desire to plant the flag a little bit further up the hill.

The formula we used works, the raw material is in each of us, our challenge to you is to make it happen in other areas. If it is your desire to leave a mark on the world, to be a part of meaningful research, then perhaps our formula can help you leave this mark. If you define your mission as finding solutions to the most pressing problems of the majority of your constituents, if you ask these constituents what their needs are, and then encourage these constituents and other affected parties to jointly sponsor the project -- to be a part of its planning, funding, and implementation, you most assuredly will plant the flag a little bit further up the hill.

UNIVERSITY-FOREST LANDOWNERS COOPERATION:
THE REGIONAL FOREST NUTRITION RESEARCH PROJECT

S. P. Gessel and H. N. Chappell

Professor Emeritus and Research Associate,
College of Forest Resources,
University of Washington,
Seattle, WA 98195

Introduction

This paper reviews the development and accomplishments of a cooperative research program in the Pacific Northwest: the Regional Forest Nutrition Research Project (RFNRP). The program was initiated in 1969 to provide information on the effects of fertilization in stands of second-growth Douglas-fir (*Pseudotsuga menziesii* [Mirb.] Franco) and western hemlock (*Tsuga heterophylla* [Raf.] Sarg.). Geographic scope of the program includes forested lands west of the crest of the Cascade mountains in Oregon and Washington, a timberland area of about 23.9 million acres (9.7×10^6 ha). Ownership is divided among federal, state, and private (Table 1). Federal lands include 9 National Forests in Oregon and Washington and Bureau of Land Management lands in Oregon. The Bureau of Indian Affairs is also involved through management of forests on a number of Indian reservations. State lands represent a number of state interests with management by the Washington Department of Natural Resources and the Oregon Department of Forestry. Private ownerships are forest industry lands, ranging from several million acres to a few thousand, and non-industrial holdings, generally small-acreage tree farms.

The area of cooperative study extends from the California border in the south to British Columbia in the north (Figure 1). It covers an elevational rise from sea level to 6,000 feet (1800 m) over a wide range of soils, climates, and forest sites. Principal species in the study are Douglas-fir and western hemlock, with emphasis on Douglas-fir forests.

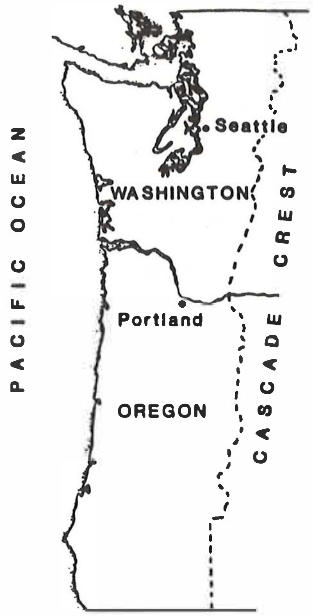

Figure 1. Geographic scope of the Regional Forest Nutrition Research Project.

Research History

As this Cooperative was formed to secure management information on the use of fertilizers in forestry, a brief review of background research is in order along with a general discussion of institutional ways of developing research information.

The preponderance of federal ownership of forest land in the region, coupled with extensive research programs of the Forest Service, led most forest landowners to historically very low investments in forestry research. This was certainly the case when the senior author arrived at the University of Michigan in 1948. The prevailing expectation among landowners was that the Forest Service would do all necessary research. Funding for research at universities and major forest landowners was very low. However, general concepts about the value of forest land research and who should do it changed rather rapidly during the 1950s. The development of the Weyerhaeuser Company research program began at that time.

Forest Nutrition Research

In the early 1950s, legislation passed in the State of Washington allocated a portion of liquor revenues into a biological research program. Under a broad interpretation of the law the College of Forest Resources was able to secure some of these funds for forestry research. Earlier work by Dr. Richard B. Walker of the University of Washington Botany Department and the senior author had demonstrated field and greenhouse growth response by Douglas-fir to an improved supply of nitrogen. This new research funding allowed the establishment of a small but active field and laboratory research program in forest tree nutrition.

Growth increases up to 50% were demonstrated in otherwise unmanaged forests after application of 200 pounds of nitrogen per acre (224 kg N/ha). Pilot trial aerial applications proved to be equally effective in generating response. Much initial work was done at the University of

Table 1. Timberland area by ownership classes for western Washington and Oregon.[1]

State	National forest	Other[2] public	Forest industry	Other private	All ownerships
	acres x 1000				
OREGON	4585	2860	3780	2442	13,667
WASHINGTON	2454	1815	3709	2229	10,207
TOTAL	7039	4675	7489	4671	23,874

[1]Data from Bassett and Oswald 1981a, 1981b, 1982, and Gedney 1982.

[2]Includes Bureau of Land Management, Indian lands, and state lands in Oregon and Washington.

Washington's Charles Lathrop Pack Demonstration Forest, which represents a very limited sample of soil and stand variables existing in the forests of Washington and Oregon. Despite this restricted database, landowners became interested in the potential economic return from application of nitrogen. Thousands of acres of second growth Douglas-fir and western hemlock stands, naturally established after removal of the original forest cover, held the potential for rapid returns on fertilizer investments. The increasing stumpage prices for wood over the period of development of this research made investments in fertilizer with the projected response financially attractive. The continuous projections of catastrophic wood shortages by many and acceptance of this by conservationists and the general public, lent support to programs which could increase wood supply through improved nutrition on a stable or reduced timberland base.

As a result of these early research results and large areas of second growth forests, aerial application of nitrogen fertilizer increased rapidly in the Pacific Northwest. Nitrogen fertilizer was relatively cheap, as was application cost, and many landowners began fertilizer programs without any kind of backup research.

We have developed this picture of a small research program producing results which attracted the attention of landowners with a wide array of forest conditions in order to show that the environment was ideal for development of a cooperative program. Suppliers of fertilizer were also interested in potential markets when they viewed the large forest area of Western Washington and Oregon. Another fortunate circumstance which helped the orderly development of a cooperative effort was intense interest in the role of soil information in forest land management. This led to the organization in 1948 of a regional group, the Northwest Forest Soils Council, whose role was to promote research on the subject of forest soils and the dissemination of relevant information. This committee was (and still is) active in this role and has been a major force in many areas of forestry. Membership includes representation of all forest landowners, including federal and state agencies. This well-organized committee served as the focal point for the development of this cooperative research endeavor in the Pacific Northwest.

Development of the Regional Forest Nutrition Research Project

Early in 1967 it became apparent that more information was needed to carry on cost-effective fertilization programs. It also was clear that information needs spanned a broad range of forest soils and stand conditions, and that individual approaches would not suffice and would be prohibitively expensive. Therefore, the Northwest Forest Soils Council began a series of meetings to consider the problem and develop a plan of action. Principal research objectives were developed, along with a research design to obtain necessary data and the administrative and financial structure to manage the cooperative. The University of Washington was invited to administer the program due to the considerable expertise in forest nutrition already developed in the College of Forest Resources.

Structure and Function of the RFNRP

From the outset of development of the cooperative two kinds of participation were deemed necessary for success. These were participation of landowners and others to fund the program and participation of a scientific group to plan necessary research. These two goals were accommodated by establishing a Liaison Committee composed of representatives from all landowners or agencies participating financially in the Cooperative, and by the formation of a Technical Advisory Committee of working forest scientists, not necessarily associated with any of the financial cooperators.

The Liaison Committee represents interests of landowners or agencies by annually reviewing research programs and approving budgets for necessary research. This group also serves as agents for dissemination of results within their organizations before general release and publication. To further this latter activity the Cooperative has sponsored short courses and conferences to report and discuss results. These courses also served as a mechanism to get cooperator input to the research program.

The Technical Advisory Committee works with the College staff and the RFNRP Director to develop research plans to provide land managers with the best advice possible regarding forest fertilization. The overall research design was also a responsibility of this committee. The Technical Advisory Committee meets as needed, and in recent years has convened 2 or 3 times annually.

The ultimate job of carrying out the research programs and reporting results is the responsibility of the College of Forest Resources. An early decision in the program was that establishment, treatment and measurement of research plots was to be done by the College. Past experience in forestry field measurements had established that consistent and reliable field data are an absolute necessity. Therefore, a field crew was assembled and trained in the necessary techniques of site selection, plot establishment, initial measurement, and remeasurement. The same approach has been used throughout the program. Cooperators provide field study areas and protect them from management activities, and assist in thinning treatments and transporting fertilizer, but plot record-keeping, treatment, and analysis has been the sole responsibility of the College of Forest Resources.

Basis for Participation

Much early organizational discussion was aimed at a suitable financial plan that would provide sufficient funds to operate the program and also allow cooperators to contribute relative to their land ownership. A geographical province concept was developed which served both as the financial assessment base and for research planning. Western Washington and Oregon cover a wide range of latitude, altitude, soils, and climates. Research study sites had to be located to sample at least the most important of these combinations. Therefore, the research area was divided into nine provinces (Figure 2) with boundaries selected to achieve as much uniformity as possible within the unit in terms of soils and climate. For example, we had reason to believe that southwestern Oregon would produce quite different growth response results than the Puget Sound Basin. Provinces also became the basis for cooperator assessment. A standard payment, changing over the years with inflation and research expansion, was set for cooperators' land in each province. For those cooperators not holding land (such as fertilizer companies), donation of fertilizer or a cash payment was accepted. Annual program budget through 1984 is given in Figure 3; organizations participating in the Cooperative are listed in Table 2.

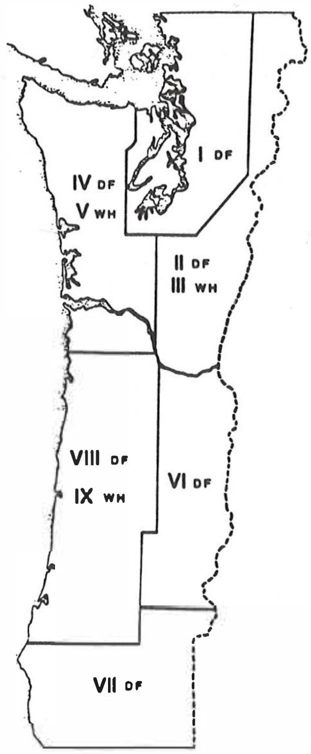

Figure 2. Douglas-fir (DF) and western hemlock (WH) provinces of the Regional Forest Nutrition Research Project.

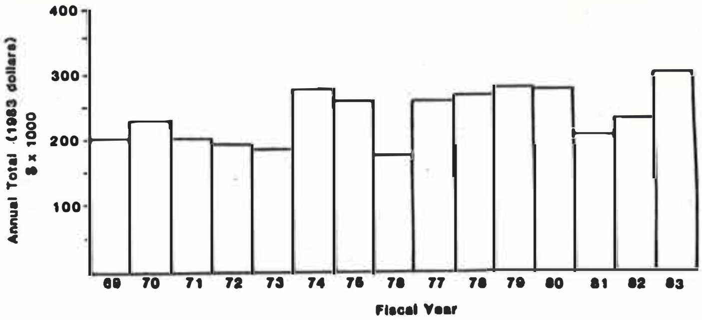

Figure 3. RFNRP budget summary (in 1983 dollars), from project initiation through June 30, 1984.

Research Design

Forest tree nutrition research in the Douglas-fir region prior to the development of this program had established that deficiencies in nitrogen supply existed in many forest soils. Field tests had demonstrated rapid and large growth response to nitrogen. Form of nitrogen had also been investigated, leading to the conclusion that urea nitrogen was effective. However, nitrogen had not been applied to a wide range of sites, stand ages, and species across Washington and Oregon in order to determine response. Little was known about growth response to amount of nitrogen, as well as duration of response. There was some information to indicate that nitrogen stimulated larger trees in a stand more than smaller ones, and would intensify competition. This prospect was viewed as an alternative to thinning.

Table 2. RFNRP Member Organizations

* Barringer and Associates, Inc.
* Boise Cascade Corporation
* Bureau of Land Management
Champion International Corporation
* Chevron Chemical Company
* Cominco American, Inc.
Crown Zellerbach Corporation
Fruit Growers' Supply Company
Georgia-Pacific Corporation
Giustina Bros. Lumber & Plywood Co.
Hampton Tree Farms, Inc.
* International Paper Company
ITT Rayonier, Inc.
* Longview Fibre Company
* Menasha Corporation
Murray Pacific Corporation
* Plum Creek Timber Company
Pope & Talbot, Inc.
Publishers Paper Company
* Quinault Indian Nation
* Reichhold Chemicals, Inc.
* Rex Timber Company
St. Regis Paper Company
Scott Paper Company
Simpson Timber Company
* State of Oregon Forestry Department
* State of Washington Department of Natural Resources
* Union Oil Company
* U.S. Forest Service
* Weyerhaeuser Company
* Willamette Industries, Inc.

* Organizations currently participating

As a result of the knowns about nitrogen, and an assessment of the unknowns, a standard experimental design was developed for the Cooperative which could be applied to any stand condition. Financial limitations did not permit a complete exploration of a nitrogen response curve, so 0, 200, and 400 lbs. nitrogen/acre (0,224, and 448 kg.N/ha) as urea were selected. Treatments were replicated so that a standard installation consisted of 6 plots: 2 control, 2 200 lbs N, and 2 400 lbs N. Measurement plots were generally 0.1 acre (0.04 ha), surrounded by a treated buffer. Measurement intervals were established for 2-year growth periods. As it was impossible to establish and treat all installations in the first year, one half were established during each of the first and the second years. As a result, remeasurement is a yearly task with analyses on a biennial basis. All trees on a measurement plot are individually tagged and diameters measured, but height measurements for site and volume determination are taken only on a sample.

The initial phase of the research program dealt only with fully-stocked second-growth stands of Douglas-fir and western hemlock. An attempt was made to select single-species stands, however, some installations included a minor hardwood component as well as other conifer species.

Because of changing forest management objectives and cultural activities over the life of the Cooperative, subsequent phases of research have shifted to other stand conditions. These primarily include stands with stocking control, either by initial planting spacing or pre-commercial and commercial thinning. Further discussion of RFNRP objectives and experimental design was documented by Hazard and Peterson (1984). The current phase of the program stresses a more precise determination of response for each stand condition (RFNRP 1984).

Functioning of the Regional Forest Nutrition Project

Although cooperators provide funds for most direct program costs, the University and the College have dedicated many resources to the program. Salaries of the senior author, the long-time Director of the Cooperative, and Dr. K. J. Turnbull, the original project mensurationist, were supplied by the College. More than 25% of these scientists' time was devoted to the program. Other faculty members have been associated with the Cooperative primarily through graduate student programs. College staff have contributed to project management, budgeting, accounting and support services, as well as in training of field personnel. The College and University have also contributed offices, computers, and data storage.

Reliable and accurate field work is an absolute necessity for continued success of a project of this kind. Therefore, project management has made a special effort to maintain a highly qualified field party. We have accomplished this and achieved efficiency and economy by integrating this work with other College projects. A project supervisor carefully plans the field work for best efficiency with respect to seasonal climates at different locations and with other work. The efficiency and accuracy have been accomplished by:

1. Making field personnel available for other College research programs during slack periods.

2. Establishing a contractual program for landowners who wish to add to their plot network by establishing additional research plots. However, it should be pointed out that landowners have generally made results on these plots available to all cooperators. Although a major share of the budget has been expended on field activity, it is not disproportionate to the importance of this effort.

Another substantial share of the budget has been devoted to data management and analysis, and publication of results. This has been a continually expanding task as the database has grown with the acquisition of more plots and growth measurements over 15 years. An initial objective of the program was to devote about 25 percent of the budget to basic research carried out by graduate students and faculty at the University. The actual amount spent in any one year has varied relative to availability of students and other program needs with total expenditures on graduate research averaging about 20 percent.

The program has opened many opportunities for sharing personnel and facilities with other College activities, thereby making for greater efficiency. Program supervision, data management and analysis, and fieldwork have been accomplished generally without full-time employment commitments.

Related Research

One of the advantages and strengths of a cooperative research effort centered at a university is its ability to attract other kinds of related research and focus these efforts on program goals. This is true for both funding and research personnel in the form of graduate students and faculty.

The RFNRP was focused on growth response of forest trees to essential elements. Questions about movement and eventual fate of applied nutrient elements were not part of the research program. However, the fact that the Cooperative was underway and the intense forestry interest in tree nutrition helped the establishment of a related research program in mineral cycling in forest ecosystems. Presentation of the details of that program are beyond the scope of this paper except to say that the focus was on movements of essential elements in forest soils and (in the case of nitrogen) potential losses from the system. This program initially received separate National Science Foundation funding, but with the establishment of the Coniferous Forest Biome research program at the University of Washington, it became part of that effort.

The Coniferous Forest Biome Program research funded by the National Science Foundation over a period of 8 years contributed a great deal to our understanding of coniferous forests, especially productivity. The fact that the University and the College were able to successfully compete for the position of lead institution in this federally funded program was certainly due, in part, to already active programs in conifer forest research such as the RFNRP. Biome research results definitely expanded our understanding of processes within the soil and the tree which control forest productivity. This, in turn, improved our interpretive ability about response information from the RFNRP. There are other examples of interrelated research including a large program in the disposal of municipal sewage from the City of Seattle onto forest lands.

The regional project is known internationally and the research has profited from contact with and site visits by many forest scientists. As a specific example, a considerable amount of the foliar analysis examining possible nitrogen-sulfur interrelationships and potential sulfur deficiencies has been done by the New South Wales Forestry Commission in Sydney, Australia. We are extending this cooperation with them in an effort to develop more site specific fertilizer recommendations for both nitrogen and other elements which may be deficient in Northwest forests.

Results

This paper is not designed to recount a history of project results, but will focus on a few overall criteria. In terms of output, over seventy reports and publications have been produced by the program through 1984. Many other scientific papers, presentations and graduate theses have used Project data, including twelve master's and doctoral theses at the University of Washington.

The Cooperative now has more than 2500 permanent growth plots to measure response of forest trees to elemental additions, mainly nitrogen. Trees in each individual plot are measured every two years, but because of the split establishment time, measurements go on each year and updated results are reported each year. After analysis and review, results are first presented to cooperators at the annual Liaison Committee meeting and through an internal report series. These are then accumulated into Biennial Reports (e.g., RFNRP 1985) which detail and interpret results, along with results from related projects. These reports are first distributed to cooperators, then sent to domestic and foreign educational institutions and research groups. Scientific papers are prepared, as appropriate, by staff, associated faculty and graduate students for publication and presentation at meetings.

The success of the cooperative effort and the efficiency of the results could be judged in a number of ways. We add some of these for the reader to further evaluate the program. First, at the outset the program was set up for five years. Decisions have been made to continue the program at the conclusion of three five-year segments and the same decision has now been made for another 5 years. These decisions have been made in the face of many vicissitudes in the general forest economy.

Second, results from the program form the basis for operational fertilization programs for Douglas-fir in the Pacific Northwest. Recommendations reflect changing forest practices especially with respect to thinning programs and initial stocking. Results show a consistent response by Douglas-fir to nitrogen across age classes and growing conditions. Western hemlock response is less consistent and no general fertilizer recommendations have been developed for this species at the present time. More than 2 million acres of forest land have been fertilized based on results from the RFNRP and related research projects. (Fig. 4)

Finally, the RFNRP was the subject of an independent study funded by the Forest Service in the project on methods of evaluating forestry research. The report (Bare and Loveless 1985) comments favorably on the overall success of the RFNRP, and notes that the annual investment in the project has been about 3 percent of the annual investment in forest fertilization for member landowners. The internal rate of return (after taxes) for the joint investment in the RFNRP plus operational fertilization was calculated to be 9 to 12 percent.

Role of Cooperatives in Future Research Strategies

This is a conference on forestry research planning and evaluation. Cooperative research projects should be considered as an effective strategy to meet long-term research needs. The importance of the role of cooperatives is emphasized by the financial problems facing many forestry research efforts at this time. The economic situation facing forest industry is manifested by reduced profits, corporate restructuring, takeovers, and sale of major forest landholdings. This has resulted in much less funding available for forest management and forest management research, in both the public and private sectors. However, there is a great need for good research information which addresses specific problems and can be used to improve cost-effectiveness of management.

In the Pacific Northwest, the need for high-quality information on the effects of silvicultural practices is demonstrated by consideration of current stand management practices and management plans. In a recent survey, RFNRP cooperators reported planting more than 1.5 million acres (6.1×10^5 ha) during 1978-1982, with the majority as Douglas-fir plantations (1.2 million acres [4.8×10^5 ha]) (Chappell and Opalach 1984). Naturally-established stands, especially for western hemlock, will significantly augment the establishment rate for the young Third Forest of the Pacific Northwest. Survey respondents also indicated silvicultural activities planned for recently established stands (Table 3). Responses show a distinction between forest industry and agencies, but overall there is a clear indication that stocking control and fertilization will exert considerable influence on growth and yield of future stands. Other factors such as genetic selection, improved nursery practices, and vegetation management

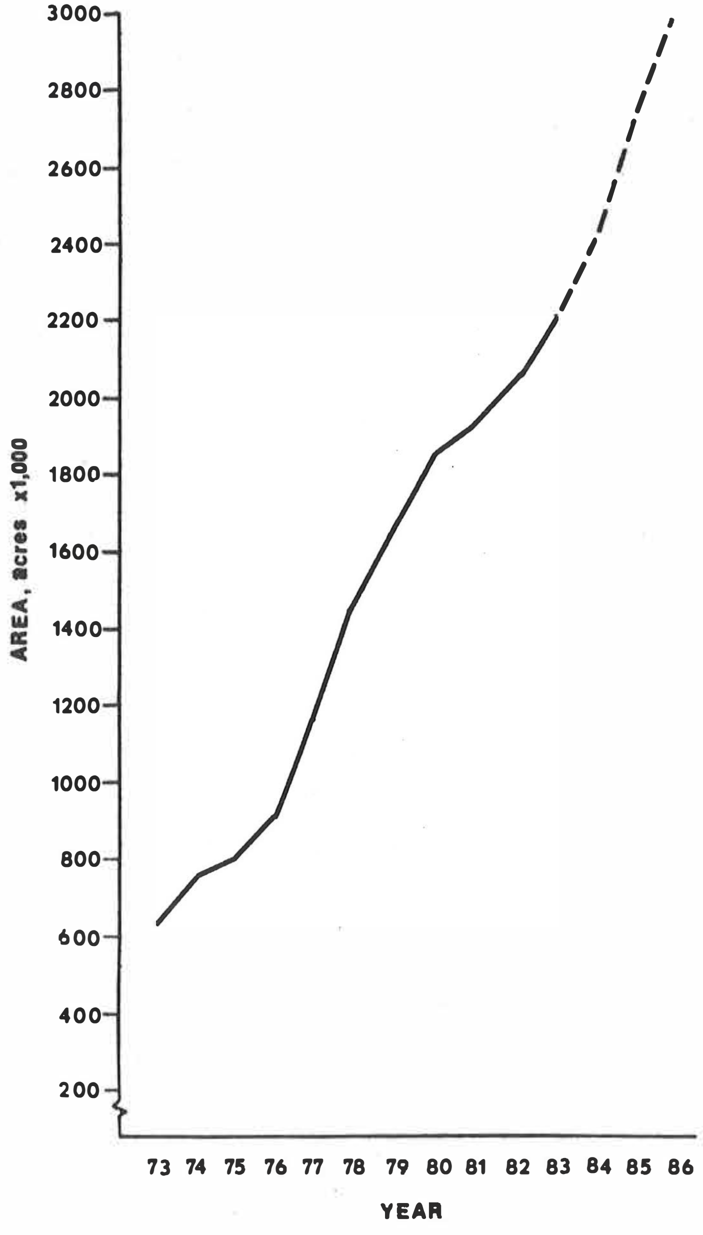

Figure 4. Cumulative area fertilized through 1982 in western Oregon and Washington. Planned fertilization indicated by broken line.

Table 3. Silvicultural activities planned for recently planted stands, western Washington and Oregon

	Area planted 1978 - 1982	Precommercial thinning	Commercial thinning	Fertilization
	Acres (ha) x 1000	----------% of area planted	------------	
Forest Industry	670 (271)	73	42	52
State and Federal Agency	833 (337)	67	67	23
Total	1,503 (608)	70	56	36

from Chappell & Opalach 1984

will also influence growth, and accurate projections of future yields must consider the effects of treatment interactions.

Cooperative programs have demonstrated the approach is an efficient and cost-effective way to meet present and future research needs, especially those which require answers covering a broad spectrum of site conditions. Individual organizations are generally unable to support research programs on the scale necessary to effectively address regional questions, especially in the current economic climate. The increasing number and complexity of management questions facing foresters, however, requires long-term research programs to develop information. This leads us to the conclusion that research cooperatives will play a greater role in research information development in the next decade. Cooperatives in the future must continue to be responsive to the near-term needs of their clients, and also must provide the stability to pursue long-term research goals.

Literature Cited

Bare, B.B. and R. Loveless. 1985. A case history of the Regional Forest Nutrition Research Project: investments, results and applications. Report submitted to USDA For. Serv. North Central For. Exp. Sta. under proj. no. PNW 82-248, Coll. For. Resour., Univ. Washington, Seattle. 53 p.

Bassett, P.M. and D.D. Oswald. 1981a. Timber resource statistics for southwest Washington. USDA For. Serv. Resour. Bull. PNW-91. 24 p.

Bassett, P.M. and D.D. Oswald. 1981b. Timber resource statistics for the Olympic Peninsula, Washington. USDA For. Serv. Resour. Bull. PNW-93. 31 p.

Bassett, P.M. and D.D. Oswald. 1982. Timber resource statistics for the Puget Sound area, Washington. USDA For. Serv. Resour. Bull. PNW-96. 31 p.

Chappell, H.N. and D. Opalach. 1984. Forest fertilization and stand management western Oregon and Washington: status and prosepct. RFNRP Report No. 1, unpublished report on file at Coll. For. Resour., Univ. Washington, Seattle, WA. 23 p.

Gedney, D.R. 1982. The timber resources of western Oregon -- highlights and statistics. USDA For. Serv. Resour. Bull. PNW-97. 84 p.

Hazard, J.W. and C.E. Peterson. 1984. Objectives and analytical methods of the Regional Forest Nutrition Research Project. Inst. of For. Resour. Contrib. No. 53, Univ. Washington, Seattle, WA. 23 p.

Regional Forest Nutrition Research Project. 1984. Proposal for five-year extension of research (Phase V), July 1, 1985 - June 30, 1990. Unpublished. Coll. For. Resour., Univ. Washington, Seattle, WA. 34 p.

Regional Forest Nutrition Research Project. 1985. Biennial Report 1982-1984. Coll. of For. Resour., Univ. Washington, Seattle, WA. 35 p.

THE FISH/FORESTRY INTERACTION PROGRAM:

A CASE HISTORY OF COOPERATION IN CANADA

V.A. Poulin, Principle, V.A. Poulin and Associates Ltd., 2153 West 46th Avenue, Vancouver, B.C. Canada V6M 2L2
and
D.R. Macdonald, Director General
Pacific and Yukon Region, Canadian Forestry Service, 506 West Burnside Road
Victoria, B.C. Canada V8Z 1M5

The organization and preliminary results of a special study team formed to investigate the impacts of forest harvesting practices on fish habitat in the Queen Charlotte Islands are described. The advantages and disadvantages of a program directed by an interagency steering committee and carried out by scientists from government agencies and private contractors is discussed.

Complex jurisdictional responsibilities and lack of definitive information on the impacts of multiple resource use often result in problems that defy solutions at local and provincial levels. In British Columbia, resolving forestry conflicts through jointly sponsored cooperative research is one approach where Federal, Provincial and Industry supported agencies have met with success in dealing with some critical resource use issues.

British Columbia, like many of its sister provinces, is fueled by a resource based economy. Timber harvesting is B.C.'s leading industry, and the same environmental conditions that create some of Canada's most productive forest lands also support valuable commercial and sport fisheries. British Columbia's forest lands are currently logged at a rate of approximately 75 million m^3 of timber annually (Timber Management Branch, BCMOF, estimated projection). Streams which drain the land base yield about 22 million salmon annually and have the potential for some 37 million with future enhancement (Pearse 1982). Of the freshwater sport catch, 75 percent of the creel is produced by natural habitat and therefore the province places high priority on maintaining production from forest lands.

While the forest industry has been the mainstay of the economy for years, stands of old growth forest remain to be cut before a silvicultural objective of converting old-growth forests to managed second-growth stands is met. Unfortunately, much of B.C.'s remaining timber supply is found on terrain that is increasingly more sensitive and less economical to log. It is for this reason that logging is, with increasing frequency, embroiled in resource use conflicts that involve interaction between fisheries and forestry.

Resolution of land use problems hinges, now more than ever, on our ability to understand what is at stake, and how to satisfy the needs of other user groups. In practice, the biologist needs to better understand the geomorphologist, the geomorphologist the forester, the forester the biologist, and so on. Given the divergent opinions of different groups, solutions are best achieved through cooperative research rather than by independent studies.

The Fish/Forestry Interaction Program

One of the most dramatic illustrations of the need for cooperative research emerged in 1978 after a series of rainstorms triggered large numbers of landslides that severely damaged fish habitat in forest lands on the Queen Charlotte Islands. Many slides originated in logged-over lands, and were symptomatic of larger problems that dated to the time when easily accessible timber was exhausted and forest operations moved onto steep terrain. Steep-slope logging utilized cable yarding systems and mid-slope roads, portions of which were constructed by crawler tractors. Many of the watersheds cut were highly susceptible to natural failure and landslides and erosion followed.

In British Columbia, jurisdictional responsibility for management and protection of fish and fish habitat is shared by the Canada Department of Fisheries and Oceans which has its mandate under the Federal Fisheries Act and the Fisheries Branch of the British Columbia Ministry of Environment. Under agreement with the Province, the Federal Fisheries' responsibility is restricted to salmon and their habitat, while the Province assumes responsibility for all other freshwater and anadromous trout. These agencies work through a referral system to provide input and recommendations to the British Columbia Ministry of Forests which is empowered to develop and manage the forest and range resources of the Province. The system works well. Each year approximately 200,000 hectares are logged and 4,000 referrals are successfully processed.

A formal protocol agreement is also in place whereby contentious issues at the field level can be elevated to reach mutual resolutions. The ultimate decision concerning how or where to log, however, rests with the Ministry of Forests. When the referral system fails, protection of fish habitat falls on the litigative power of the Federal Fisheries Act. The act specifically prohibits anyone from destroying fish or undertaking harmful alteration or destruction of fish habitat through direct damage or introduction of deleterious substances into water frequented by fish.

The landslides triggered by the 1978 storms prompted a public outcry on the Queen Charlottes that elevated the issue of what, where and how much could be logged and still preserve downstream fishery values. A serious jurisdictional dispute also arose between the provincial and federal agencies concerning a particular hillside that subsequently failed and deposited sediment and debris into an important salmon stream. In the aftermath of the storm, the three agencies which had jurisdiction over the resources agreed to establish a cooperative framework for dealing specifically with steep slope logging problems. Part of the resolution of the dispute was the establishment of a task force which later recommended to the Ministers that a major interdisciplinary research program be undertaken on the Queen Charlotte Islands. The program became the "Fish/Forestry Interaction Program "(FFIP).

Overall objectives of the program were to:

1. Provide documentation on the extent and severity of mass wasting and assess impacts on fish habitat and forest sites;

2) Investigate the feasibility of rehabilitating stream and forest sites damaged by landslides;

3) Assess the use of alternative silvicultural treatments for maintaining and improving slope stability by establishing and maintaining thrifty root systems; and,

4) Investigate the feasibility and success of reducing logging-induced failures through the use of alternative logging methods including skylines, helicopters, and by improved logging planning.

The FFIP began in 1981. Although it was originally conceived as a 4-year study, it was extended to cover a 5-year period when it became obvious that more work was needed to address many of the fundamental questions. The program is now in its final year and activities are now largely directed towards final analyses, writing technical reports, and planning possible Phase II studies.

Organizational Structure

The program reflects strongly the importance placed on cooperative efforts, in funding as well as in ensuring the credibility of the work. The program is financed through direct appropriations from agencies representing two levels of government - the Canada Department of Fisheries and Oceans, the B.C. Ministry of Forests, and the Fisheries Branch of the B.C. Ministry of Environment. However, it was clear from the outset that the Canadian Forestry Service and industry, through the Forest Engineering Research Institute of Canada (FERIC), would have a key role through provision of much-needed scientific expertise, manpower and logistical support. Total funding for the 5-year program is 1.45 million dollars excluding support provided by the participating agencies through seconded salaries, overhead, and field operations not reimbursed by the program.

The organizational structure of the program is illustrated in Figure 1.

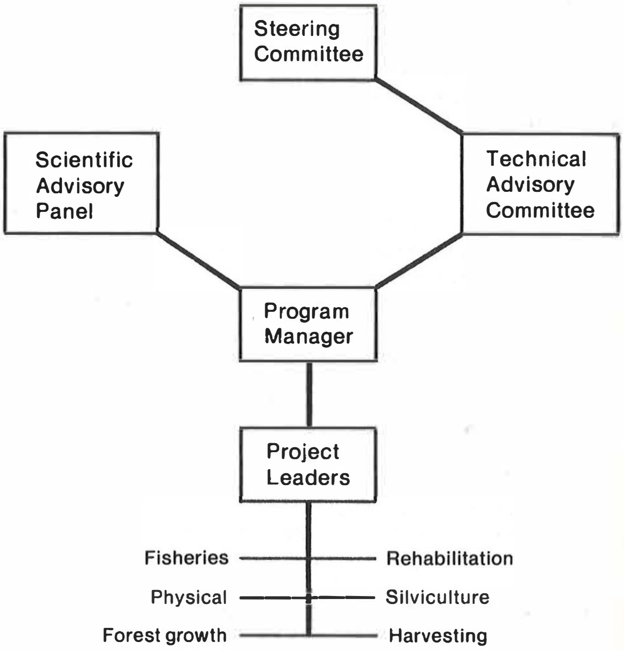

Figure 1. Fish/Forestry Interaction Program Organizational Structure

It consists of several supervisory committees which provide executive and technical direction to a program manager who is responsible for the research. The program manager and most senior project leaders are contract personnel, and as such are not affiliated with the funding agencies. This adoption of an "arm's length attitude" was established to maintain the objectivity of the Research team, and to develop trust among the agencies and industries concerned with the issues.

An important element of the organizational structure is the Steering Committee, which, in itself is an example of interagency cooperation and commitment to problem solving. The committee is made up of senior executives from the funding and participating agencies. Members of the committee in large part hold positions enabling them to develop and finalize agency policy. Since committee members have the power to enter into formal funding agreements, it is unnecessary to go outside the program framework to secure funds. The Steering Committee had a major role in establishing the scope and direction of the study. Now that the program is well underway, the committee meets on average every 4 to 6 months to provide overall policy direction and to monitor progress. These briefing "updates" provide opportunity for committee members to discuss preliminary results and develop a stronger understanding of resource interactions. This process of elevating information to the policy forming level is more direct than the traditional approach of synthesizing applied results from primary publications. All too often the latter requires an inordinant amount of time, and some information may not reach the level where decisions are made.

Several other important components of the organizational structure include a Technical Advisory Committee (TAC) and a Scientific Advisory Panel. The TAC is the working arm of the Steering Committee and consists of managers and senior researchers from the sponsoring and participating agencies. The committee provides technical direction and reviews and approves all working plans, technical reports, and budgets submitted by the program manager. Contact between senior members of the TAC and the Steering Committee is made as required, usually within the same agency. This line of communication circumvents normal departmental channels and thus establishes a responsive link between all administrative positions. To provide advice on project study design, and serve as reviewers for program publications, a panel of distinguished experts was invited to serve on an advisory panel to the program. Members of this committee include scientists from B.C., Washington and Alaska.

The contract status of the program manager and most project leaders adds another dimension to the program structure. As a contractor, the program manager has the ability to cross agency boundaries and cut through administrative red tape. He has full fiscal control over program contracts and disbursements, thereby allowing him to manage projects on a day-to-day basis independent of the funding agencies. The manager is responsible for developing all budgets, working plans, logistics and schedules, staffing, preparation of reports, and coordination of the interdisciplinary studies.

Project leaders report directly to the program manager. Because of success in seeking support for the program, a total of 14 individuals, whose backgrounds include fishery biology, geomorphology, forest ecology, bio-engineering, silviculture, and forest engineering, have become involved with the research. The project leaders have included:

- Researchers seconded from Federal and provincial ministries;
- University post-graduate students;
- Senior industry researchers; and,
- Contract FFIP staff.

Program Achievements

With final reports on several key projects yet to be completed, it is too early to anticipate the outcome of the program. Some preliminary results are highlighted, however, to demonstrate the effectiveness of the interdisplinary approach undertaken. It is important to understand that FFIP is an applied, integrated study best described as an environmental impact assessment with objectives that include problem solving. In this respect, the goals of the FFIP are to: first, define the problem; second, describe the impact; and, third, develop prescriptive solutions.

Defining the Problem: Extent of Mass Wasting

Aerial photography studies were used to establish the linkage between logging and potential downstream impacts by examining the extent and severity of mass wasting in terms of numbers and volume of debris delivered to streams. Using 1:50,000 air photography, approximately 8,000 slope failures were inventoried across the Queen Charlotte Islands (Gimbarzevsky 1983). These data further indicated that landslide activity of one failure per km^2 or greater occurs on nearly one-third of the 10,000-km^2 land base. In a similar study, landslide frequency related to logging was studied in 27 basins using 1:12,000 scale air photographs. Logging was shown to increase the number of failures from 2.8 per km^2 in unlogged terrain to 10.7 failures per km^2, an absolute increase of approximately four times (Rood 1985, in press). Estimates of the volume of sediment and debris entering streams were needed to relate geomorphological changes observed in stream channels to increased sediment input. Calculations derived from the air photography suggest that slides originating in logged areas increased mean annual sediment delivery to streams by 20 times. FERIC engineers further examined 103 of these slides to determine the specific causes of the failure. Common causes were sidecast failures, insufficient drainage, lack of road maintenance, and yarding disturbance.

Stream Channel Response

With information describing the extent and severity of mass wasting and relationship to logging being compiled, other studies were undertaken to examine the effects of mass wasting on fluvial processes and channel geomorphology. Outcomes of the channel studies have reinforced our understanding of the importance of woody debris for creating and maintaining stable fish habitat in Queen Charlotte streams, which are typically devoid of rock ledges and boulders. In stream debris, tree boles and large branches serve as hydraulic controls that manipulate stream flow to produce pool and riffle sequences, carve stream depths, and create low flow refugia for fish under banks and amid the debris itself. Other studies have been undertaken to examine stream flow responses to rainfall and the transport of introduced sediments through the stream systems.

Impact Assessment: Effects of Mass Wasting on Fish Habitat

Depending upon the magnitude and slope position of an event, a slope failure can have minimal to catastophic impact on a small coastal stream. Of the 400-odd streams which drain Queen Charlotte forest lands, about 94 percent are first and second order systems (Northcote 1984 pers. comm.). By matter of size they commonly are steep gradient, small streams that originate in gully headwalls at high elevation but can yield up to 2000 to 3000 salmon annually. When slope failures occur in areas lacking adequate runout zones, the debris can enter the nearest gully network and flow downslope with immense force. These events, known as debris torrents, have the most obvious and severest impact on fish populations.

In stream reaches utilized by fish, debris torrents can reduce stable debris by 40 to 82 percent, and pool habitat by 45 to 65 percent. Stream width can be increased by 50 to 70 percent, having the effect of isolating the stream from adjacent banks. increasing channel braiding and, in some cases, causing extreme dewatering. Streams subjected to mass wasting are also highly susceptible to removal of gravel by scouring. Scour at salmon spawning sites has been measured to depths of 35 cms, with egg losses as high as 65 to 95 percent.

Overwinter survival, the number of fish surviving from summer to seaward migration, provided a measure of the cumulative effects of stream alterations attributed to major events. In several torrented streams, survival ranged from 1.7 to 1.9 percent compared to 14.7 and 36.3 percent in two stable systems. The underlying outcomes of the fisheries studies suggest slides on the Charlottes have a tremendous potential for inflicting serious losses to small stream anadromous and resident fish populations. In fact, survival values in some of the severely affected streams are inadequate to maintain the natural stocks.

Forest Site Productivity

Another component of the impact equation is the effect of mass wasting on forest site productivity. Forest growth, vegetation succession and soil development studies have provided data on the implications of slope failures on forest sites (Smith 1983, 1984). These studies have shown that vegetation establishments on slides is much delayed compared with adjacent logged terrain. The upper one-third of a slide, for example, barely reaches 80 percent cover in about 100 years time compared to 30 years on adjacent slopes. Rapid invasion by alder on the lower one-third of most Charlotte slides produces similar crown density in about the same time as adjacent logged terrain, but then dominates the stand for 30 to 60 years. The suppression by alder on the lower portions of slides and poor soil conditions on the upper portions reduce conifer stock levels and growth rates resulting in approximately a 50 percent reduction in timber volume by the time of the second rotation (70 years).

Prescriptive Solutions - Alternative Harvesting Systems

Timber harvesting studies undertaken by FERIC have focused on finding alternative harvesting methods to reduce the number of logging induced failures. Study results have suggested that with improved site data, careful planning, yarding systems suited to the terrain and improved road construction and maintenance practices, forest engineers could prevent 30 to 40 percent of the logging related slides. Problems such as water diversions, ditch blocking, placement of material on unstable fill slopes, and lack of maintenance on abandoned roads are traditional issues that result in a significant number of logging road failures. Yarding disturbance caused by lack of deflection, sidehill yarding, yarding through gullies and yarding roads through sensitive areas were factors contributing to clear-cut failures. Recommendations emerging from the research include using better terrain and topographic maps to design yarding systems tailored for unstable slopes, ensuring adequate deflection, reducing concentration of logs over yarding roads, and designing, maintaining and constructing stable logging roads that do not adversely affect the natural drainage patterns of hillslope gully systems.

Other Alternatives

Additional work undertaken by the program has included literature reviews on forest rehabilitation (Carr 1983), silviculture, and stream rehabilitation (Bustard 1983) measures to help identify techniques which may be applicable to the Queen Charlottes for mitigating the impact of landslides. Several stream rehabilitation techniques have also been field-tested with promising results, including debris replacement in streams and the use of gabions to improve spawning habitat.

Preliminary Evaluation of Organizational Approach

Recognizing that the full story is not in yet, we see considerably more advantages to this type of cooperative program structure than disadvantages.

Primary Advantages

1. Applicability and usefulness of the data

   Cooperative studies provide a more realistic perspective because they avoid divergent opinions and individual agency bias. Applicability of study results is inherently increased and there is greater opportunity to focus on broader issues.

2. Interagency committee structure.

   This type of structure promotes dialogue, understanding, and interaction among policy makers, managers and technical staff from participating agencies.

3. Higher profile research.

   Joint studies make it easier for administrators engaged in partnership agreements to priorize research funding and establish fixed timing and budgetary commitments. Fiscal constraint notwithstanding, partnership agreements lock in participating agencies and thereby ensure the successful completion of the program.

4. Lower costs.

   Research dollars are usually spent to the limit, making it difficult for any single agency to engage in many high profile programs. Cost sharing is an obvious way to reduce the funding burden of agencies, branches, or departments within agencies.

5. Consolidated results.

   Presentation of results in a single package rather than numerous independent reports increases the probability that the results will influence policy.

Secondary Advantages

6. Contract staff.

   "Arm's length" contracting of program management and staff positions eliminates the natural bias of agencies and increases credibility with industry and other concerned public sector groups. Costs are lower since contractors are hired only for the period required.

7. Agency independent program management

   Red tape is reduced and agency boundaries can be crossed without difficulty. Responsive day-to-day fiscal management permits smooth operation of field logistics and overall budgetary control. Costs are reduced through lower overhead and purchase of only required materials, supplies and equipment.

8. Availability of qualified staff.

   Participating agencies offer greater scope in the range of experts potentially available to undertake sub-component studies within the overall framework of a major program.

9. Reduced administration.

   A lead agency handles administration of a single contract to the manager who supervises and manages sub-contractors on behalf of the program. Participating agencies simply journal voucher their share of program costs or provide the funds by interagency contract.

10. Increased probability of participation.

    Well structured programs are viewed positively by potential participants, increasing likelihood of their involvement.

Disadvantages

1. Maintaining time frame.

   The larger the study the greater the number of players, making dollars and scheduling more difficult to keep on track.

2. Larger committees.

   Cooperative studies require representation from all participating groups. The larger the committees, the greater the chance for divergent opinions. At technical committee levels, where decisions on study design, reporting format, staffing, and allocation of funds are made, this problem can be particularly significant. If all groups do not perceive they are being served equally, decisions can be delayed an inordinant amount of time, or not made at all.

3. Product delivery.

   Contracting services are an attractive alternative to solving agency staffing freezes, but they make it more difficult to guarantee the product. Even with management supervision and monetary penalties built into contracts, ensuring that contractors meet the terms of reference can be difficult in non-performance situations where individuals are not part of some larger corporate structure.

4. Part-time contracts.

   While contractors reduce program costs when they are not required on a full-time basis, it is difficult to ensure their availability for subsequent projects should more lucrative projects present themselves.

Prospects for the Future

Proposals for a Phase II study are currently being developed for consideration by the Steering Committee and funding agencies. The program envisaged focuses on information transfer and implementation while also pursuing some new initiatives and studies aimed at filling existing data gaps. Strong support for continuation of the program exists within industry and all technical advisory levels. Ad budgets continue to tighten in Canada, we see more and more smaller independent research projects being continued or winding down. Greater emphasis is being placed on cooperative research, no doubt reflecting the many advantages discussed in this paper. While the prospects for some projects are dim, we are optimistic for a Phase II continuation of the Fish/Forestry Interaction Program.

BIBLIOGRAPHY

Bustard, D.R. 1983. Queen Charlotte Islands stream rehabilitation studies - a review of potential techniques. Fish/Forestry Interaction Program Working Paper No. 8 Vancouver, B.C. 31 p.

Carr, W.W. 1983. Watershed rehabilitation options for disturbed slopes on the Queen Charlotte Islands. Fish/Forestry Interaction Program Working Paper No. 10. Vancouver, B.C. 40 p.

Church, M. 1983. Concepts of sediment transfer and transport in the Queen Charlotte Islands. Fish/Forestry Working Paper No. 2. Vancouver, B.C. 30 p.

Gimbarzevsky, P. 1983. Regional overview of mass wasting on the Queen Charlotte Islands. Fish/Forestry Interaction Program Working Paper No. 3. Vancouver, B.C. 35 p.

Klassen, H.D. 1983. Spawning area improvement using gabions - 1983 progress report. Fish/Forestry Interaction Program Working Paper No. 9. Vancouver, B.C. 56 p.

Pearse, P.H. 1982. Turning the tide. A new policy for Canada's Pacific Fisheries. The Commission on Pacific Fisheries Policy. Final Report. Vancouver, B.C. 292 p.

Rood, K.M. 1985. An aerial photograph inventory of the frequency and yield of mass wasting on the Queen Charlotte Islands. Ministry of Forest Land Management Report (in press). Vancouver, B.C.

Sauder, E.A. 1984. Utilizing alternative logging methods to reduce mass wasting. pp. 223-230, In: C.L. O'Loughlin and A.J. Pearce, eds., Proc. Symposium on Effects of Forest Land Use on Erosion and Slope Stability. Honolulu, Hawaii, May 7-11, 1984.

Smith, R.B., P.R. Commandeur and M.W. Ryan. 1983. Natural revegetation, soil development and forest growth on the Queen Charlotte Islands - Progress Report. Fish/Forestry Interaction Program Working Paper No. 7. Vancouver, B.C. 46 p.

Smith, R.B., Commandeur and M.W. Ryan. 1984. Vegetative succession, soil development and forest productivity on landslides, Queen Charlotte Islands, British Columbia, Canada. pp. 109-116, In: C.L. O'Loughlin and A.J. Pearce, eds., Proc. IUFRO Symposium on Effects of Forest Land Use on Erosion and Slope Stability, Honolulu, Hawaii, May 7-11, 1984.

EVALUATING FORESTRY RESEARCH: AN OVERVIEW

Robert Z. Callaham

Program Coordinator, Wildland Resources Center
University of California,
145 Mulford Hall, Berkeley, CA 94720; and

Consultant on Research Management,
26 St. Stephens Drive, Orinda, CA 945 63

A framework is given for evaluations, with particular attention to management and conduct of forestry research and development. Many and divers considerations must be taken into account during evaluations. Special care must be taken to assure utilization of findings from evaluation. Additional sources of information and definitions of terms are provided.

Introduction

Evaluation is nothing new to the world of science and research; it is part and parcel of each scientist's investigative process. What is new is the increasing emphasis during the last decade for those involved in science and research to become accountable for their actions in public arenas. New also is the emergence during the late 1970's of evaluation as a profession and field of scientific study.[1] This report provides an overview of the science and art of evaluation as it applies to management of research and development (R&D) with particular attention to forestry R&D.

Politicians, bureaucrats, special interest groups, even concerned laymen are demanding accountability of scientists and researchers. Under examination are problems chosen for investigation, expenditure of funds, delivery of new technology resulting from the R&D process. The tough scrutiny and cutbacks in funding for R&D during the last ten years stand in marked contrast to the preceding twenty years. During the post-Sputnik era, scientists seemingly were asked only what topic they wanted to research and how much money they needed.

Today's climate of retrenchment in spending, balancing of budgets, and questioning of scientists and research managers sets the stage for evaluations of forestry research in nearly every country around the world. In 1979 the representative from Africa to the Executive Board of the International Union of Forestry Research Organizations (IUFRO), Dr. Dominic Iyamabo, called for that global organization to become involved in developinng methodology for evaluating forestry research. The Board's affirmative and enthusiastic response resulted in Dr. Grayson's circulation of two rounds of questionnaires to the member organizations and the presentation of his report at the IUFRO Congress in Japan (Grayson 1981).

Another action taken by IUFRO at Kyoto in 1981 was the formation of a working party to deal with the topic of evaluation of forestry research. I now serve as the chairman of that working party and Louis Huguet, Director General of the Centre Technique Forestier Tropical in France, serves as cochairman. More than 130 research managers and scientists in 40 countries have signified their interest in evaluations of activities related to R&D in forestry. Goals of the working party are to outline purposes, areas, and methodology for evaluation, to clarify terminology related to evaluation, and to provide examples of evaluations in forestry research.

The primary purpose of this report is to provide a framework for evaluations of forestry research. The second purpose is to provide, particularly for neophytes to the topic of evaluation, an overview of the science, literature, and profession. Particular attention is given to R&D organizations. Literature is cited in reference to the text, but a selected bibliography is provided for those whose interest would lead to further reading.

A glossary is included to help all of us sharpen our understanding and usage of the language of evaluation of R&D organizations. This glossary should be particularly useful to those whose first language is not English and who may have difficulty in translating the technical jargon of R&D literature.

Evaluation Defined

Words defined in the glossary will not be redefined in this section; however, evaluation as used in this paper deserves attention here. The classical definition of evaluation provided by Webster is:

> . . . to find or ascertain the value or amount of; to judge or determine the worth or quality of; to appraise carefully; (or in mathematical usage) to express numerically or to find the numerical value of.

Salasin *et al.* (1980) defined evaluation in terms of objectives:

> . . . collection and analysis of structured information in order to answer questions related to: decisions about duration and funding of a program; changing the program to improve its probability of success; altering policies or procedures under which a program operates; and building support with policy makers and constituencies.

These contrasting classical and utilitarian definitions demonstrate that evaluation has meaning in both social sciences and the art of management.

[1] Problems of this fledgling profession are revealed by Morel and Flaherty (1978), Conner and Dickman (1979), Freeman and Solomon (1979), and Windle (1979).

Fundamentals of Evaluation

Space does not allow for an exhaustive discussion of questions related to evaluation, but our framework requires that they be explored briefly.

Why evaluate? The situation facing forestry research as outined in the introductory section of this report has become critical in most countries of the world. This need for evaluation of research and science is not peculiar to forestry, but rather it is a part of what Atkinson (1979) and Phillip Handler (1980) called "a new social contract between the scientific community and the larger society . . ." Handler noted that support for the scientific endeavor:

> . . . is provided very largely because of the role of science as the progenitor of useful techonologies. And because of the very scale of that support, science itself is now a public venture. Very reluctantly I have concluded that the supporting society will, increasingly, insist not only that the uses of science be democratically accountable, but that the conduct of science be accountable as well--however uncomfortable that may make scientists."

Besides fulfilling our social contract, useful benefits result from evaluation. Evaluation first provides an overview for higher bodies that establish policies and provide funds for the research. Second, it benefits managers by enabling them to improve the allocation of resources among activities and by assisting them in planning, guiding, or directing activities and assessing progress. Often overlooked is a third benefit, that received by scientists through information and motivation resulting from evaluations in which they participate. Improved cooperation among research organizations and between research organizations and groups of users can also be a benefit of evaluation. A final purpose for evaluation is to provide information for education of professionals and the public at large.

What are the objectives for evaluation? The primary objective is to establish the adequacy of quantity, quality, and responsiveness of outputs from a research program in terms of the clients it serves. Another objective is to establish the potential and actual uses of knowledge and methodology that result from the research effort. Evaluations can establish whether either the state of knowledge or the state of practice has changed as a result of the effort. If beneficial uses have resulted, then evaluations can identify the effects and impacts. Another objective is to determine efficiency, that is, expenditures of effort relative to production of outputs. Usually evaluations have as a final objective the justification for some change in status of research such as acceleration, redirection, or even termination of effort.

What are the topics or areas for evaluation? Three are primary: responsibilities of management, responsibilities of groups of scientific and technical people, and performance of individuals. Areas for managerial evaluation can look at the process or results in such areas as: adequacy and allocation of resources; delegations of authority and responsibility; technical services to scientists; directives on policies and procedure; safety and health; equal-employment opportunities; and training or advanced education for employees. Evaluations of scientific and technical activities might look into planning and controlling research programs, measuring outputs and achievements, and assessing impacts and returns from investments. Performance and accomplishments by individual scientists or specialists can be examined through inquiry into: job assignments, teamwork, leadership, supervision of subordinates, availability of guidelines and support for their work, achieved qualifications, demonstrated outputs and contributions, maintenance of technical competence through continuing and on-the-job training, and compensation and rewards.

Who should be involved in evaluation? One of the most important lessons learned through evaluations during the past 20 years is that evaluators should not work alone. Those who make policies, decisions, and commitments should be involved, as should those who manage, those who perform work, and outsiders who have special interests. Evaluation leads to useful effects only when it is a participatory process involving all interested parties. It is a multi-ringed circus with a diverse cast of actors having radically different styles, values, and objectives.

Evaluations of the long-term performance of individual researchers seems better done by their peers than by their individual supervisors (Stahl and Steger 1977; Litras 1980). Panels of peers are reliable in measuring innovation and productivity and in assessing overall performance and promotability. Such panels have long been the mainstay of evaluating performance and promotability in most of our universities. They have been extensively used in the U.S. federal government since the early 1960's. Three papers to be presented later in this program will bring us up-to-date on evaluating the individual's research and productivity in the USDA Forest Service, in Canada, and in the tenure evaluation process used in universities.

When should evaluation be done? Some who are enamored of evaluation would have you believe that evaluation is integral to every aspect of management and investigation in R&D programs. Formal evaluation certainly should not be that all-pervasive. But it should start before the planning process in the so-called preplanning process, when needs and objectives are identified, and it should occur repeatedly during implementation of any R&D program. To evaluate only at the end of a program is too late because corrective actions cannot be taken in time to increase efficiency or to improve upon the outcome. Evaluation should be done soon after a new program is organized, just after it is underway, again near the midpoint, and just prior to the wrap-up, when results can be evaluated and further needs identified. Postmortem evaluations may be warranted under special conditions.

Pitfalls in Evaluation

Salasin et al. (1980) pointed to particular problems in evaluations of federal R&D organizations (but these problems probably are found in all R&D organizations).

The first problem is to define success of R&D. Often in R&D rejection of a hypothesis, what some might consider a failure, has more profound effects than does discovery of some new fact or thing. Research in its own right usually does not solve problems, but it provides the technology which, upon application by others, can bring about change. To whom then does success belong?

This points to the second problem, assignment of responsibilities for success or failure. The spectrum of responsibilities in R&D is very broad. To partition and assign benefits to the basic, developmental or application phases of R&D programs is most difficult.

The third problem is that R&D programs have a multiplicity of objectives. Intertwined objectives--technical, political, economic, professional, institutional, or even international--make evaluation a confounding job. Salasin and his associates pointed out that "evaluation of a program may not be achieved simply by aggregating evaluations of constituent projects." They also pointed to difficulties of reconciling political and scientific viewpoints and value systems. At the political level, evaluation is generally in the context of broad goals and services to constituencies. At the program level, the context may be contribution to much more restricted goals. At the individual level it may be contributions to personal goals or to a profession or discipline.

A frequent problem of evaluations is their lack of effect (Rieker 1980). Too often evaluators ignore the connection between the design and execution of an evaluation and the later use of the information generated by that evaluation. Part of the trouble may result from negative attitudes of some research managers, scientists, and users of technology towards the evaluator or the evaluation process. They may feel threatened by the evaluation or its outcome. More often vested interests of sponsors and judgmental attitudes of decision makers who receive the findings of evaluations may effectively render any evaluation moot.

Particularly troublesome in some evaluations may be that the R&D organization may have neither charter, assignment, nor funding to transfer technology being generated. Evaluations in such situations are circumscribed. They should look only into efforts, productivity, and efficiency and they should stop short of evaluating effects and impacts.

Considerations in Evaluation

The fundamental tenets of evaluation apply to all approaches to the evaluation process. These approaches vary according to whether the evaluation is to be ex post facto, of previous work, or whether it is to be ex ante, to foresee the future. A major consideration when approaching any evaluation is the evaluability of the topic or problem. Criteria and assumptions must be established by which the evaluation can reach conclusions about the success or failure of the venture. To reduce the range of choices on approach, evaluators have adopted several standard conceptual models of evaluation research. How these models are applied depends upon the organizational levels of the R&D effort that are to be evaluated. When to evaluate, how long to take in the process, and whether an evaluation is to be done by an individual or a structured team are important considerations that may affect the outcome and utilization of the information that is gathered. To ensure the ultimate utilization of the findings of the evaluation, many stakeholders need to be involved in the process, and a user-oriented framework of evaluation should be applied. Each of these considerations will be discussed in turn.

Ex Post Facto Versus Ex Ante Evaluations

Retrospective, ex post facto, evaluations start from the current position or some past time and look backwards. Usually, inputs and outputs are easy to measure under these conditions. Individuals, teams, or organizations can be involved in the process. Frequently individuals themselves or groups of peers can provide inputs to the evaluation process. Impacts and returns may be readily discernible if sufficient time has elapsed to use such measures of productivity. Sociometric or network approaches can be used to derive ratings of performance.

Predicting the consequences and likelihood of solving future problems is the topic of forward-looking, ex ante evaluations. Such evaluations are done relatively infrequently, and they have special problems of methodology and of involvement of those who are stake-holders in the evaluation. Futuristic evaluations require testing of alternative assumptions about what will affect the future and arriving at reasonable assumptions. Realistic estimates must be made of the quantity and quality of a variety of outputs and the time of their arrival in the future. Impacts of the program that is being evaluated need to be foreseen. Probabilities must be estimated for accomplishment of objectives. Implications of changes in the state of science or the state of practice must be anticipated. Stake-holders in the future might be quite different from those who are involved in the topic at the present time. Foreseeing who will receive what benefits evaluations.

Some Aspects of Ex Ante Evaluations

Procedures and models for evaluating proposed research projects and selecting among proposals are topics that are dealt with extensively in the literature (Baker, 1974; Becker 1980). Some insight into this literature was provided by Rubenstein and Schroder (1977). They discussed the problems concerned with reliability and validity of probability assessments. Closer to the field of forestry research, Kuusela (1977) provided an example where goals of the organization financing future projects were used to establish criteria for setting priorities among research projects.

A system of reallocation of funding based on the ex ante evaluation of research projects in Canada, was outlined by Andarawewa (1969). More recently a review of the literature related to agricultural research management and the ex ante evaluation of research proposals was prepared in Australia by Greis (1981). A thought-provoking defense of the old ways of deciding about ex ante research projects in agriculture has been offered by Shumway (1981). The four-step process used by USDA Forest Service Research in long-range planning was briefly presented by Callaham (1985). One step of this process, a paired comparison technique for selecting projects for future research by USDA Forest Service, was presented to the Society of American Foresters by Davis and Shafer (1984). Tauss (1975) also used paired comparisons by forced choice in order to rank and rate proposed R&D projects. A model by Menke et al. (1981) for evaluating future basic research strategies in the pharmaceutical industry may have applicability to evaluating future strategies for research in forestry. In a completely different vein, Nichols (1979) described important considerations for planning and performing evaluations that are part of future interventions to achieve organizational change.

Evaluability

Assessments of evaluability explore the objectives, expectations, and information needs of program managers and their superiors (Wholey 1979). Prior to an evaluation, such an assessment explores program realities and the likelihood that program activities will achieve measurable progress towards program objectives. Included is an assessment of the likelihood that information from the evaluation will be used in program management, and if it is to be used, what kinds of decisions will be influenced. Products of the evaluability assessment are: 1) a set of agreed-upon program objectives; 2) important indicators of performance and side-effects on which the program can be held accountable; and 3) a set of options representing ways in which management can change programs, activities, or objectives to improve performance. Obviously, evaluability ought to be assessed before any commitment is made to an evaluation by either managers or evaluators.

Criteria for Evaluating Success or Failure

Suchman (1967), in his classic early text on evaluation, proposed five categories of criteria by which to evaluate the success or failure of a program. The first of these was effort, a criterion of success related to the quantity and quality of activity, the input of energy without regard to output. Evaluation of effort seeks answers to the questions: What did you do? How well did you do it?

Suchman's second category of criteria was performance, under which evaluation would measure results or effects of efforts. Measures of performance require clear statements of objectives. The questions to be answered by evaluation would include: Did any change occur? Was the change intended?

A closely related but different category of criteria is adequacy of performance. Adequacy refers to the degree to which effective performance was adequate to the total need. A question that might be asked in this regard is: What is the relation between achievement and expectation?

Suchman's fourth category of criteria was efficiency. This category was concerned with evaluation of alternative paths or methods. Evaluation of efficiency looks at the chosen or alternative paths and examines the relative cost, effort, and performance to achieve the same effect. Some questions to be answered are: Could it have been done cheaper or quicker? Was all of this effort necessary?

The fifth and final category proposed by Suchman was process. Process included four dimensions: 1) attributes of the program; 2) the population exposed to the program; 3) situational context of the program; and 4) different kinds of effects produced by the program.

Factors Influencing Success in R&D

Several authors have indentified a multiplicity of complex factors related to success or failure of R&D projects. An awareness of these factors is important when one contemplates evaluation of an R&D organization. These factors raise flags that should be recognized during the design and implementation phases of evaluation.

Rubenstein et al. (1976) identified both barriers and facilitators encountered in industrial R&D projects. These factors were grouped according to whether they were related to: work site; impetus for innovation; process of and criteria for decisions; structure and process of the project; structure and process of the overall organization; and outcomes. Specific factors within each group variously affected technical success, overall economic success, or both technical and economic success.

De Cotiis and Dyer (1979) defined and measured industrial project performance. They identified 12 empirical determinants and their relative effects on project performance.

With respect to forestry innovations, Moeller and Shafer (1981) found 22 factors were influential. Innovation by USDA Forest Service research was most frequently influenced by adaptation of existing technology or techniques, pilot testing, and cooperation with private industry. Other significant factors were: cooperation and personal contacts with users; theory development; and computer capabilities. The probability of successful innovation depended more on factors taking place during the conception period, before direct research began, than on technology efforts during the post-innovation period.

Conceptual Models of Evaluation

In order to reduce the range of choices to manageable levels, evaluators have adopted a few conceptual models of evaluation. These models define the means to be employed as well as the ends to be served in the evaluation process. Recently, Rafter (1984) defined three major paradigms used in evaluation.

Rafter's scientific approach prescribed a deductive research strategy. It assumes objectivity and employs quantitative measures of performance, systematic sampling, experimental designs, and multivarate analyses. The process for implementing this mode is deductive and investigator centered, with little involvement of the client.

Rather than generating knowledge for understanding, the purpose of the scientific approach, Rafter's second interactive approach emphasized generating knowledge for use in actions. The political and personal factors significant in effecting use of findings are more significant than the scientific quality of the evaluation. The interactive approach emphasizes the role of the evaluator as the organizer of the information users and facilitator of group decisions. The interactive model differs from the scientific approach in that it aims to solve problems through the use of research and group decision making.

The hybrid approach, Rafter's third model, features:

> the two-stage process whereby the experts first provide information for the decision makers, and second, the decision makers are involved in all decisions about the utilization of the information. For this approach to work there must be confidence in the evaluator to conduct quality, credible research, which means there must be an ongoing structured relationship between information users and (evaluators). . . . The hybrid model permits the research stage to be under complete control of the evaluator, the critical dissemination stage allows the policymakers to determine significant findings and the form of utilization.

Rafter goes on to provide three useful guidelines for evaluations:

> Guideline One: Evaluators should employ a contingent strategy in designing evaluation research by selecting from among a range of choices the approach best suited to the utilization desired.
> Guideline Two: In implementing evaluation research, equal attention should be paid to how a program works as to the outcomes of a program
> Guideline Three: In addition to traditional research skills, evaluators need "non-standard" group process skills.

Organizing to Serve Managers

Every manager must be an evaluator, for evaluation is a basic diagnostic tool of managing R&D. Keaton (1980), in a very pragmatic way, has outlined features of five different types of evaluaton. Included are "show-and-tell," financial reviews, program status reviews, technical workshops, and problem search. From the first to the last, the manager's visibility in the evaluation changes from high to low. Keaton makes a key point: "The purpose of program review is as much for the project to show how management can help the project as it is for management to see how well the project is going."

Evaluation is often both conceptually and functionally separated from line management. Program managers may not see themselves as served by evaluations. Bunker (1978) has outlined potential benefits of close collaboration between managers and evaluators. Each must maintain independence, but interdependence is to be strived for. Organizing evaluations so as to serve the needs of managers pays off in greater acceptance and utilization of the results of the evalation.

Organizational Levels

Most commonly evaluations are made of projects or programs. Only occasionally are institutions or large organizations evaluated. As the dimensions of the organization become wider or deeper, then the purpose and scope of the evaluation necessarily must become narrower. If one looks at institutions or national organizations, only some part of what they do and how they do it can be examined. It is important to tailor the evaluation to fit the issue. A well done, narrow evaluation will be better received than a shoddy one of a broader part of the problem.

R&D Phases

Easy to appreciate is the difference in evaluative processes for basic or applied research. Little or no impact should be expected of basic research, whereas developmental and applied research should have demonstrable effects. Quinn (1971) summarized available information and categorized methods and techniques of measuring the spectrum of industrial

R&D activities. He found that qualitative methodology must be applied to the evalutations of the very uncertain area of pure or basic research. At the opposite end of the uncertain spectrum, developmental and demonstration projects allow for quantitative methods of evaluation.

In an exactly parallel fashion, phases in the R&D cycle progress from uncertainty, such as when research is proposed or just beginning, through greater certainty as the cycle of applied research, development, and application is closed. Given this relationship, qualitative methods of evaluation would be more appropriate to the proposal phase or beginning of investigation, but quantitative methods would become more applicable during later phases in the cycle.

Timing and Duration of Evaluations

Reference has already been made to the importance of evaluating early in the planning stage and at early, mid- and late times in the life of a program or project. Another consideration in the timing of evaluations is that they should not be conducted at periods of high stress in the organization, such as at the end of a fiscal year or during a busy, short period of experimentation or seasonal field work. Evaluations will be more successful if the organizational entity being evaluated is relatively at ease and unstressed.

Critical dates for decisions need to be considered in scheduling an evaluation. An evaluation providing information to decision makers one month after decisions were made about the next annual budget probably will be forgotten in the eleven months before the next time for such decisions. The start and end of a fiscal year, dates for regular meetings of bodies that establish policy and set budgets, and even such sensitive times as holidays and vacations for the affected employees all need to be considered when scheduling an evaluation.

Duration of an evaluation is critical: it should not take too long. On the other hand, time must be sufficient for a deliberate and considerate inquiry. Duration of the evaluation must allow for: adequate planning; notification of those who will be involved; and sequential involvement, perhaps with reiterations, of all stake-holders in the evaluation. Finally, time must be allowed for drafting, reviewing, and completing an adequate, professional report and delivering it in written and oral presentations to the various stake-holders.

Evaluation During Implementation

During the life of any activity, evaluations are warranted to help managers judge the adequacy and direction of work being done. Such evaluations range from informal communications to formal requirements for progress reports, or at the extreme, to mounting of formal, on-site reviews.

Choice of the appropriate approach depends upon several factors. If the R&D phase is basic research, then less cumbersome and less formal, qualitative methods would be called for. If demonstrations are the objective, then more quantitative, objective methods might be warranted. Naturally, trust levels between manager and performer will influence the frequency, intensity, and nature of reviews. In the same way, less experienced performers should be reviewed more frequently and with deeper penetration into their activities. Relative levels of investment and risks of failure are other compelling reasons to adjust the frequency and nature of evaluations.

Within a forestry research setting, Kuusela (1977) describes evaluative controls during the life of research projects. A more general strategy and methodology for evaluating programs during implementation is laid out by Leithwood and Montgomery (1980).

The touchy topic of distinguishing between projects to be continued and those to be abandoned was approached in a sophisticated manner by Balachandra and Raelin (1980), and in a pragmatic fashion by Tauss (1975).

Choosing Evaluators

Many factors must be considered before choosing individuals to conduct an evaluation. Objectives and contemplated approaches may dictate the acceptability or unacceptability of certain individuals. Costs of evaluation can be kept low if people already on the payroll are the evaluators. Involving individuals or teams of people from outside the organization may significantly increase costs. Of course, hiring professional evaluators will incur the greatest cost of all.

Intramural evaluation. Responsible managers, a designated employee, or a team from the organization can be effective evaluators. This strategy has many advantages besides salary savings. Those involved in the evaluation will already be familiar with the area to be evaluated. Similarly, they are more likely to be personally involved in the issues, acquainted with the stake-holders, and held in high regard by them. On the debit side, the intramural evaluation has costs because the individual(s) will not be able to perform their regular duties and others may have to fill in for them. Also, the evaluators may be biased or timid. They may lack the necessary professional skills and competences required for the evaluation. An offsetting and sometimes overriding benefit is that insiders learn more about the organization, the particular topic, and the perspectives of all stake-holders.

Scientific or technical peers. Technical peers of the people being evaluated, being already knowledgable about the scientific and professional intricacies of the work to be evaluated, have special understandings and capabilities that are valuable in evaluation. The problems with using peers as evaluators relate primarily to choices among peers and the biases that are represented in peer groups. Peers, although technically competent, have definite biases depending upon their education, job experiences, and places and conditions of employement. To overcome these individual biases, peers usually are used in panels, where the strengths and weaknesses of individuals are compensated for by others. Just as scientists usually seek three or more reviews of their technical work and publications, so peers employed as evaluators should number three to five so as to get a cross-section of technical perspectives and values.

A major problem with using panels of peers is often that they are not available for a long enough period of time to conduct an evaluation. They are best used for a few days or at most a few weeks to critique the approaches to evaluation and the intermediate and final outcomes.

Panels of clients or users. When the effects and impacts of R&D are to be evaluated, panels of clients or users offer an effective means of determining where, when, how, and to what extent objectives were attained and program accomplishments can be evaluated. These panels have the same limitations as do those of peers. Such panels also have the additional complication that clients and users are characterized by strongly held views about their best interests and lack the objectivity that characterizes scientific and technical peers. Panels of clients and users, like panels of technical peers, are limited in the amount of time they can devote to the evaluation process. They are best used at the outset and at the conclusion of the evaluation. They can be especially effective in helping gain acceptance and appreciation of the evaluation among the body of people who are affected by the program being evaluated.

"Blue-ribbon" panels. Evaluations made by people who have rank, authority, and esteem often will open doors in the evaluation process and will result in greater utilization of findings. Blue-ribbon panels always should include at least one person who is highly respected by the decision makers involved in the evaluation. This person, sometimes referred to as a "door opener" can be very helpful in drawing the attention of people in high places to the evaluation and its outcome. Such persons need not participate during the entire evaluation, but they must be involved at least enough to have credibility in discussing the approach to the evaluation and its outcome. When blue-ribbon panels can be composed of individuals who are scientifically and technically expert, the strength of those two approaches is magnified.

Outside professional evaluators. Specialists, knowledgable in the relevant sciences and the arts of evaluation, are available for hire to conduct evaluations. These people bring special expertise to the process of evaluation, but they often lack the necessary special understanding of the program or situation to be evaluated. They spend far more time in the planning and introductory phases of an evaluation than would be necessary for the evaluators already discussed.

Many evaluations are contracted to management consultants. People who operate under such titles may or may not be especially qualified and skilled in evaluation. The similiarities and differences between the two professions of management consulting and evaluating were highlighted in a recent publication by Stanfield and Smith (1984).

Utilization of Findings

The notorious fact is that little of what is learned or recommended as the result of evaluations ever is utilized in decisons to make changes or improvements. All who attest to this problem recognize that what can be learned justifies the cost of an evaluation. However, too often, as has been noted repeatedly, the evaluation results in disappointment when findings are not used by decison makers. Hence the evaluation should focus throughout its process on utilization of its findings by sponsors, by the evaluated entity, and by clients and customers.

The sponsor's primary concern is to identify problems, needs, opportunities, and constraints or alternatives for action. Another possible use is for decisions on policies, procedures, and rewards or penalties. In this manner, the sponsor benefits from discussions, seminars, and conferences. During the evaluation often the sponsor will want to produce parts or all of the evaluation as in-house documents with appropriate transmittals.

The evaluated entity needs the information contained in the evaluation as a basis for continuation of or change from the status quo. Here again, the evaluated entity may want to use the evaluation in order to further its goals, which might even include additional compensation and rewards.

Clients and users are concerned because of costs and attention given to evaluation, and they want to see that useful outcomes are achieved. They also may want to use the resulting information for their own special interests, either positive or negative.

Finally, evaluators use the output from evaluations as a basis for payment or acclaim and other support. Evaluators also use evaluations as a basis for reports, publications, presentations at professional meetings, and justification for future evaluations.

Numerous publications focus on the design-to-use approach to evaluation. Patton (1978) outlined a utilization-focused approach to evaluation. In this approach, relevant decision makers are identified, organized, and their questions relevant to the evaluation identified and focussed. Methods of evaluation are selected to generate useful information. Decision makers and information users participate in the evaluation with respect to analysis and interpretation of data. Evaluators and decision makers negotiate and cooperate in disseminating information forthcoming from the evaluation. The purpose is to accommodate rather than manipulate the views of all stakeholders.

The design-to-use process was proposed by Rieker (1980). She postulated that the results of evaluation are not used because the evaluator ignored connections between design of the evaluation and use of the information generated. She described the major problems affecting design and use of evaluations and suggested structural conditions for improving design so as to improve use. She stressed the superiority of informal communication practices. Evaluators and decision makers should interact during discussions, seminars, and conferences. Downplayed should be the formal information dissemination practices that evaluators often use, such as publication in journals where large audiences can be reached but not those who want, need, and will use the information that is generated.

A "user-oriented framework" for the design of evaluation studies proposed by Mandell (1984) layed out the key strategic aspects of organizing, designing, and managing evaluation efforts so as to involve the client in the design and selection of evaluation studies.

Evaluating Management of R&D

Little has been written about evaluating management of R&D, but much can and should be written about this important topic. Millions of dollars and thousands of people are involved in managerial functions within R&D organizations. Managers have a major role to play in making R&D productive. Yet, managers above the level of project leader or program manager seem to have been forgotten. Managers need to be evaluated with respect to management of available resources, services provided for scientists, directives on policies and procedures, training and advanced education for the work force, and the environment for innovation.

Adequacy and Allocation of Resources

Managers have responsibilities to manipulate the human, physical, and financial resources at their disposal. Certainly managers can be evaluated in terms of the balance and quality achieved within their work force. Numbers of scientists and engineers should be balanced with numbers of supporting personnel, technicians, and clerical assistants. Researchers who spend their time doing routine tasks because they lack technical and clerical help are not being effectively used (Cuthbert and Blinder 1973). At the other extreme large cadres of workers who may not contribute significantly to the R&D outputs should be examined critically. Quality of the work force, as affected by practices of recruitment, training, and advancement, also deserves examination.

Funding is a special item for evaluation of managers of R&D. Increasing levels of funding might indicate success on the part of the manager in justifying R&D programs and gaining more financial support. Increased funding deserves evaluation to see that the purposes for which additional funds were provided are being met and that equity is being maintained within the overall R&D establishment. If funding levels are not changing, an evaluation may be appropriate to learn why this situation exists and what actions might be undertaken to change it. If funding levels are decreasing, then an evaluation may discover the reasons for the decline and uncover possibilities for reversing the trend.

Another aspect of financial management that deserves evaluation is year-end spending and the amount of funds remaining unobligated at the end of a fiscal year. An evaluation might demonstrate that controls over the process of allocating and spending funds are either too lax or too strict or that delegations of authority for financial management need to be changed.

The management of facilities within R&D organizations, a complicated function that is the responsibility of management, should be subject to evaluation. Some major topics for review might be: condition and trend of buildings and specialized equipment with respect to their age, deterioration, maintenance, and needs for renovation or replacement; allocations of space within facilities to assure that territoriality among researchers is not causing maldistributions and that those who have strong justifications for additional or different facilities may have them; and possessiveness of scientists over specialized equipment and facilities. Evaluations might show that equipment purchases are being duplicated unnecessarily and that expensive items are being underutilized because of the attitudes of researchers having responsibility for them.

Another topic related to facilities is the need for facilties that are not now in existence or available. To foresee these needs and to reach out into unexplored areas related to facilities is an important topic for evaluation. Safety and health considerations related to faciities are also in need of evaluation and are discussed under a separate heading.

Services to Scientists

Researchers are more productive if scientific and technical services are provided to them so they avoid spending their time, energy, and funds for these essentials. Included among such scientific and technical services would be: editing and publication functions; library and technical information; statistics and biometrics; computers and wordprocessing, graphic arts, drafting, and photography.

Publication services. Evaluation of the editing and publication function should be directed to see that the flow of manuscripts is steady and without inordinate delays. Simple evaluations can determine the time and effort required for manuscripts to be processed. Another aspect for evaluation is the handling of publications including stockpiling of supplies to meet requests and processes of announcing or distributing publications to clients, users, and those who request them.

Sci-tech information services. Scientific and technical information functions relate primarily to libraries, but might also include management of databases. Topics for evaluation would include: number and nature of requests made by scientists for services; time required to deliver requested items; adequacy of collections of reference document; acquisition of new documents; and particularly, acquisition and routing of periodicals to assure economies in purchasing these costly items and their rapid circulation among users. Another topic for evaluation in this area would be the liaisons established with other libraries and sci-tech systems in order that networking, interlibrary loans, and sharing of common services could result in economies of operation.

Statistical and biometrical services. Design of experiments, analysis of data, and subsequent publication of statistical and biometrical data are areas that are ripe for evaluation. Evaluations can delve into the technical adequacy of these services and the resulting experimental designs, analyses of data, and published information. Here again the adequacy of special consultation services and of training for researchers and technicians can be evaluated.

Computers. The world of computation has become extremely complex and deserves evaluation in all R&D organizations. Evaluations should look not only at availability of hardware and software, but also at the training for use. Potentials for sharing equipment to save costs deserve exploration. Use of antiquated manuals, methods, and inadequate computers can be identified through evaluation. The goals should be two-fold: to relieve scientists of the drudgery of computing and to provide computational capability to unfetter their experimental designs. Evaluations of word-processing may point to the need for typing pools that can use new machines efficiently.

Graphic arts, drafting, and photography. Researchers often enjoy making their own drawings and processing their own photographs, but such inefficient methods do not make the most effective use of their time. How these activities are carried on is an excellent topic for evaluation. Potentials for experts to provide services in each of these areas to meet the needs of researchers could be identified.

Directives on Policies and Procedures

Management has the responsibility to establish goals and objectives for an organization. It also has the obligation to operate within the laws, regulations, and policies ordained for the organization by external forces. Management also has the responsibility of orienting personnel with regard to its expectations for: performance of work, ethical conduct, time and attendance, pay and incentive systems, and ways of doing business. All of these suggest topics for evaluations to examine whether management is doing its job effectively and efficiently.

Safety and Health

Incumbent upon management is the responsibility to provide for the safety and health of all employees. In this connection, a plan of action with appropriate requirements for inspections, special facilities, training of employees, and maintenance of records are all evaluable. Training of vehicle drivers and vehicle accident records are items that might be considered either under the safety and health program or under the facilities program.

Training and Advanced Education

Management has the responsibility first for hiring well-trained people and second, for giving them additional, specialized training or education so as to make them most productive. Management also should provide advanced education to raise the potential performance levels and career horizons of employees. Each employee should have realistic aspirations and commitments of the organization to improve the individual's status over time. These responsibilities of management for training and educating the work force all are good topics for evaluations.

Environment for Innovation

Encouragement of creativity, originality, and inventiveness provides a favorable climate for innovation. Evaluations can inquire into the freedom that is granted to researchers, the stimuli and opportunities that are provided for interaction with their colleagues at other institutions, and the frequency of attendence of researchers at professional and scientific meetings. Another area for evaluation is the climate for scientific excellence that exists within the research organization. The training and advanced education previously mentioned are particularly appropriate to foster a climate of scientific excellence and to provide for innovation.

Evaluating Conduct of R&D

Considerable attention in the literature has been directed to achievements of R&D and returns from investments in R&D. The important topics of planning and controlling R&D activities are evaluable, but neglected in the literature. Because they are too often neglected, they will be discussed here briefly.

Planning for R&D

An important topic worthy of evaluation under this heading would include whether users and audiences, and their expressions of needs or priorities for research and development work, have been recorded. Another topic would be whether or not strategic or long-range plan exists within the institution. Such plans should include: identified needs and priorities for R&D; estimates of finances, personnel, and facilities required to carry out such plans; and schedules for starting and stopping R&D activities. All research organizations should have a planning process to assure that individual studies are appropriately designed and approved. The toughest topic for evaluation under the heading of planning is determination of whether there is evidence of imagination, creativity, and intuitiveness displayed in the research program that is currently underway.

Controlling R&D

Under this heading topics that should be considered for evaluation include adherence to policies, schedules and budgets, delegation and use of authorities, and organization and progress of work. Cooperation with outside organizations and coordination among units within the R&D organization are other topics that are important for evaluation. Controls should provide for timely termination of R&D activities, and such terminations should be examined at regular intervals.

Measuring Achievements

The quantity, quality, and timeliness of outputs and achievements by an R&D establishment are of greatest importance. Whether or not such accomplishments are produced and found to be relevant to solving the original identified problems are topics deserving of evaluation.

Evaluation of performance of R&D organizations often is expressed in business terms where new products and economic profitability are paramount (Quinn 1971). But these terms are not applicable to forestry R&D. More than a decade ago, Galloway (1971) proposed a simplified and easy way of evaluating R&D performance. In essence it was to conduct a self-analysis in which the innovations resulting from R&D are ennumerated each year and the value of those innovations each year is roughly estimated. I applied this same strategy ten years later to the organization I directed, the Pacific Southwest Forest and Range Experiment Station at Berkeley, California (Callaham, 1985).

Our evaluation resulted in enumerization and categorization of all achievements by the Experiment Station during five years (Callaham and Hubbard 1984). Achievements were categorized as innovations, modifications to innovations, and scientific findings. Counts of these achievements under each heading were easily provided by the researchers. Using this inventory, we were able to estimate the benefits that accrued as a result of each of the innovations or modifications of innovations. Users, that is practitioners chosen by our researchers, were brought into our process of reviewing and evaluating achievements and rating the benefits from our innovations. Scientists easily prepared historiographs for selected innovations. These diagrams were useful to chart significant events and to estimate mean costs and time to produce innovations and modifications of innovations. Through this process we produced a meaningful estimate of the benefits and of the achievements realized during a fixed period of time for an experiment station.

My idea to use innovations as measures of achievements of an experiment station stemmed from our earlier hindsight analyses of major innovations resulting from USDA Forest Service research nationwide (Callaham 1981). That evaluation examined 81 selected innovations. Objectives were: (1) to determine the magnitude of benefits and costs associated with a wide range of recent innovations; (2) to describe important factors contributing to successful innovations. Each innovation was evaluated as to the kind and amount of benefits accrued, cost for research in terms of scientist-years, time required, and factors influencing the innovative process. More than sixteen general categories of benefits were found to result from Forest Service research. At least half of the innovations resulted in benefits such as creation of income or employment, increased use of natural resources, and improved quality of environments. From 40 to 50 percent of the innovations, new or improved products resulted, decision-making was improved, management costs were reduced, or prices for commodities were reduced. The first-year benefits alone, $2.6 billion, exceeded the cost of all prior Forest Service research. The innovative period was found to be long, averaging 15.3 years. The innovative process was influenced by the twenty-two factors already referred to (Moeller and Shafer 1981). This evaluation of selected achievements by Forest Service researchers had a profound effect on those who decided the future of funding for research by USDA Forest Service.

Other publications that deserve mention under measurement of achievements are those by Rubenstein (1976) and Salazin et al. (1980). Rubenstein pointed out problems in where to measure effectiveness of R&D, particularly the flow of outputs. He categorized as immediate the outputs produced directly by the researchers including reports, manuscripts, and applications for patents. Intermediate outputs were those immediate outputs incorporated as inputs to social and economic systems resulting in innovations. Preultimate outputs were those where societal change and effect is beginning to be realized and may be attributed, at least partially, to intermediate outputs. Ultimate outputs related to the enhanced quality of life, as through enjoyment of liesure, better health, opportunities for employment or other social or environmental benefits made possible by R&D's innovation.

Salazin and his associates pointed out the complications in assessing the relationships among outputs from a federal R&D program. They stressed that an output may be used in a different area than was originally expected or anticipated. Time-lags, especially those related to commercialization of an innovation are substantial. Different R&D outputs often follow different paths in proceeding from the immediate output to the ultimate output. The partitioning of benefits along these paths to the R&D process is exceedingly difficult. This leads to the last point by Salazin and his associates: sources of R&D outputs are disputed and debatable.

Assessing Impacts and Returns

Innovations resulting from the R&D process have measurable impacts and returns in the social and economic sectors. Costs of producing these impacts and returns are relatively easily measured or estimated. The social and economic benefits resulting from innovations, particularly those emanating from forestry R&D, are less easy to quantify and verify. This entire area has been masterfully reviewed and analyzed by Allen Lundgren (1982) and his associate David Bengston (1985).

Evaluating Individual Performers

Performance and productivity of scientists, engineers, and specialists involved in the R&D process has been studied extensively. Despite the duration and intensity of such evaluations, we still are unable to evaluate accurately the quantity and quality of production by the R&D staff. A comprehensive and scholarly review of 52 publications (Edwards and McCarrey 1973) revealed numerous methods that have been proposed and used. These authors, properly questioning the validity of many methods, found that much more study was needed in this area.

Edwards and McCarrey concluded that the first question to be answered relates to scientific output, described as: 1) the discovery of new facts; 2) the invention of new methods of doing things; and 3) combining known concepts to create new devices. They documented a general lack of agreement on operational definitions of scientific performance, components to be included, and who should measure it. Despite the problems, those who have evaluated scientific output generally use one or more of these measures: overall performance, quantity of written output, quality of the written output, and some measure of creativity or originality.

Short-term and long-term performance should be evaluated differently. Performance during a quarterly, semiannual, or annual term should use different measures from those used for evaluation over a longer term. Long-term productivity and performance should be evaluated over spans of three to five years.

This two-tiered distinction is recognized in the federal government of the United States. Annual or more frequent ratings of performance are by the supervisor based on criteria negotiated between the supervisor and the subordinate. Longer term performance spanning three years is evaluated by panels of peers. Panels advise managers on the grade level at which the individual seems to be performing.

In all systems for rating for research performers, quantity of written output is the most common criterion of productivity. Nevertheless, research on this topic, as described by Edwards and McCarrey (1973), indicates that:

> the only valid interpretation of the number of publications is as a measure of a scientist's written output. In other words it is not acceptable to operationally define scientific performance in terms of bibliographic counts alone.

A state-of-the-practice review of research in industrial companies revealed no system of productivity measurement that is able to make meaningful comparisons over time in a given industrial organization or among different organizations (Schainblatt 1982). Industry, too, suffers from the difficulty of measuring productivity of scientific and engineering groups.

Refinements to improve the measures of written outputs have often proposed weighting or scoring of length or outlet for the particular writing. In all of these studies, the researchers lacked any real empirical basis for using particular weighting systems, and none of them incorporated any consideration of the quality of the paper (Edwards and McCarrey 1973).

Shaw's (1967) monumental study of publications in USDA's Agricultural Research Service substantiated that scientists can be rated just as well by counting total number of citations as they can by separately crediting senior and junior authorship of publications. He contended that publications per year is a better measure than total count of credited publications, since publications per year eliminates the variables of age and length of experience. Shaw also established conclusively that authors who were most prolific were rated by their peers as having produced the higher quality publications. Of course, today's reader may rightfully ask whether the publish-or-perish syndrome, prevailing during the quarter of a century since Shaw made his analyses, has altered the positive correlation between quantity and quality of an author's publications.

Citations to an author's publications are another common objective method used to obtain a qualitative measure of scientific performance (Narin and Moll 1977; McAllister et al. 1980). As with bibliographic counts, citation counts are restricted to an evaluation of the individual's written output. While citation counts have become widespread, numerous problems in citation analysis are recognized by those who are familiar with the technique.

Creativity and originality are highly esteemed traits of some individual performers of R&D (Smith 1982). While these traits are recognized, the methods for evaluating and predicting them are weak at best (Whiting 1972).

The relationship of age to performance in R&D has been evaluated for some time (Shaw 1967; Decker 1973; Kimblin and Souder 1975; Bayer and Dutton 1977). The general conclusion is that scientists in different disciplines and engineers with different specialties have achieved their peak productivity at different periods in their lives. Factors in the personal life of the investigators, particularly those related to family status, change in employment, or retraining can significantly affect productivity.

The conclusion from all of this work is that scientific performance and output are multidimensional. They cannot be satisfactorily measured by any one criterion alone. Furthermore, performance must be evaluated not only by contributions to both science and social systems, but also by contributions to the employing organization, to the profession, and to the scientific community at large.

Overall, individual scientists, engineers, or specialists should be evaluated in several dimensions. Scope and difficulty of their jobs, responsibilities toward the employing organization and toward other employees, level of supervision received, authority granted for independent action, and qualifications, recognition, and achievements of the individual all should be evaluated simultaneously. Finally, evaluations must assure that performance is linked with the several reward systems including salaries, other compensations, and perquisites.

Evaluations and the Process of Decisions

Some interesting social research has been done lately to show how the decison-making process works and how evaluations should relate to this process. McNeese et al. (1983) surveyed the types of decisions that were made in federal agencies and the conditions under which these decisions were likely to utilize evaluative data. Their review of the literature indicated that "administrators are more likely to be interested in (evaluation) which is specifically targeted to short-term problem solving." They also observed repeatedly that evaluation is most useful when it reduces ambiguity, unfamiliarity, and complexity. Their study indicated that evaluative data are seldom utilized in program decision-making at the local level.

Locatis, Smith, and Blake (1980) investigated the effects of evaluative information on decision making. They found that:

> information does have an effect on decisions, particularly negative information. Even equivocal and conflicting information depressed judgments. Positive information inflated judgments, but not to the same degree that negative information depressed them. . . . familiarity tended to blunt the effects of positive and negative information and had little effect on information that was conflicting or equivocal. . . . timing of information and presentation had little effect on judgments.

More recently Pflum and Brown (1984), similarly structured their research on decison-making situations and learned that:

> Interactions of conflict and quality (of decisions) affected need for more information and more time in decision making. A conflict and time interaction affected preference to talk to other board members. Changes in situational contexts affected information need and use in small, decison-making groups.

Using a related but different approach, Ross (1980) addressed the importance of decision rules and their crucial role in program evaluation. He pointed out that "evaluators rarely discuss issues related to the development of decision rules". He then goes on to consider decision rules in program evaluation. This use is most helpful when the task is to assess which of several alternatives should be taken. Decision rules are not useful if the task is to generate fresh alternatives. Decision rules help the decision makers retrieve the pertinent data and to categorize it with respect to the decisions facing them.

Sources of Further Information

The foregoing ssections of this report confirm that evaluation is an immense and complex field. Those who are interested in learning more about evaluation may want to joint others having similar interests or to subscribe to serial publications. Two organizations, Evaluation Network and Evaluation Research Society, were created during the last few years to meet the needs of academicians, consultants, contractors, and other professionals. These two organizations are being merged into the American Evaluation Association (AEA) effective January 1, 1986. Further information on AEA is available from Conrad Katzenmeyer, 2816 South 13th Street, Apt. 2, Arlington, VA 22204, telephone (202) 254-5830. Those who join AEA (annual dues are $25) will receive two periodicals, "Evaluations News" and "New Directions in Program Evaluation." The Canadian Evaluation Society (CES) is an active organization deserving attention. Information about joining CED may be obtained from Burt Perrin, Ministry of Tourism and Recreation, Sports and Fitness Branch, 77 Bloor Street W., 8th Floor, Toronto, Ontario M7A 2R9, telephone (416) 965-3124.

Periodicals dealing with evaluation include:

Evaluation and Program Planning: an international journal. Pergamon Press, Inc. Fairview Park, Elmsford, New York, 10523; or Pergamon Press, Ltd., Heddington Hill Hall, Oxford Ox3 OBW, U.K. Vol. 7, 1984.

Evaluation Studies Review Annual: Sage Publications, Inc., 275 S. Beverly Drive, Beverly Hills, California, 90212. 22 Banner Street, London, U.K. EC1Y 8QE. Volume 1, 1976.

Evaluation Review (formerly Evaluation Quarterly, from 1977): a journal of applied social research; Sage Publications, Inc.

Evaluation News: Sage Publications, Inc.

Knowledge-Creation, Diffusion, Utilization. Sage Publications Inc., Beverly Hills, California. 90212.

Research Management: Technomic Publishing Co., 265 Post Road W., Westport, Connecticut 06880.

R&D Management: Basil Blackwell Publisher Ltd., 108 Cowley Road, Oxford, U.K. OX4 1JF.

In 1981, the U.S.D.A. Forest Service established a project on methods for evaluating forestry research at the North Central Experiment Station, 1992 Folwell Avenue, St. Paul, Minnesota 55108. The project leaders, sequentially Allen Lundgren, Christopher Risbrudt, and now Pamela Jakes, are knowledgeable about evaluation and welcome inquiries by letter or phone (612/642-5288).

Glossary

Activity--Any process of research, development, or application involving mental function and in pursuit of knowledge, methodology, or technology for either scientific or practical purposes.

Adequacy of performance--Refers to the degree to which performance is adequate with respect to total need; refers to how effective a program was with respect to total need.

Applications--A process of change through people and communications resulting in widespread acceptance and use of technology.

Creativity--A process leading to new or novel outputs.

Decision--A choice in the face of some degree of uncertainty between two or more alternatives.

Decision rules--Tools for making decisions; interpretive principles used to summarize a large body of information about values of each alternative in a decision in order to determine which course of action is most desirable.

Decision-making--Process by which the best course of action is selected from an array of alternatives; optimal selection is made by assessing values of each alternative in an array, using one or more criteria.

Development--A process of design, improvement, testing, and engineering utilizing available knowledge and methodology and resulting in technology available for practical application and use.

Effect--Changes in status, condition, trends, procedures, or actions resulting from effort toward objectives; ultimate influences of a program upon its targets.

Efficiency--Focuses not on final results, but on ability to proceed successfully; refers to how well and at what costs an effect was achieved relative to other ways to produce a similar effect; in a sense, represents a ratio between performance or output divided by input or effort.

Effort--Energy expended and actions undertaken in pursuit of objectives; quantity and quality of activity that takes place; refers to what was done and how well it was done.

Evaluation--Process or result of ascertaining or appraising values or amounts, both positive and negative.

Evaluation research--Inquiry using methodology of the social sciences, involving a variety of approaches and stake-holders, including those who decide on policies and funding, those who manage, those who perform and those who receive or benefit from outputs and accomplishments.

Goal attainment scaling--A method of defining and measuring goals that permits both description of the activity and evaluation of its individual elements. The five steps include: (1) defining a set of dimensions in which change is anticipated for the client; (2) assigning weights to the dimensions according to importance; (3) developing a set of possible outcomes for each dimension; (4) scoring outcomes; and (5) computing a summary score based on the outcomes across all dimensions (Kiresuk and Sjerman 1968; Kiresuk 1980).

Innovation--Act of introducing something new or novel; any change in ways of doing things or in objectives; can be a product, process, system, technique, or algorithm; certain activities by which our society improves its productivity, standard of living, and economic status.

Manager--A person who directs or supervises research, development, or applications.

Originality--Effort resulting in discrete jumps or discontinuities in knowledge, theory, or technique, or product that was not readily predictable before the fact.

Peer--Other technically coequal workers operating in the same disciplines.

Performance--Results of effort rather than effort itself; refers to how much was accomplished and of what quality relative to immediate objectives; includes measures of originality, creativity, and effect.

Process--Refers to how and why effects were achieved or not achieved; points to factors related to success or failure; deals with attributes of programs, exposed groups, situational context, and kinds of effects.

Productivity--A measure of the efficiency with which resources are used to achieve some desired output or stated objective.

Program--An organizational level above the level of project. The program may have responsibility for a number of projects. The program manager usually is below what is considered the policy level. A coherent group or sequence of research activities having a schedule and system for progress toward a desired goal and usually having a budget with associated accountability. The program manager's responsibilities include some discretion in deciding on the nature and direction of efforts.

Project--A specific research activity; a definitely formulated piece of research.

Propositional analysis--A method of extracting statements from research reports which represent "findings" of fact or judgment. Panels rate these for accuracy, reliance, understandability, and significance.

Research--A process of inquiry, often using the scientific method, leading to creation of new knowledge and methodology and discovery of new facts, their correct interpretation, and revision of accepted conclusions and theories. A craft skill usually evaluated by peer review.

Scientific output--Discovery of new facts (knowledge), invention of new methodology, and combining of known concepts to create new devices or to modify innovations.

Scientist--One who is learned in science; a scientific investigator having the ability to state problems, frame questions, formulate hypotheses, collect data through observation and designed experiments, and write learned papers presenting findings and conclusions.

Study--A limited area of investigation, examination, or analysis of a phenomenon, development, or question.

Technology--The application of knowledge or methodology to practical purposes in a particular field; applied science.

Literature Cited and Supplementary Reading

Andarawewa, A.B. Evaluation of public research programs in agriculture. Can. J. Agr. Econ. 17(3): 157-169; 1969 November.

Atkinson, Richard C. Reinvigorating the contract between science and society. Chron. of Higher Educ., 1979 March 19. p. 80.

Baker, N. R. R&D project selection models: an assessment. R&D Management 5 (Special Issue on Project Selection): 105-111; 1974.

Balachandra, R.; Raelin, Joseph A. How to decide when to abandon a project. Res. Manage. 23(4):24-29; 1980 July.

Bayer, Alan E; Dutton, Jeffrey E. Career age and research--professional activities of academic scientists. J. Higher Educ. 48(3): 259-282; 1977 May/June.

Becker, Robert H. Project selection checklists for research, product development, process development. Res. Manage. 23(5): 34-36; 1980.

Bengston, David N. Economic evaluation of agricultural research: an assessment. Eval. Rev. 9(3): 243-262; 1985 June.

Bunker, Douglas R. Organizing evaluation to serve the needs of program planners and managers. Eval. and Prog. Plan. 1: 129-134; 1978.

Callaham, Robert Z. (tech. coord.) Criteria for deciding about forestry research programs. Gen. Tech. Report WO-29. Washington, DC: U.S. Department of Agriculture, Forest Service; 1981 July. 52 p.

Callaham, Robert Z. Evaluating social benefits of forestry research programs. IEEE Trans. on Engin. Manage. EM-32(2); 47-54; 1985 May.

Callaham, R. Z.; Hubbard, R. L. Evaluation of research achievements by an experiment station. 1984; Unpublished report; Berkeley, CA: USDA Forest Service, Pacific Southwest Forest and Range Experiment Station. 39 p.

Conner, Ross F.; Dickman, Frances Baker. Professionalization of evaluation research: conflict as a sign of health. Eval. and Prog. Plan. 2(2): 103-109; 1979.

Cuthbert, F. L.; Blinder, S. Technicians and support personnel--how many? Res. Manage. 16: 26-28; 1973.

Davis, James B.; Shafer, Elwood J. The use of paired comparison techniques for evaluating forestry research. In: Proceedings, new forests for a changing world; 1983 October; Portland, OR, Bethesda, MD: Society of American Foresters; 1984; 512-516.

Decker, W. D.; Van Atta, C. M. Controlling staff age, and flexible retirement plans. Res. Manage. 16: 16-21; 1973 January.

DeCotiis, Thomas A.; Dyer, Lee. Defining and measuring project performance. Res. Manage. 22(1): 17-22; 1979.

Edwards, Shirley A.; McCarrey, Michael W. Measuring the performance of researchers. Res. Manage. 16: 34-41; 1973.

Fernandez, Don. Post hoc procedures for planning and evaluation. Eval. and Prog. Plan. 2(3): 219-222; 1979.

Freeman, Howard E.; Solomon, Marian A. The next decade in evaluation research. Eval. and Prog. Plan. 2(4): 255-262; 1979.

Galloway, E. C. Evaluating R&D performance--keep it simple. Res. Manage. 14(2): 50-58; 1971 March.

Gee, Robert E. The opportunity criterion--a new approach to the evaluation of R&D. Res. Manage. 15:64-71; 1972 May.

Grayson, A.J. Assessing the value of forestry research. Voluntary paper to Congress Group 4-3; XVII IUFRO Congress, Kyoto, Japan; 1981 September. 16 p.

Greis, I.D. Agricultural research management and the ex ante evaluation of research proposals, a review. Rev. of Market. and Agri. Econ. (Haymarket, NSW, Australia) 49(2):73-94; 1981 August.

Guttentag, Marcia. Conducting useful evaluations. In: The management of federal research and development; McLean, Virginia; MITRE Corporation; 1977 December; Tech. Report MTR 7657; 61-65.

Handler, Philip. When science becomes a public venture, to whom is the scientist accountable? Chron. of Higher Educ.; 1980 March 3. p. 64.

Keaton, P. W. Five ways to review R&D programs. Res. Manage. 23(5): 8-9; 1980 September.

Kimblin, C. W.; Souder, W. E. Maintaining staff productivity as half-life decreases. Res. Manage. 18: 29-35; 1975 November.

Kiresuk, T. J.; Sjerman, R. E. Goal attainment scaling: a general method for evaluating comprehensive community mental health programs. Com. Health J. 4: 443-453; 1968.

Kiresuk, Thomas J. Planned change and evaluation. Knowledge: Creation, Utilization, Diffusion 1(3): 405-420; 1980 March.

Kuusela, Kullerva. Evaluations in selecting and controlling a research project and estimating the applicability of its results. In: Proceedings, IUFRO Subject Group S6.06, Management of forestry research for results; 1977 September; High Wycombe, Buckinghamshire, U.K.; 1977: 54-61.

Leithwood, Kenneth A.; Montgomery, Deborah J. Evaluating program implementation. Eval. Review 4(2): 193-214; 1980 April.

Litras, T.S. A practical application of peer evaluation. Res. Manage. 23(2): 13-19; 1980 March.

Locatis, Craig N.; Smith, Jeffrey K.; Blake, Virgil L. Effects of evaluation information on decisions. Eval. Review 4(6): 809-823; 1980 December.

Lundgren, Allen L. Research productivity from an economic viewpoint. In: Proceedings, 1981 Convention, Society of American Foresters; Bethesda, MD: Society of American Foresters; 1982: 256-262.

Mandell, Marvin B. The design and selection of evaluation studies. Knowledge: Creation, Diffusion, Utilization 5(4): 419-445; 1984 June.

Marshall, Catherine. The case study evaluation: a means for marshalling organizational and political tensions. Eval. and Prog. Plan. 7(3): 253-266; 1984.

McAllister, Paul R.; Anderson, Richard C.; Narin, Francis. Comparison of peer and citation assessment of the influence of scientific journals. J. Amer. Soc. for Info. Sci. 31(3): 147-152; 1980 May.

McNeece, C. Aaron; DiNitto, Diana M.; Johnson, Peter J. The utility of evaluation research for administrative decision-making. Admin. in Social Work 7(3/4): 77-87; 1983.

Menke, M. M.; Gelzer, J.; Pezier, J. P. Evaluating basic research strategies. Long Range Plan. (UK); 14(3): 44-57; 1981.

Moeller, George H.; Shafer, Elwood L. Important factors in the forestry innovation process. J. Forestry. 79(1): 30-32. 1981.

Morell, Jonathan A.; Flaherty, Eugenie Walsh. The development of evaluation as a profession: current status and some predictions. J. Eval. and Prog. Plan. 1(1): 11-17; 1978 January.

Mosteller, Frederick. Innovation and evaluation. Science 211(4485): 881-886; 1981 February 27.

Narin, Francis; Moll, Joy K. Bibliometrics. Ann. Rev. Info. Sci. and Techn. 12: 35-58; 1977 September.

Nicholas, John M. Evaluation research in organizational change interventions: considerations and some suggestions. J. Applied Behav. Res. 15: 23-40; 1979.

Niedenfuhr, Francis W.; Lands, Lark E.; Thompson, Rochelle D. Evaluating R&D programs: workshop report. In: The management of federal R&D programs; McLean, Virginia; MITRE Corporation; 1977 December; Tech. Report MTR-7657; 111-116.

Nielson, James; Brazzel, John M. Evaluation as an aid to decisionmaking in the food and agricultural sciences. USDA Sci. and Educ. Admin., Joint Plan. and Eval. Staff; Washington DC; Paper 80-DD-02; 1980 March. 15 p.

Patton, Michael Quinn. Utilization-focused evaluation. Beverly Hills, CA: Sage Publications; 1978. 292 p.

Pflum, Glenn D.; Brown, Robert D. The effects of conflict, quality, and time on small group information use and behavior in evaluative decision making situations. Eval. and Prog. Plan. 7: 35-43; 1984.

Quinn, James L. A categorization of the methods and techniques of measuring industrial R&D activities. Air Force Institute of Technology, Wright-Patterson Air Force Base, Ohio SLTR2-71; NTIS AD-727 028; 1971. 97 pp.

Rafter, Davis O. Three approaches to evaluation research. Knowledge: Creation, Diffusion, Utilization 6(2): 165-185; 1984 December.

Rieker, Patricia P. Evaluation research: the design-to-use process. Knowledge: Creation, Diffusion, Utilization 2(2): 215-235; 1980 December.

Ross, Abraham S. Practical problems associated with programme evaluation in third world countries. Eval. and Prog. Plan. 7: 211-218; 1984.

Ross, John A. Decision rules in program evaluation. Eval. Review 4(1): 59-74; 1980 February.

Rubenstein, A. H. Effectiveness of federal civilian-oriented R&D programs. Policy Studies J. 5: 217-227; 1976.

Rubenstein, A. H.; Chakrabarti, A. K.; O'Keefe, R. D.; Souder, W. E.; Young, H. C. Factors influencing innovation success at the project level. Res. Manage. 19(3): 15-20; 1976 May.

Rubenstein, A. H.; Schroder, Hans-Horst. Managerial differences in assessing probabilities of technical success for R&D projects. Manage. Sci. 24(2): 137-148; 1977 October.

Salasin, John; Hattery, Lowell; Ramsay, Thomas. The evaluation of federal research programs. McLean, Virginia. MITRE Corporation; 1980; Tech. Report MTR-80W129. 69 p. plus appendices.

Schainblatt, Alfred H. How companies measure the productivity of engineers and scientists. Res. Manage. 25(3): 10-18; 1982 May.

Shaw, Byron T. The use of quality and quantity of publication as criteria for evaluating scientists. Washington, DC: USDA Agr. Res. Serv.; 1967; Misc. Publ. 1041. 78 p.

Shumway, C. Richard. Subjectivity in _ex ante_ research evaluation. Amer. J. Agr. Econ. 63(1): 169-173; 1981 February.

Smith, Nick L. The creative process. Knowledge: Creation, Diffusion, Utilization 3(3): 371-380; 1982.

Stahl, Michael J.; Steger, Joseph A. Measuring innovation and productivity--a peer rating approach. Res. Manage. 20(1): 35-38; 1977 January.

Stanfield, Jonathan; Smith, Nick L. Management consulting and evaluation. Eval. and Prog. Plan. 7: 87-93; 1984

Stats, Elmer B. Evaluating federal R&D programs. In: The management of federal research and development; McLean, Virginia; MITRE Corporation; 1977 December: Technical Report MTR 7657. 43-48.

Suchman, Edward A. Evaluative research. New York: Russell Sage Foundation; 1967. 186 p.

Tauss, Kurt H. A pragmatic approach to evaluating R&D programs. Res. Manage. 18(5): 13-15; 1975 September.

Tornatzky, Louis G. The triple-threat evaluator. Eval. and Prog. Plan. 2: 111-115; 1979.

United States General Accounting Office. Assessing social program impact evaluations: a checklist approach. Exposure Draft, PAD-79-2; 1978 October.

Whiting, Bruce G. How to predict creativity from biographical data. Res. Manage. 15: 28-34; 1972 November.

Wholey, Joseph S. Evaluation: promise and performance. Washington, D.C.: The Urban Institute; 1979. 234 p.

Windle, Charles. Developmental trends in program evaluation. Eval. and Prog. Plan.: 2(3): 193-196; 1979.

BRIEF HISTORY OF FORESTRY RESEARCH EVALUATION IN THE UNITED STATES

Allen L. Lundgren

Consultant,
765 Redwood Lane
New Brighton, MN 55112

Abstract

The evaluation of forestry research is not new. In choosing among competing research projects and programs, those who do research, manage research, and fund research have always had to evaluate research alternatives in making their selections. What has been lacking in these evaluations is a systematic, scientific approach to evaluation. Sporadic attempts have been made over the years to develop improved methods for evaluating forestry research projects, but these have never been widely applied or accepted. Only recently has the forestry research community supported a sustained, systematic scientific approach to evaluating forestry research.

Tightening budget constraints have renewed interest in improving research evaluation methodologies in forestry. These concerns initiated a modest but sustained effort to explore and develop improved methodologies that could be used to evaluate forestry research. Studies have been and are being conducted within the U.S. Forest Service and at several universities throughout the country. Results from several of these studies are just becoming available. They indicate that some forestry research has provided relatively high rates of return on the resources invested in forestry research. Other studies of forestry research have provided a better understanding of the research process and of the impact of particular research in forestry. Research evaluation in forestry is still in its infancy, and much remains to be done in developing evaluation methods to improve research decisions.

Introduction

This brief history of forestry research evaluation will highlight past efforts to evaluate forestry research and review recent developments. Most of this history is restricted to developments in the United States, although a few references to Canadian work have been included as well. But before reviewing the history of forestry research evaluation it may be helpful to outline my view of this topic so you can better understand my thinking about the subject.

The topic "forestry research evaluation" is difficult to define. As we will see, forestry research evaluation includes a complex bundle of many different evaluation problems. First, evaluation implies determining the worth of something. But this in turn implies some scale or scales of values. Values are subjective and depend upon the human individuals involved. In any evaluation problem the question of whose values must be addressed in the analysis. Those who fund research, manage research, disseminate research, and use research each have different interests in the research process. They may want answers to different questions about the research process. They certainly would apply different value systems in evaluating reseach alternatives. Second, forestry research itself is a complex activity.

Many different agencies, organizations, and groups of people are involved in forestry research, or have a direct or indirect interest in forestry reseach including:

- those who fund forestry research, including federal and state legislatures and agencies, public and private foundations, industry associations and companies;
- those who do research, including administrators, managers, scientists, and research support personnel;
- those who disseminate research findings, including professional journals, research organizations, individual researchers, extension and technology transfer agents, consultants, users of results;
- those who use results from forestry research, including other researchers, land planners, administrators, and managers, resource users, forest product industries;
- those who are affected by the use of research results, either directly or indirectly, as consumers of goods and services in society, or through the effects of changing technology on employment, income distribution, and other social factors.

The forestry research system involves many elements, including:

- Organizations
- People
- Facilities
- Funding
- Problem areas

* Scientific and technical knowledge

* Users

Many different kinds of research are conducted at various locations, on different problems, using different scientific disciplines. Choices must be made among kinds, locations, timings, and intensities of research, among research methodologies, and among methods of dissemination.

Any of these aspects, or any combination of them, could be the subject of an evaluation, using different methodologies, and involving different value systems. In reviewing the history of forestry research evaluation I have chosen to adopt a broad view of research evaluation, one that includes a wide range of interests in the entire process of forestry research.

Approaches To Evaluation

The possible subjects for evaluation in forestry research appear to be almost endless. For example, one could evaluate:

* various organizational structures, public or private, in terms of research effectiveness;

* productivity of different scientists;

* adequacy of available facilities and their impact on research;

* potential contributions to economic or social development of research programs or projects in different problem areas or in different scientific fields;

* contributions of scientific knowledge to technological development.

These only hint at the variety of potential subjects for research evaluation in forestry. What they do suggest is that there is no one single problem of forestry research evaluation. Rather, there are a large number of different problems, each arising from the concerns of different people and requiring different approaches to evaluation.

These different concerns and evaluation approaches are evident when one studies the literature dealing with the evaluation of forestry research. In reviewing the history of forestry research evaluation I will discuss some of the different approaches to research evaluations that have been used in the past under several broad headings:

* Peer evaluation

* General surveys of forestry research

* Reports of research organizations

* Research programs and statements of research needs

* Investigative reports of forestry research

* Methods for research planning and project selection

* Research studies of forestry research

These headings reflect different research concerns and approaches to the evaluation of forestry research.

Peer Evaluation

One type of research evaluation that arose early in the development of science was evaluation by scientific peers. Peer evaluation may be formal or informal, direct or indirect, operating through reviews of study plans or problem analyses, reveiws of manuscripts, and critiques and evaluations of previously published works. This type of research evaluation is a long-established practice in forestry research, as in other branches of science. It is such an in-grained part of forestry research that I see no need to review its history in any detail.

Peer evaluations generally take place within scientific disciplines. Although such peer evaluations are not always effective in providing an unbiased evaluation of research (Broad & Wade 1982), they do provide a means of socializing science, of developing commonly accepted paradigms within a field of science.

General Surveys of Forestry Research

Over the years there have been several major national studies of forestry research that assessed the status of forestry research and suggested new directions for future research. Many of these general surveys included evaluations of existing forestry research institutions and programs. An early study by Clapp (1926) described forestry research efforts at existing research institutions, assessed the adequacy of existing knowledge about forestry and the forest resource, identified gaps in knowledge, and recommended increases in funding for forestry research in the U.S. A 1929 report by Bailey and Spoehr (1929), "The role of research in the development of forestry in North America," evaluated the capabilities of forestry research institutions in North America to conduct research in forestry. In 1938, a report on "Forest research in the United States" by the National Research Council, Committee on Forestry, Division of Biology and Agriculture described the extent and character of forestry research in the U.S. in terms of numbers of research projects by field of investigation, region of the country, and agency. It concluded that forestry research had strongly influenced forest practices and policy in only a few fields.

In 1955 Kaufert and Cummings prepared a report for the Society of American Foresters, Forestry Research Project, entitled "Forestry and related research in North America." They reported on programs, funding, personnel, and accomplishments of forestry research institutions. Although the report provided considerable information about forestry research in North America, including Canada and Mexico, there is little systematic formal evaluation of research programs, and no establishment of research priorities in this report.

In 1971 Smith and Lessard published a comprehensive study of forest resources research in Canada conducted for the Science Council of Canada. The authors assessed existing research programs and concluded that there were a large number of inequitable allocations of resources among disciplines, regions, agencies, and programs.

Such broad general surveys of forestry research programs provide general information about the extent and direction of forestry research that can be helpful in developing research policies and programs. Any recommendations usually have been based on the personal judgements of the members of the study team. To date they have typically provided little or no information about the costs and benefits of research.

Reports of Research Organizations

Periodic reports by research organizations, such as the annual attainment reports of the USDA Forest Service, often describe existing research programs and report research accomplishments, usually in terms of publications and other outputs produced. By their very nature such reports tend to concentrate on reporting the production of new knowledge and technologies. Although such reports may contain estimates of potential use of the new knowledge produced, they say little about the actual application of such knowledge.

Research Programs and Statements of Research Needs

Every researcher, research group, and research organization is periodically confronted by the need to develop a program of research based on perceived needs, however derived. Such statements of programs and needs imply some evaluation of potential alternative research projects and programs. Undoubtedly much of this exists in the form of internal unpublished documents, but a large published literature also exists. A detailed review of even the published literature is beyond the scope of this paper, but it is instructive to examine a few of the more recent reports for their evaluation methodologies.

A national program of research for forests and associated rangelands in 1975, with projections to 1980 and 1985 was described by Holland (1978). This was later updated to 1990 by Shafer (1982). The program was developed in a series of regional workshops, where forestry researchers, research users, and interest groups evaluated potential research program areas and developed research priorities based on their concerns, value judgements, and preferences. Such broad research appraisals may be helpful in determining national research priorities, but they are of little help in determining funding levels for research because they do not address the important issues of the level, distribution, and timing of the costs and benefits to be expected as a result of such research.

In 1979, a national task force on basic research in forestry and renewable natural resources met at Grey Towers, Milford, Pennsylvania. The report of this task force, "Our natural resources: basic research needs in forestry and renewable natural resources" (Krugman & Cowling 1982), identified five general areas of science that offered challenging and valuable opportunities for basic research:

- basic biology;
- ecological research;
- engineering and materials sciences;
- economic and consumer benefits; and
- management systems.

These needs for basic research were developed by a highly qualified panel of scientists, and undoubtedly reflect their own particular knowledge and concerns.

In addition to such general assessments of research needs, the literature contains many assessments of research needs and proposed research programs within individual scientific fields. Time does not permit reviewing these here, but generally they reflect the evaluation of research alternatives based upon the personal judgement of professionals working in the field, along with consultation with other scientists, professionals, and other interest groups.

Investigative Reports of Forestry Research

In 1971 the General Accounting Office (GAO) of the U.S. Government conducted a review of the U.S. Forest Service research program to evaluate the extent to which Forest Service research was being used in managing forest lands and resources (Comptroller General 1972). The GAO study concluded that although some managers were using some of the findings, many research findings were used to only a limited extent, and some were not being used at all, although these findings had been cited by Forest Service research as accomplishments. The report pointed out the need to improve the usefulness and use of research findings.

In 1975 the Office of Audit, Office of the Secretary, U.S. Department of Agriculture, conducted an unpublished internal review of Forest Service research dissemination in the Eastern U.S. Although this evaluation found that the Forest Service had made several improvements in disseminating and applying research results, it pointed out the lack of a system for delivering research findings to some potential users, and an inadequate system for getting feedback from research users to research doers.

In 1982, the GAO conducted a follow-up review of its 1972 evaluation of Forest Service research. Although it noted improvements in research dissemination within the Forest Service, GAO concluded that a more systematic approach was needed to ensure that research was directed to maximize benefits from land management and that the best use was made of research findings.

These three investigations of the U.S. Forest Service were concerned with evaluating the uses being made of its research. All three investigations implied that if research findings are not used by the intended users, then that research is of little value to society. All three stressed the need to consider the entire research system, from research to user, in evaluating the effectiveness of research.

Methods for Research Planning and Project Selection

In planning forestry research and selecting research projects to work on, forestry researchers have often used a number of different approaches to evaluate and to justify their choices. We don't have time to review these in any detail. Most of these methods are well known to all of you, and you may have used several of these in your own work. Nevertheless, it may be helpful to briefly review these to remind ourselves that the evaluation of research alternatives is an on-going process within forestry research, and that over the years forestry researchers and managers have developed a number of approaches to deal with this problem.

Informal/Intuitive

Many of us have often relied on our experienced judgement and intuition in selecting research programs or projects, without any formal method of evaluation or consideration of alternatives. Despite its non-rational basis, such an evaluation approach has often proven highly effective in science throughout history. Documentation of this approach is rare in the published literature, and I have found no formal evaluations of its effectiveness.

Pressing Problem

Forestry research is often undertaken to solve pressing problems identified by various interest groups in forestry. Here, the evaluation of research alternatives is based upon the need to find a solution to some critical problem. It is frequently used by applied forestry research organizations to justify research expenditures. Research choices reported by the USDA Forest Service Forest Products Laboratory (Fleischer 1974) illustrate this method of evaluating potential research.

Peer Judgement

Peer judgements are commonly used in science to evaluate research proposals, scientific publications, and scientific accomplishments of individuals, research groups, and research institutions. Historically, this is perhaps one of the most common methods used to evaluate research, although the results of such group deliberations are rarely published.

Scientific/Logical

The scientific/logical method of evaluating research alternatives depends on a formal logical analysis of the scientific problem, together with the development of testable hypotheses (Platt 1964). There are few published examples of this hypothetico-deductive approach in the forestry literature. Romesburg (1981) examined wildlife research publications and concluded that this approach was rarely used in wildlife research. He strongly urged the increased use of this method of evaluating research alternatives as a means of improving the quality of research in the field.

Ranking Of Alternatives

One approach to evaluating research alternatives is to develop systems for scoring alternatives according to pertinent characteristics, and then ranking research alternatives based on their scores. For example, Addy et al. (1971) developed a scheme for ranking insects as potential subjects for research based on their economic, ecologic, environmental, and social importance. Babcock

(1974) reported on a system developed by the Canadian Forestry Service for evaluating forestry research projects based on:

- Contribution to national income;
- Contribution to foreign exchange earnings;
- Amelioration of income disparity;
- Meeting outdoor recreation needs;
- Contribution to public health and safety;
- Meeting housing needs;
- Maintaining or enchancing the natural environment;
- Expanded employment opportunities; and
- Contribution to knowledge.

A subjective, but systematic approach was used in developing scores for these characteristics for each potential project. Benefit/cost ratios were also estimated for each project. This type of systematic approach allows others to better understand the basis for research evaluation.

Strategic Planning

Research has been evaluated using a strategic planning approach, where the goals of the research organization are stated, and a set of strategies to reach these goals are developed. This approach to research evaluation was used at Weyerhaeuser (Gregory 1974):

- Corporate goals were stated;
- Potential gains from the adoption of new technology were identified;
- Costs and benefits of developing and adopting the new technologies were evaluated; and
- Research priorities, defining a program of research, were set.

Structured Analysis

Structured methods, such as critical path scheduling and convergence analysis, have been used by research organizations and groups to evaluate forestry research opportunities. However, their use has rarely been documented in the literature.

Mathematical Optimization

Attempts have been made to apply some . orm of mathematical optimization to evaluating alternative research opportunities. Bethune and Clutter (1969) reported on attempts to use a dynamic programming model to allocate research funds among and within broad forest types in Georgia to maximize benefits from investments in forestry research. Claxton and Renzi (1972) and Renzi and Claxton (1972) suggested an analytical procedure incorporating an integer programming algorithm for determining an "optimum" research program. I have not found any published reports of the use of either of these two models. Potential users of such mathematical optimization models face almost insurmountable problems of uncertainty in forecasting potential benefits and costs of any proposed research.

Systems Evaluation

One approach to evaluating research is to construct a model of the problem of interest using existing knowledge and data. This model can then be used to evaluate the adequacy of existing information and to determine critical information needs. Holling et al. (1979) developed a model of the spruce budworm/forest system to define a list of research priorities focused on critical management needs. Clark and Stankey (1979) identified several research needs in outdoor recreation in the process of developing a comprehensive system for identifying types of outdoor recreation opportunities. Such models of complex systems do not of themselves constitute a research evaluation system, but they do systematically uncover deficiencies in knowledge that, coupled with estimated costs and benefits of having improved information, provide a basis for the evaluation of potential forestry research programs and projects.

Research Studies of Forestry Research

Over the years many interesting but isolated studies, have been conducted on various aspects of forestry research. Many of these exist only as unpublished graduate theses. These studies have not as yet developed a recognizable sub-discipline within the forestry profession with its own scientific traditions. They have been on diverse subjects, scattered over time, and often bear little or no relation to previous work in forestry research evaluation. For purposes of discussion I have arbitrarily classified these diverse studies into eight classes:

- History of Research;
- Broad Research Policies, Programs, Directions, Opportunities;

* Research Organizations, Administration, Management;
* Research Process;
* Evaluation of Individual Projects and Programs;
* Dissemination of Research Results, Technology Transfer;
* Adoption of Technology, Technological Innovation; and
* Technological Change, Productivity.

History of Research

Although histories of science and biographies of scientists are not evaluations of research, they do provide useful information for research evaluations, and clues as to important factors in the development of science. For example, the biography of Raphael Zon (Schmaltz 1980), who was the first director of the USDA Forest Service Lake States Forest Experiment Station, illustrates the impact that a strong personality can have on the development of a research organization and on its research program.

A recent study of the evolution of forest yield determination and site classification (Tesch 1981) illustrates how such studies can provide an understanding of the complex dynamic process by which science develops over time in particular fields.

Verrall (1982), in reviewing research accomplishments at the USDA Forest Service Southern and Southeastern Forest Experiment Stations, concluded that the monetary savings to southern forestry and wood utilization more than offset the costs of the research at the two stations up to 1960, although he lacked detailed estimates of monetary costs and benefits. In his history of electronic communication in the USDA Forest Service, Gray (1982) illustrates the strong role that personalities and their individual beliefs can play in the adoption of a new technology. Doig (1981), in his history of the USDA Forest Service Pacific Northwest Forest and Range Experiment Station, and Rudolf (1985), in his history of the old Lake States Forest Experiment Station, illustrate how the particular interests and abilities of individual scientists, their relationship with research users, and the funds and facilities available at the time can influence the research program of a research institution.

A history of fire policy and research in the USDA Forest Service (Pyne 1981) illustrates how changes in management policies can affect the course of research that is closely tied to field problems.

Broad Research Policies, Programs, Directions, Opportunities

Broad research policies, programs, directions, and opportunities have been evaluated in a number of studies. For example, Fege (1979) described programs of forest management research in the United States by discipline and organization, providing documentation of research funding.

A review of forest and rangeland research policies in the U.S. by a symposium at Airlie House, Warrenton, Virginia, May 30 - June 3, 1977 (Renewable Natural Resources Foundation 1977), developed several recommendations for enhancing the performance of the entire renewable natural resource research establishment.

Whaley and Bell (1982), in analyzing the health of forestry economics research in the USDA Forest Service, recommended a number of changes in the research program that reflected their views as research administrators and their previous experience in the field, although they made no formal study of the program.

Research Organization, Administration, and Management

Much of the published information about research organizations, administration, and management relies heavily on the experience of the authors. Few publications are the result of formal studies of forestry research organizations. This literature often documents existing practices, and offers insights into potential problems within research organizations that would be helpful in developing research evaluation methodologies to meet specific organizational needs.

Anderson (1973) suggested evaluating three aspects of research laboratories: the organization itself, the individuals in it, and the leadership. Carter (1982) pointed out the need to consider alternative sources of funding for research. Ethington (1979) and Guthrie (1979) explored future requirements for research personnel. Ketcham and Shea (1982), Malac (1982), and Tombaugh (1982) described research decision making in different forestry research organizations. Nissan (1981) explored potential conflicts between traditional goals of academic research and management of research by objectives.

In the early 1970's the University of Michigan's Center for Research on the Utilization of Scientific Knowledge (CRUSK) conducted a detailed study of USDA Forest Service research installations and scientists (Lingwood and Morris 1976). They identified a number of factors that affected the attitude and performance of scientists within a major research organization such as the Forest

Service. In a more detailed report on one phase of this study, Barnowe (1973), in his unpublished PhD thesis, identified several factors under the control of research administrators that appeared to affect research effectiveness, including: rewards, opportunities for information exchange, and encouragement of researcher-client linkages. Schreyer (1974) reported on another aspect of this study, the orientation of individuals and research factors that influenced scientific effectiveness. He identified factors that could be used in recruiting and developing individuals within the Forest Service research organization.

Huddy (1979) evaluated the administrative-operative procedures of the Cooperative Forestry Research Program under the McIntire-Stennis Act of 1962. Huddy concluded that the procedures currently used to establish research priorities in the program are not an appropriate or accurate means of evaluating research output.

Research Process, Planning and Conducting Research

There are few published studies of the actual processes used in planning and conducting forestry research. One of the few is a study by de Steiguer (1979) of public participation in the National Program of Research for Forests and Associated Rangelands, a research planning effort sponsored by the U.S. Department of Agriculture and the National Association of State Universities and Land Grant Colleges. He found that although the research priorities expressed by the conference delegates did represent the research priorities of the populations from which they were chosen, their preferences were not reflected in the final allocation of scientist years to research. In a further analysis, de Steiguer and Massey (1981) found that research priorities of industry were clearly different from the priorities of other groups.

Evaluations of Individual Projects and Programs

Techniques for the economic evaluation of forestry projects are well-known. These techniques have also been applied to forestry research. Herrick (1982) estimated the potential benefits of research and subsequent adoption of whole-tree chipping in northern U.S. forests, but did not estimate the costs of research, dissemination, or technology adoption. Risbrudt and Kaiser (1982) evaluated the U.S. Forest Service State and Private Forestry sawmill improvement program. They estimated the benefits from adopting this new technology in terms of reduced lumber prices to consumers, and the costs of technology transfer, but did not include the costs of the research itself in their analysis.

Rose (1983) evaluated the Douglas-fir Tussock moth research and development program, which is part of the USDA Combined Forest . est Research and Development Program. Using a sensitivity analysis, he found that the estimated benefit/cost ratios for the program were sensitive to the assumptions made, and under some conditions the benefits would be less than the costs. He suggested a framework for evaluating research funding alternatives using benefit/cost analysis.

In attempting to measure benefits from ten selected research projects at the Western Forest Products Laboratory of the Forestry Service, Environment Canada, Taylor (1973) encountered several major methodological problems, including:

* Benefits that were difficult or impossible to measure;
* Benefits that could only be measured in part;
* Measurements that were unrepresentative;
* Assumptions that were necessary.

Because of complicating institutional, technical, marketing, and cost factors he was able to derive benefits that could be used for benefit/cost analysis for only one project. He concluded that some projects defy benefit measurement.

In a study of eighty-one innovations resulting from research by the USDA Forest Service, Callaham (1981) categorized benefits into 16 general classes:

* Income/employment generated in forest industry or regional economy;
* Increased utilization of natural resources;
* Improved quality of physical/biological environment;
* Lowered prices or costs to consumers;
* Reduced costs for managing resources;
* Improved methods for planning and evaluating alternative investments;
* New and improved products;
* Improved visual environment and related amenities;
* Increased resource productivity;
* Reduced costs through improved processes;
* Improved scientific methods and theory;

- Improved social environment;

- Increased quality, lowered cost of housing;

- Enhanced health and safety;

- Improved cultural/historical/geological environment;

- Enhanced public involvement in decision making.

Callaham reported that the median time from the conception of the research to its general availability was 12.5 years, and ranged from 3 to 48 years. He estimated that the monetary benefits from 22 of the 81 innovations would, in the first year alone, exceed the cost of all prior Forest Service research on these innovations. Although he did not calculate an actual rate of return, he concluded that investments in such research have yielded a high rate of return.

Dissemination of Research Results, Technology Transfer

There have been several studies of research dissemination and technology transfer in forestry. Muth and Hendee (1980) reviewed the literature on diffusion and suggested five characteristics of innovation that influence the rate and degree of diffusion and adoption:

- Relative advantage;

- Compatability;

- Complexity;

- Trialability; and

- Observability.

They characterized the adoption behavior in a social system in terms of innovators, early adopters, early majority, late majority, and laggards.

In a study of technology transfer within the national forest system of the USDA Forest Service, Pugh-Roberts Associates, Inc. (1981a, 1981b) concluded that effective communication can facilitate technology transfer, but it was the pressures and rewards for improved performance that drove managers to seek better ways to do things. They recommended that the Forest Service:

- Develop procedures and reward systems to encourage closer working relationships between researchers and potential users.

- Develop an implementation strategy for each innovation that separately targets: potential users with existing task needs; potential users identified as risk-takers; and other potential users.

- Establish communication systems about new technologies which facilitates the setting of task objectives based on the innovations, and which foster familiarity among potential users.

In studying the diffusion of the Code-A-Site campsite inventory system within the USDA Forest Service, Roggenbuck and Watson (1980) documented the communication channels through which land managers obtained information about this innovation. The primary source for first learning about this new technology was the distribution of Forest Service publications by regional recreation personnel. For more information about the advantages of using this system, managers relied on other individuals within the Forest Service.

Lucas (1981) studied the distribution of a brochure intended to influence the choice of trails by visitors to the Selway-Bitteroot Wilderness in Montana. He found that most visitors never saw the brochure. Only one-quarter of them had the brochure before they reached the trailhead, and only one-quarter of these actually used it to choose trails. The lesson from this study is that careful attention must be given to the distribution of information intended for use. Such information must address potential user's needs, and be given to them at a time and place that will be effective in changing their behavior.

In a study of how forestry personnel in the Wisconsin Department of Natural Resources obtained technological information, Nicholls and Prey (1982) found that the most frequently cited sources were DNR training and workshops, and from co-workers and other professionals. Scientific and professional journals were the least cited sources. This strong reliance on internal and personal communication in the transfer of information among people within organizations supports the findings of Roggenbuck and Watson (1980) cited earlier.

There have been other studies of research dissemination and technology transfer, but those cited above illustrate the type of work that has been done. It seems obvious that the dissemination and transfer of information about technological innovations is a complex process, one that we do not yet fully understand. Careful study of this process could lead to more effective dissemination and transfer of technology, and thus affect the distribution of potential benefits from research over time and among people.

Adoption of Technology, Technological Innovation

Relatively few studies have been made of how technology is adopted by potential users in forestry. Moeller and Shafer (1981) studies 81 successful innovations from research sponsored by the USDA Forest Service. The factor most frequently cited as important in the forestry innovation process was the adaptation of existing technology. Testing and pilot studies to demonstrate feasibility were also frequently cited. They concluded that many practical problems could be solved by using existing knowledge and technology, through the use of special teams focusing on these problems.

Cox (1974) argued that the key to successful innovation was the commitment of top management to technological innovation and the support of an entrepreneur or venture manager to forceably back the innovation. To nourish technological entrepreneurs an organization must have a business environment characterized by decentralization and delegation. He spoke from his experience as Director of Technology Assessment with MacMillan Bloedel Ltd.

Porterfield (1980) cited the need for researchers to work closely with intended users for effective adoption of technology. He argued that research rarely considers the full operational environment, and hence findings must be modified in the process of adoption. He pointed out that at Champion International Corporation research planning is done jointly by research and operations, and that often experiments are installed and data collected by operations people, which greatly enhances the implementation of research results.

Technological Change, Productivity

Although there have been several studies of technological change and its influence on productivity in forestry and forest products industries, there has been no comprehensive effort to study technological change systematically throughout forestry. An early paper by Ruttan and Callahan (1962) examined prices and productivity changes in agriculture and in forest products industries as indicators of resource scarcity. They concluded that if technological change in timber production, harvesting, and utilization were to move ahead rapidly in the future, timber supply and demand projections may have to be modified to account for changes in input requirements. Bentley (1970) reviewed the literature on technological change in the forest industries and found only a few studies.

Robinson (1975) found substantial technological change in the lumber and wood products industry over the years 1949-1970, with a resulting reduction in industry employment and stumpage requirements per unit of output. A later study of this same industry segment for the years 1951-1973 by Greber and White (1982) found that during this period the efficiency of labor rose steadily at an annual rate of 2.9 percent, while the efficiency of capital fluctuated and showed an overall decline of about 0.5 percent annually. They concluded that during this period technological change provided most of the growth in productivity in this industry. Stier (1980) found that technological change had been almost exclusively labor-saving in ten U.S. forest products industries. In contrast, a later study by Jorgenson and Fraumeni (1981) found that technical change in the U.S. lumber industry was capital using, labor using, energy using, and material saving. Technical change in the paper industry was found to be capital saving, labor using, energy using, and material saving. The use of different analytical approaches can lead to drastically different conclusions.

Buongiorno and Gilless (1980) estimated that real prices of paper and paperboard declined 1.5 to 2 percent annually as a result of technological change, but found no effect on wood pulp prices.

This brief review has not been able to report on all of the studies that have been made of forestry research and the use of its findings. If any message comes through from this review it must be that much of the work to date has been piecemeal. Until recently there appears to have been no concerted effort to develop a systematic research program to study forestry research in its many dimensions, or to develop methodologically sound and broadly acceptable methods for evaluating forestry research.

Recent Developments in the Evaluation of Forestry Research

Judging by the increase in published studies in recent years, there appears to be a rising interest in the evaluation of forestry research. Further evidence of this was the initiation in 1980 by the USDA Forest Service of a national program to develop improved methods for the evaluation of forestry research. To my knowledge, this was the first formal program of research established with the goal of developing improved methods for evaluating forestry research.

This program was begun at the USDA Forest Service North Central Forest Experiment Station in St. Paul, Minnesota, where it is still headquartered today. I directed this program during its first two years. In 1983 a research work unit was established at the NCFES to continue this research under a more formal program designation, under the direction of project leader Chris Risbrudt. Pamela Jakes is now project leader of this program.

One of the aims of this program is to encourage and fund the establishment of a few centers for research on forestry research evaluation at universities around the country. This was done through the development of cooperative research agreements and research grants with researchers at several universities. Many of these original research projects have just recently been completed, and the findings from several of them are in the process of being published.

I will not review the current status of these research projects here. The current status of many of these and other ongoing research projects relating to the evaluation of forestry research were recently reported at a Research Evaluation Workshop held at the Holiday Inn, Roseville, Minnesota, 20-21 August 1984, the proceedings of which are now in press (Risbrudt & Jakes 1985). Other papers presented at this session will report on parts of this research program.

One of the few publications available to date as a result of this national program of research on research is the evaluation of research on structural particleboard by Bengston (1984). Bengston estimated both the benefits and costs of this research, and reported an average internal rate of return from investment in structural particleboard research that ranged from 19 to 22 percent, depending on assumptions regarding price differentials between structural particleboard and softwood plywood, price elasticity of demand, and projected future production. Marginal rates of return for additional research on structural particleboard ranged from 27 to 35 percent.

In addition to the USDA Forest Service's research evaluation program of studies, a number of other studies on research evaluation have been undertaken in recent years. A study of "The Process of Productivity Change in the Forest Products Industry," funded by the USDA Forest Service Forest Products Laboratory and conducted by the University of Minnesota College of Forestry, was completed in 1983. Final results have not yet been published, but Strees (1984) reported on "Productivity Change in the U.S. Forest Industries" in her PhD Thesis.

A major program to evaluate forestry research in the Southern U.S. was started in 1982 at Duke University School of Forestry and Environmental Studies, funded by the USDA Forest Service and Cooperative State Research Service in cooperation with Duke University and Mississippi State University Agricultural and Forestry Experiment Station (see Hyde 1983).

A number of other efforts to evaluate forestry research are underway at several universities in the U.S., but a thorough documentation of their current status would have to be the subject of another paper. In the next year or two we can expect to see the publication of results from several significant studies of forestry research that will enhance our understanding of forestry research.

Conclusions

What have we learned from this historical tour of forestry research evaluation? One important lesson may be that we now recognize that the evaluation of forestry research is not a single evaluation problem, but a complex of evaluation problems. Many people, both inside and outside of forestry research have concerns about the benefits, costs, direction, and usefulness of forestry research, which must be addressed by some form of research evaluation. The problem we face in forestry research is that we lack the systematic knowledge we need to improve our present research evaluation practices. Improved evaluation methods are needed so that we can provide more useful information to the many different groups of people who need it - those who fund research, administer, manage, and do research, disseminate research results, use research results, as well as those who use or consume goods and services affected, directly or indirectly, by forestry research, and pay taxes to provide the funds for public research expeditures.

Many useful methods for studying and evaluating research are available. Studies to date suggest that many of these can be applied to various aspects of forestry research. The problem facing forestry research evaluation is not so much the development of new evaluation methods, as the development of evaluation strategies that will provide the information that evaluation users need at the time they need it. We need to find out what methods of evaluation are applicable to particular problems, given the availability of information about the research and the particular questions to be answered about the research.

In order to adequately evaluate the benefits and costs of forestry research, any research evaluation strategy must consider not only the production of knowledge and technology, but also its dissemination, adoption, and use. It also must include the assessment of the impacts of technological innovation on the economy and society.

Generally, strategies for evaluating forestry research should consider both quantitative and non-quantitative methodologies to adequately address the many different concerns about forestry research. At best, quantitative methods will provide only part of the information needed for a full appraisal of research alternatives. In general, fundamental or basic research should rely more on qualitative than on quantitative approaches. As the research becomes more applied and developmental, quantitative methods become more appropriate.

We cannot afford to neglect the lessons to be learned from the substantial experience in research evaluation in other fields outside of forestry. Forestry has always been an integrative science, drawing upon the knowledge, expertise, technologies, and experience in other fields for adaptation to forestry. This same approach would serve us well in forestry research evaluation. There is a large literature available in many scientific disciplines that can be of considerable use in evaluating and studying forestry research.

The evaluation of forestry research is still in its infancy. We have a lot to learn about the process of forestry research and how best to go about evaluating research alternatives. If we are to convince others of the effectiveness of forestry research, then those of us who are in forestry research need to develop a better understanding of it, and develop better information to help us in planning and justifying research programs. Research on forestry research may be one of the most important areas of research in the field of forestry today.

Literature Cited

Addy, Norton D. et al. 1971. Impact of insects on multiple-use values of north-central forests: an experimental rating scheme. USDA For. Serv. Res. Pap. NC-57, St. Paul, MN; North Central For. Exp. Sta. 8p.

Anderson, Arthur G. 1973. Some parameters of productive research organizations. p. 9-14, In, "Research Management," Kent T. Adair (chmn.), Proceedings, Technical Session, Forest Product [sic] Research Society, 27th Annual Meeting, Anaheim, CA, June 28, 1973. Madison, WI; Forest Prod. Res. Soc. 68 p.

Babcock, H.M. 1974. Deciding on priority projects. For. Prod. Jour. 24(9):52-54. Sept.

Bailey, I.W.; Spoehr, H.A. 1929. The role of research in the development of forestry in North America. New York; Macmillan Co. 118 p.

Barnowe, James Thaddeus. 1973. Scientific and applied effectiveness of research installations in the U.S. Forest Service, PhD Thesis, Ann Arbor, MI; University of Michigan. 206 p.

Bengston, David N. 1984. Economic impacts of structural particleboard research. Forest Science 30(3):685-697.

Bentley, William R. 1970. Technological change in the forest industries -- a problem analysis. Univ. of Wisconsin Forestry Res. Notes No. 151. Madison, WI. 9 p.

Bethune, James E.; Clutter, Jerome L. 1969. Allocating funds to timber management research. For. Sci. Mono. No. 16. Washington, DC; Soc. Amer. For. 16 p.

Broad, William; Wade, Nicholas. 1982. Betrayers of the truth. New York; Simon and Schuster. 256 p.

Buongiorno, Joseph; Gilless, James K. 1980. Effects of input costs, economies of scale, and technological change on international pulp and paper prices. For. Sci. 26(2): 261-275. June.

Callaham, Robert Z. (coordinator). 1981. Criteria for deciding about forestry research programs. USDA Forest Service Gen. Tech. Rep. WO-29. Washington, DC. 52 p.

Carter, Mason C. 1982. Realities of research planning and implementation. p. 292-296, In, "Increasing Forest Productivity," Proc. of the 1981 Convention of the Society of American Foresters. Bethesda, MD.

Clapp, Earle H. 1926. A national program of forest research. Washington, DC; Society of American Foresters; American Tree Assoc. 232 p.

Clark, Roger N.; Stankey, George H. 1979. The recreation opportunity spectrum: a framework for planning, management, and research. USDA For. Serv. Gen. Tech. Rep. PNW-98. Portland, OR; Pacific Northwest Forest and Range Exp. Sta. 32 p.

Claxton, H. Dean; Rensi, Giuseppe. 1972. An analytical procedure to assist decision-making in a government research organization. USDA For. Serv. Res. Pap. PSW-80, Berkeley, CA; Pacific S.W. For. & Range Exp. Sta. 20 p.

Comptroller General of the United States. 1972. Report to the Congress: The Forest Service needs to insure that the best possible use is made of its research findings. Comptroller General Report to Congress, Dept. Agr. B-125053. Washington, DC. 29 p.

Cox, Lionel A. 1974. Transfer of science and technology in successful innovation. For. Prod. Jour. 24(9):44-48. Sept.

deSteiguer, Joseph Edward. 1979. Public participation in forestry research planning. PhD thesis, Texas A & M Univ., 115 p.

deSteiguer, J. E.; Massey, J. G. 1981. Forestry research priorities of various user groups. Forest Prod. Jour. 31(10): 86-88.

Doig, Ivan. 1981. Early forestry research - a history of the Pacific Northwest Forest & Range Exp. Sta. 1925-1975. Portland, OR; USDA Forest Service Pacific Northwest For. & Range Exp. Sta. 35 p.

Ethington, R.L. 1979. Forecast of Forest Service needs for scientists trained in wood science and technology. Wood & Fiber 10(4): 275-279. Winter.

Fege, Anne S. 1979. Inventory of forest management research programs. Report prepared for the National Forest Products Association. Review Draft. 241 p.

Fleischer, H.O. 1974. Response of a public research laboratory to changing needs. For. Prod. Jour. 24(9):49-51. Sept.

Gray, Gary Craven. 1982. Radio for the fireline; a history of electronic communication in the Forest Service, 1905-1975. USDA Forest Service FS-369, Washington, DC 304 p.

Greber, Brian J.; White, David E. 1982. Technical change and productivity growth in the lumber and wood products industry. For. Sci. 28(1):135-147. March.

Gregory, A.S. 1974. Strategic planning of research and development at Weyerhaeuser. For. Prod. Jour. 24(9):37-43. Sept.

Guthrie, F.K. 1979. Weyerhaeuser research skill requirements and implied educational needs. Wood & Fiber 10(4):259-263. Winter.

Herrick, Owen W. 1982. Estimating benefits from whole-tree chipping as a logging innovation in northern U.S. forests. For. Prod. Jour. 32(11/12):57-60.

Holland, I. Irving. 1978. National program of research for forests and associated rangelands. Washington, DC; USDA and Nat'l. Assoc. of State Universities and Land Grant Colleges. 41 p.

Holling, C.S. et al. 1979. Quantitative evaluation of pest management options: the spruce budworm case study. p. 82-102, In, "Current topics in Forest Entomology, Selected Papers from the XVth International Congress of Entomology, Washington, DC, August 1976," W. E. Waters (ed.). USDA For. Serv. Gen. Tech. Rep. WO-8, Washington, DC. 174 p.

Huddy, Michael Dean. 1979. An evaluation of the McIntire Stennis cooperative forestry research program. PhD Thesis. East Lansing, MI; Michigan State Univ. 133 p.

Hyde, William F. (ed.). 1983. Economic evaluation of investments in forestry research. Durham, NC; Acorn Press. 106 p.

Jorgenson, Dale W.; Fraumeni, Barbara M. 1981. Relative prices and technical change. pp. 17-47, in "Modeling and Measuring Natural Resource Substitution," E. R. Berndt and B. C. Field (eds.). Cambridge, MA; MIT Press.

Kaufert, Frank H.; Cummings, William H. 1955. Forestry and related research in North America. Washington, DC; Society of Amer. Foresters. 280 p.

Ketcham, David E.; Shea, Keith R. 1982. Research decision-making in a concerted research program -- USDA's combined forest pest program. p. 308-310, In, "Increasing Forest Productivity," Proc. of the 1981 Convention of the Society of Amer. Foresters. Bethesda, MD.

Krugman, Stanley L.; Cowling, Ellis B. (Chrmn.) 1982. Our natural resources: basic research needs in forestry and renewable natural resources. Report of the National Task Force on Basic Research in Forestry and Renewable Natural Resources. Moscow, Idaho; Univ. of Idaho Forest, Wildlife and Range Exp. Sta. 35 p.

Lingwood, David A.; Morris, William C. 1976. Research into use: a study of the Forest Service research branch. Ann Arbor, MI; University of Michigan Institute for Social Research. Center for Research on Utilization of Scientific Knowledge. 290 p.

Lucas, Robert C. 1981. Redistributing wilderness use through information supplied to visitors. USDA Forest Serv. Res. Pap. INT-277. Ogden, Utah; Intermtn. Forest & Range Exp. Sta. 15 p.

Malac, Barry F. 1982. Decision-making in forestry research at Union Camp Corporation. p. 297-299, In, "Increasing Forest Productivity," Proc. of the 1981 Convention of the Society of Amer. Foresters. Bethesda, MD.

Moeller, George H.; Shafer, Elwood L. 1981. Important factors in the forestry innovation process. Jour. For. 79(1):30-32. Jan.

Muth, R.M.; Hendee, J.C. 1980. Technology transfer and human behavior. Jour. For. 78(3):141-144. March.

National Research Council. 1938. Forest research in the United States. Multilithed report. Prepared by the Lake States Forest Exp. Sta., USDA Forest Service, in coop. with the Committee on Forestry, Div. of Biology and Agriculture, National Research Council, Nov. 1, 1938. 138 p.

Nicholls, T.H.; Prey, A.J. 1982. Providing information: researchers to practitioners. p. 156-162, In, Proceedings, "North Central Urban and Suburban Trees: Pest problems, needs, prospects and solutions" East Lansing, Mich., April 18-20, 1982. East Lansing, MI; Michigan State Univ.

Nissan, Alfred H. 1981. Academic research - can it be "managed by objectives?" Tappi 64(10):39-42. Oct.

Platt, John R. 1964. Strong inference. Science 146(3642):347-353. Oct 16.

Porterfield, Richard L. 1980. Successful application of research findings in managing Champion timberlands. For. Prod. Jour. 30(10):16-20. Oct.

Pugh-Roberts Associates, Inc. 1981a. A dynamic model of technology transfer in a national forest. Milford, PA; Pinchot Institute for Conservation Studies, USDA Forest Service. 80 p.

Pugh-Roberts Associates, Inc. 1981b. Technology transfer within the national forest system. Conclusions and policy recommendations from a dynamic modeling system. Milford, PA; Pinchot Institute for Conservation Studies, USDA Forest Service. 41 p.

Pyne, Stephen J. 1981. Fire policy and fire research in the U.S. Forest Service. Jour. of Forest History 25(2):64-77. Apr.

Renewable Natural Resources Foundation. 1977. A review of forest and rangeland research policies in the United States from a symposium held at Airlie House, Warrenton, Virginia, May 30 - June 3, 1977. Washington, DC. 45 p.

Rensi, Giuseppe; Claxton, H. Dean. 1972. A data collection and processing procedure for evaluating a research program. USDA Forest Service Res. Pap. PSW-81, Berkeley, CA; Pacific Southwest Forest & Range Exp. Sta. 18 p.

Risbrudt, Christopher D.; Jakes, Pamela (eds.) 1985. Forestry research evaluation: Current progress, future directions. USDA Forest Service Gen. Tech. Report GTR-104. St. Paul, MN; North Central Forest Experiment Station. (In Press).

Risbrudt, Christopher D.; Kaiser, H. Fred. 1982. Economic analysis of the sawmill improvement program. Forest Prod. Jour. 32(8):25-28. Aug.

Roggenbuck, Joseph W.; Watson, Alan E. 1980. Technology transfer: lateral diffusion of innovation in forest recreation planning and management. Final Report prepared for the Southeastern Forest Exp. Sta. and the Pinchot Inst. for Conservation Studies, U.S. Forest Service. Blacksburg, VA; Virginia Polytechnic Institute and State Univ., Dept. of Forestry. 85 p.

Romesburg, H. Charles. 1981. Wildlife science: gaining reliable knowledge. Jour. Wildlife Mgm't. 45(2):293-313.

Rose, Dietmar W. 1983. Benefit-cost evaluation of the Douglas-fir Tussock moth research and development program. Jour. For. 81(4):228-231. April.

Rudolf, Paul O. 1985. History of the Lake States Forest Experiment Station. St. Paul, MN; USDA Forest Service, North Central Forest Exp. Sta. 290 p.

Ruttan, V. W.; Callahan, J. C. 1962. Resource inputs and output growth: comparisons between agriculture and forestry. For. Sci. 8(1):68-82. March

Schmaltz, Norman J. 1980. Forest researcher, Rafael Zon. Jour. of Forest History 24(1):25-38. Jan.; Part 2. J. For. Hist. 24(2):87-97. Apr.

Schreyer, Richard Michael. 1974. Individual orientation and research factors influencing scientific effectiveness in the U.S. Forest Service. PhD Thesis Ann Arbor, MI; University of Michigan. 314 p.

Shafer, Elwood L. 1982. 1980-1990 national program of research for forests and associated rangelands. USDA For. Serv. Gen. Tech. Rep. WO-32, Washington, DC. 61 p.

Smith, J. Harry G.; Lessard, Gilles. 1971. Forest resources research in Canada: current status, adequacy, desirable and future development. Background study for the Science Council of Canada, Special Study No. 14, May 1971. Ottawa; Information Canada. 204 p.

Stier, Jeffrey C. 1980. Estimating the production technology in the U.S. forest products industries. For. Sci. 26(3):471-482. Sept.

Strees, Anne. 1984. Productivity change in the U.S. forest industries. PhD Thesis. Minneapolis, MN; University of Minnesota. 242 p.

Taylor, J. MacGregor. 1973. Measurement of benefits arising from selected Western Forest Products Laboratory research projects. Internal Report VP-71, Forestry Service, Environment Canada. 34 p.

Tesch, Steven, D. 1981. The evolution of forest yield determination and site classification. Forest Ecology and Management 3(3):169-182. Apr.

Tombaugh, Larry W. 1982. Research decision-making in a state university. p. 300-302, In, "Increasing Forest Productivity," Proc. of the 1981 Convention of the Society of Amer. Foresters. Bethesda, MD.

Verrall, Arthur F. 1982. A history of forest pathology research in the South and Southeast. USDA Forest Service Gen. Tech. Rep. SO-36. New Orleans, LA; Southern Forest Exp. Sta. 193 p.

Whaley, Ross S.; Bell, Enoch F. 1982. Health of Forest Service economics research. Jour. For. 80(6):347-349, 364. June.

Self Evaluation of Research Programs

Arnett C. Mace, Jr.

Director
School of Forest Resources and Conservation
University of Florida
Gainesville, FL 32611

Self evaluation of research differs for various organizations and units due to variability of mission, administrative structure, funding, and particularly, the objective(s) of evaluation. Self evaluation assists planning, program support and enhancement, budget allocation, and rewards for scientists. Two frequently used criteria are: i) expansion of basic knowledge and ii) contribution to the solution of problems, which in the final analysis reflect productivity. The productivity of individual scientists or the unit is often contingent on constraints of other functional responsibilities, level of support, facilities and equipment and scientific environment, e.g. availability of other cooperating scientists and administrative support and encouragement. Productivity is measured by parameters such as acceptance by the scientific community, publications in prestigious journals, extramural support, interaction with clientele, and training of junior scientists. Quantitative and subjective methods used include self study documents, peer evaluation and user acceptance of results.

Introduction

Evaluation is the central process for motivation of scientific productivity, scientific relevance, acceptance of new knowledge by the scientific community, and useful application. Self evaluation, whether an formal or informal process, is one form of evaluating the success of individual scientists or programs. Although the self evaluation process has been rigorously defined for accreditation of academic programs, information for self evaluation of research is quite limited. This dichotomy, in part, is understandable because a significant part of self evaluation is a continuous informal process undertaken by the research scientist or administrator and is inherently an internal process. Also, research programs differ among organizations and units due to varying missions, funding sources, and constituencies. A continuous informal and unstructured critique of one's research or research program often determines program direction, motivation and ultimately contributions to science.

More formalized processes of self evaluation are initiated by scientists or administrators to meet specific needs of the individual or organization. These processes will vary among organizations, and often within organizations dependent upon the objective and planned use of the evaluation. The usefulness of any self evaluation is dependent on specific definition of objectives and intended use. Development of sound self evaluation systems responsive to today's complex world has and will continue to challenge both scientists and administrators. The absence of formal standards speaks to the difficulties encountered by organizations seeking to develop and utilize self evaluation systems for different objectives.

Keys to Self Evaluation

The central component of self evaluation is the individual, whether research scientist or administrator. Each must pose relevant objectives, accept and utilize constructive criticism, and maintain an environment that is conducive to cooperative and productive exchange.

The scientist must constantly evaluate research as to its relevance, contribution to knowledge, scientific soundness, "state of the art" technology, and relationship to organizational mission. The scientist must critically evaluate his ability to pose appropriate questions, devise and implement experimental procedures to answer questions and explain the experimental results and thus consequences.

Measures used by scientists to evaluate research include: publication of manuscripts (particularly in prestigious journals); extramural support of proposals; attraction of outstanding junior scientists; requests for seminars and invited papers; professional service; utilization of results by management organizations and industry; and rank and salary; honors and awards. Peer review remains the best and most used process for evaluating research. However, three aspects of peer review must be kept firmly in mind. First, reviewers must have considerable insight into the environment and constraints in which the scientist functions. Secondly, peer evaluation can be no more astute than the competence and integrity of those making the judgment. Finally internal peer reviews often contain more flaws than external reviews due to obvious reasons of competitiveness.

Most scientists conduct research that is consistent with the reward system of their organization. Therefore, the evaluation procedures of the organization will be proxies for scientist self-evaluation.

Research organizations normally define the general area of research for the individual scientist. Hence, the evaluation process becomes a summation of individual or team achievements. Consequently, it is incumbent upon the administrator to evaluate the relevance of the collective program in much the same manner as the individual scientist. If the achievements of the scientists are individually significant, the goals of both the scientists and the organization are fulfilled. Greater weight or attention is given to the program and its relevancy (particularly by sponsors and users of research), and integration of scientific effort at an organizational level.

Purposes of Self Evaluation

The primary purpose of self evaluation is to acquire additional information to make judicious program decisions. One critical role of the administrator is establishing thoroughly understood (usually written) objectives, methods, and planned use of the self evaluation. This approach greatly facilitates involvement and cooperation by all necessary parties.

Another purpose of self evaluation is to facilitate program planning and/or program enhancement. Acquisition and evaluation of information pertaining to fiscal resources, facilities, equipment, external perceptions, and needs, provides an excellent means for scientist and administrator to determine and define and agree on program priorities and opportunities. Utilization of external input often facilitates this process. Furthermore, if conducted with sufficient and candid input from affected parties, this process provides the organization an excellent opportunity to describe the current program, future program direction, rationale for future activities, required resources and expected benefits. Further, it is an excellent mechanism for defining and articulating current and future programs both internally and externally. This process is often used to develop justification and support for budget requests, identification of scientist priorities and recruitment of needed scientists and staff. The content of such a self evaluation normally contains goals of program, rationale or justification, short- and long-term objectives, personnel, facilities and equipment needs, and anticipated benefits.

In addition, self evaluation may serve as a basis for budget allocation, rewards for scientists, allocation of space, and capital improvements.

Contents of Self Evaluation

The content of a self evaluation may vary significantly dependent upon the objectives of the evaluation. The following are common items for consideration by the individual scientist:

a. Clearly defined functional responsibilities
b. Clearly defined short and long term objectives
c. Ability to conceive, implement and conclude research
d. Quality and contribution of research recognized by peers
e. Effective communication with peers and clientele
f. Publication of research in appropriate media
g. Effective supervision of junior scientists and staff
h. Success in obtaining appropriate extramural support
i. Utilization of research findings
j. Other professional contributions
k. Administrative support to effectively conduct research

Contents of an organization self evaluation may include:

a. Clearly defined program goals
b. Clearly defined short- and long-term objectives
c. Parent organization and supporting units
d. Description of unit missions, organizational structure and characteristics
e. Program leadership
f. Scientific personnel
g. Financial support and physical facilities
h. Program accomplishments
i. Future plans and recommended changes

These items are found in self evaluation documents of numerous organizations and can be modified for specific purposes, objectives and organizational goals. Much of the self evaluation document tends to be subjective including assessment of quantitative items. However, a comprehensive, candid self evaluation provides an excellent documentation of past performance and future goals.

Summary

Self evaluation provides an excellent mechanism to assess and stimulate progress, to evaluate and plan for new and enhanced programs, to allocate resources and to develop support for an individual scientist or organization. In both cases, this may be an informal or formal process that is continuous and largely subjective. The identification of goals and objectives, methods and planned use of the final product is essential to the committed involvement of all parties. The individual scientist or administrator must be prepared for constructive criticism and its utilization for improvement. The content may vary dependent upon goals, objectives and organizational mission and structure. The final product may be a valuable tool for articulating the scientist's or organization's programs both internally and externally.

THE SOUTHERN INDUSTRIAL FORESTRY RESEARCH COUNCIL (SIFRC)

D. M. Crutchfield

Technical Assistant to the Forest Research Director
Westvaco Corporation
P. O. Box 1950
Summerville, SC 29484

SIFRC was created to monitor publicly funded research, to identify research needs, and to be a focus for forest industry research concerns. Since its founding in 1978 it has published 3 reports. Highest ranked research needs are: Growth and Yield, Soil Classification-Site Quality; Maintaining Productivity; Management Systems; Vegetation Control; Fertilization/Nutrition. Other benefits to industry have accrued. SIFRC, representing industry, is a partner with the U.S. Forest Service and Forestry School Deans in guiding the wise use of federal funds for research.

For some years, industry foresters have been concerned about the level of public funding for forestry research, about information gaps, as well as problems related to prompt release and distribution of useful information.

The American Pulpwood Association formed the Southern Industrial Forestry Research Council to address these concerns by providing a mechanism to monitor publicly funded research and by providing a forum where forest research topics relevant to forest industry could be discussed.

To understand how SIFRC works and how it makes constructive comments on public research, it is first necessary to have an understanding of sources of research funding. This presentation will outline where the funds come from, where and by whom the work is actually done, and how the projects are selected and assigned.

Total national funding from all areas for forest research and development in 1978 was $331 million. Of this, 42% came from the federal government, 7% from the states, and 51% from industry. Forest industry's funding had 75-80% targeted at short-term high-return research in products and processing, and about 20% for forestry research in growing and harvesting.

Funds for federal forestry research represent less than one half of 1% of the entire federal research budget, and only 15% of the natural resources research budget.

Bringing this closer to home, when comparing agricultural to forestry research in the South, federal and state funding amounts to $4 to $6 per acre per year for each agricultural acre, and only 4 cents to 6 cents per acre per year for each forested acre. There is no agricultural product grown that yields 100 times the value of a forested acre each year. Clearly, a disproportionate value has been placed on forestry research, far less than its value to the nation. This is a major concern of SIFRC.

Who actually performs federally funded forestry research? The largest amount of forestry research in the South is conducted by the U.S. Department of Agriculture's Forest Service with 244 scientist years devoted to research. The Forest Service contracts out approximately 10% of its research effort to forestry schools.

A small number of scientist years is also contributed by other federal agencies such as the SCS, Tennessee Valley Authority, Park Service and Corps of Engineers.

Who else does forestry research? Forestry Schools and Ag Experiment Stations provide another important research effort in the South. There are 187 scientist years devoted to forestry research work by faculty members being done under grants from McIntyre-Stennis, the Forest Service, state legislatures, industry/university co-ops and contracted research for industry.

How are federally funded forestry research projects determined? Every ten years, the USDA's Forest Service updates its Renewable Resources Assessment. This update provides facts and figures on timber supply and demand, which are used in developing the Forest Service long range policies and programs under the Resources Planning Act. These programs are carried out at the regional level in the South by the Southern Forest Experiment Station at New Orleans, Louisiana and the Southeastern Forest Experiment Station in Asheville, North Carolina.

How are the university forestry research projects determined? Each southern state has at least one fully accredited state-supported forestry school with substantial research capability. These universities are financially assisted by their state legislatures and are committed to report their progress for review. In addition, overall program guidance and research evaluation is furnished by the Cooperative State Research Service review teams.

How are industry forestry research projects determined? Generally, industry focuses its research efforts in two areas. One is solving individual company problems conducted in-house with their own research staff, or under contract or graduate student support at forestry schools, or, to a lesser extent, the U.S. Forest Service.

The second way is by working through industry and public groups to solve what industry sees as regional forestry problems. The industry/university cooperatives presently operating in the South are excellent examples of this.

How are other forestry research projects determined? Many local and national landowner, environmental, recreational, conservation and preservation groups are concerned with the status of our forests and rangelands. These groups are vocal and active lobbyists for funds for programs that interest them.

Until the formation of SIFRC, forest industry's role in public funding of forestry research had been poorly defined.

The Southeastern and Southwestern Technical Divisions of the American Pulpwood Association felt it was vital that the limited research funds available be well spent and that the industry speak with one voice on research needs. The American Pulpwood Association's Southern Technical Divisions formed the Council in 1978. They set up operational guidelines, and charged the Council to review federally and other publicly funded research, to evaluate the coverage, to identify research gaps and to report its findings.

The Council was originally composed of twelve members, each from a different southern forest industry company. Since it was set up by the Southern American Pulpwood Association's Technical Divisions, it was instructed to report back to them.

Later, government and university researchers pointed out the Council did not represent the building products part of the forest industry, so the Council was expanded to include members of the Southern Forest Products Association as well as the American Pulpwood Association, but retains its twelve-member concept. The Council now receives direction from both associations and reports to them. Members are appointed to three-year terms to provide continuity. A rotation system is established so that four vacancies occur on the Council each year. A member may be reappointed when his three-year term expires.

Each Council member monitors at least one forestry school and several USFS research work units. He contacts the facility, reviews their research, discusses their plans, and evaluates their programs. He then prepares a summary of his findings which he presents to the Council.

These findings from all the forestry schools and the USFS research work units are reviewed, and form the basis of SIFRC's Research Report. The report is the official southern forest industry statement of forestry research concerns and is distributed throughout industry and to government and university researchers.

The Council prides itself on being a southern organization that is directly concerned with southern forestry research.

However, the council is pleased that the National Forest Products Association, a forest industry association headquartered in Washington, D.C., has formed a National Forest Resources Research Committee. The Council feels that many research funding problems need attention at the national level. SIFRC is now the independent southern arm of the National Forest Products Association. NFPA has formed similar groups in four other regions across the country.

What have been the benefits to industry since the formation of the Council in 1978?

- It has published Reports 1, 2 and 3 evaluating significant federally funded research. These reports identify research gaps needing more attention. Highest ranked gaps are:

  A. Growth and Yield

  B. Soil Classification, Site Quality, and subsequent Rotation Productivity

  C. Management Systems

  D. Vegetation Control

  E. Fertilization/Nutrition.

- It has sponsored state of the art Conferences, pointing out the need for additional research thrusts, for strengthening some programs and for leaving other programs at present levels.

- It has become a forum for new research ideas, particularly proposals for industry/university cooperatives.

- Its members have participated with the USDA Forest Service in their research planning groups.

- A member participates with forestry school deans in their discussion of research.

- It now confers with the National Forest Products Association Research Committee.

- It has provided a forum for all southern researchers to express their concerns.

- It has become a "model" for groups in other regions and even foreign countries.

In conclusion, the Southern Industrial Forestry Research Council has established itself as an effective monitor of publicly funded forestry research; and, as a forum for industry researchers. It is the council's goal to focus research efforts on subjects of industry need while serving as a third team member with the U.S. Forest Service and forestry school deans to direct the wisest use of the limited federal funds.

PUBLICLY SUPPORTED FORESTRY RESEARCH AN OVERVIEW OF REVIEW MECHANISMS

Robert E. Buckman
Deputy Chief for Research

H. Gus Wahlgren
Staff Assistant, Forest Products and Harvesting Research

USDA Forest Service
P. O. Box 2417
Washington, D.C. 20013

Review mechanisms are important to all publicly supported forestry research. Internal and external reviews such as advisory committees are common in both the USDA Forest Service and in forestry schools and departments. In addition important areas of research are continually identified for external review. User reviews foster a vigorous and healthy exchange among those who perform research and those who use it.

Research review is a process that is absolutely essential to publicly supported forestry research programs. External and internal reviews such as those conducted by advisory committees are common to all forestry research programs in both the USDA Forest Service and in the forestry schools and departments of major universities. Other reviews such as those by the National Forest Products Association's (NFPA) Committee on Research and Evaluation (CORE) annually help to focus the research program of the Forest Products Laboratory at Madison, Wisconsin. Another example of the review process is the one undertaken by the American Pulpwood Association's (API) Southern Industrial Forest Research Council (SIFRC) that Doug Crutchfield has just described for us.

Before exploring the review process with you, I would like to briefly mention research program development. There are procedures--very successful ones--used by both the USDA Forest Service and the 61 forestry schools and departments throughout the Nation receiving McIntire-Stennis funds to guide research priorities and to allocate scientific effort to forestry research problems. Research through these sources comprises about 90 percent of the publicly supported forestry research in this country.

Two major pieces of legislation, the Renewable Resources Planning Act (RPA) and Title XIV of the Farm Bill, strongly influence the nature of forestry science programs in the Department of Agriculture. Both these statutes require the Forest Service and the forestry schools to do long-range planning and coordination of their research programs.

The program planning approach promulgated by this legislation was developed through four regional conferences and a national conference attended by nearly a thousand participants, including representatives of environmental, grazing, water, and mining interests, as well as more traditional public and private land management organizations and the forest products industry.

As a result of these conferences, about 50 researchable problem statements or "thrusts" were identified. The final output of this effort was a publication outlining research needs and priorities both regionally and nationally. These priorities are updated periodically, as they were in 1982 with the document entitled "1980-1990 National Programs of Research for Forests and Associated Rangelands". This planning document addresses research conducted by the Forest Service and the 61 universities receiving McIntire-Stennis funds.

Review mechanisms are just as important as planning procedures to our research programs. A variety of such mechanisms exists, including periodic reviews of virtually all research conducted with Federal funds both in the Forest Service and at the universities. User groups are almost always involved in those reviews. Doug Crutchfield provided us with insight into one user review mechanism. We would like to describe four other examples of user reviews that we have found especially valuable, and then conclude with comments on the significance of such reviews to publicly supported forestry research programs. In the examples I'm about to describe, the Forest Service and the universities had identified, individually or jointly, the important areas of research for an external review.

First: A national review of forest inventory and analysis research in the USDA Forest Service was conducted in a workshop atmosphere during late 1983.

It was sponsored by the National Association of State Foresters and the National Forest Products Association and also included the American Pulpwood Association, in consultation with the Forest Service. This workshop was a systematic evaluation of the current Forest Service program in forest inventory and analysis research. The recommendations were very significant in suggesting new directions for forest inventory in the next decade, the most important of which were:

-Reduce inventory cycle to 5-10 years.

-Permit user access to detailed inventory data.

-Develop better methods for updating data between inventories.

-Develop consistent data between Experiment Stations.

-Increase efficiency with new technology.

Second: Growth and yield of southern forests were reviewed in 1984 by the Southern Industrial Forestry Research Council (SIFRC) and the National Association of Professional Forestry Schools and Colleges (NAPFSC). This review was under the auspices of the Southern Research Planning Group for Forestry, one of the regional groups we have already mentioned that have been involved in program development. This review summarized the status of southern growth and yield research and identified important research needs, some of which were:

-Develop improved growth and yield models to predict diameter distributions.

-Develop models to quantify growth and yield responses to intensive management practices.

-Develop standardized data collection through coordinated efforts of forest industry, the Forest Service, and universities.

Third: Another review was held in California last year involving the new and very promising field of biotechnology research. Key Forest Service scientists were joined by others from the universities, the biotech industry and forest industries to establish goals and priorities for forest biotechnology research. Three research directions were suggested:

-Concentrate research on relatively few important tree species.

-Direct research to the development of methodology for the direct transfer of genetic information.

-Emphasize research in cell fusion and stress selection and recombinant DNA technology.

Fourth: A final example is drawn from the pulp and paper field. Following several years of basic research on the engineering properties of paper, Forest Service scientists discovered that fiber bonding and strength could be improved if the fiber mat was pressed and restrained simultaneously rather than separately as in conventional papermaking. This innovation, called press-drying, permits utilization of the short fiber hardwood species, which are more abundant and less costly than softwoods. The advent of this technique led to the establishment of a press-drying subcommittee by the American Pulpwood Institute (API) and the Technical Association of the Pulp and Paper Industry (TAPPI). Through the subcommittee's encouragement, a continuous pilot press dryer was developed that demonstrated the press-drying system as a viable method for producing linerboard from hardwood trees.

Forest Service scientists, with the subcommittee's advice and counsel, further developed and built a high-speed paper machine simulator to test the press-drying system with the paper web traveling at speeds of 1,800 feet per minute. We are optimistic that this user involvement will hasten implementation of press-drying into practical use by the pulp and paper industry. The same user review procedure is anticipated for another promising new product concept called "Spaceboard". This development incorporates the basics of press drying into the formation of a new structural "sandwich" that provides equal strength in both principal directions of the structure. While the original concept was aimed at an improved corrugated fiberboard container, it is apparent that this high tech Spaceboard has even broader impact and potential for a wide variety of structural sandwich designs.

These examples illustrate but a few of the review mechanisms that are used in publicly supported forestry research activities. One might well ask what do we gain from such reviews? In answer to that question, let me say that each of the user groups mentioned envision benefits they hope to derive from publicly supported research, including the opportunity to influence the problems selected for research priority. We who conduct the research have some objectives also, and feel it's important to point them out. We see reviews as a means to:

-Foster a vigorous and healthy exchange among those who perform research and those who use it.

-Learn about industry and other research programs to avoid unnecessary duplication of effort but without compromising industry's proprietary interest.

-Speed up application of new or existing technology that has been generated by publicly supported research.

-Seek assistance where appropriate in cooperative research efforts, including joint funding, services in kind, and provisions for experimental areas and facilities.

-Gain support and understanding for publicly supported programs. This is support that builds confidence in the quality and the timeliness of research programs.

To conclude, let me restate that user reviews are essential in forestry research programs. While we are reasonably pleased with present review mechanisms, we continually seek better ways to review publicly supported research.

CO-AUTHORSHIP PATTERNS OF USDA FOREST SERVICE RESEARCH SCIENTISTS AT TWO REGIONAL EXPERIMENT STATIONS, 1981-1984

Pamela J. Jakes

Research Forester, North Central Forest Experiment Station, USDA Forest Service, 1992 Folwell Ave., St Paul, MN 55108

Anne S. Fege

Staff Assistant, Timber Management Research, USDA Forest Service, Washington, D.C.

Introduction

In many public research agencies, publication counts are a primary measure of productivity for scientists and research teams. There are numerous problems with this measure of productivity, but one problem receiving increasing attention concerns changes in the social organization of research. Zuckerman[1/] has described in detail how the visibility of individual performance has been reduced as the social organization of science has become more complex. She states that this change in social organization is reflected in the rapid increase in multi-authored papers. Zuckerman argues that the reward system in science, particularly when it depends on publication counts as a basis for rewards, is better geared to allocate credit to individuals rather than groups of scientists.

Forest Service research managers have recently become interested in the topic of co-authorship of publications. This interest has been spurred by two developments. First is a movement within the Forest Service to organize research efforts into large programs and multi-disciplinary research work units, rather than the more traditional, smaller, research units. In programs, research is conducted by multi-disciplinary teams rather than more single-discipline individual efforts common in research work units. Examples of Forest Service research programs include the Spruce Budworm Program at the Northeastern Forest Experiment Station and the Intensive Culture Research and Development Program at the North Central Forest Experiment Station.

A second reason for increased interest in co-authorship centers on the use of extramural research agreements to conduct Forest Service research. Cooperative agreements and grants are examples of extramural research vehicles. They are contracts between the Forest Service and an individual (usually a university professor) to conduct research to answer specific questions. In a cooperative agreement, Forest Service researchers are actively involved in the research effort, while in a grant, the non-Forest Service researcher is responsible for satisfying the terms of the contract. There is concern within the Forest Service that the Forest Service does not receive adequate credit, in the form of co-authorship of publications, for extramural research.

Objectives

Our objectives were to analyze publications resulting from research sponsored by the North Central and Northeastern Forest Experiment Stations, USDA Forest Service, for Fiscal Years (FY) 1980 (October 1, 1979 through September 30, 1980) through FY 1984 (October 1, 1983 through September 30, 1984) in order to:

1. identify changes in publication patterns, particularly in regard to co-authorship of publications, and

2. establish a baseline against which future changes in publication patterns could be measured.

Methods

We collected data from research attainment reports published by the North Central and Northeastern Forest Experiment Stations for FY 1980 through FY 1984. Research attainment reports are published annually by all Forest Service regional experiment stations and the Forest Products Laboratory, and contain budget and staffing information for each research work unit or project in the station. They also include a list of citations for each manuscript published by unit researchers or extramural researchers in that Fiscal Year, and a narrative on research accomplishments.

We created two data bases: (1) a publication data base, and (2) an author data base. The publication data base contains one entry for each publication. The following information was included for each entry:

--Station	--Year
--Work unit claiming publication	--Classification code
--Type of publication	--Number of authors
	--Affiliation of authors

Although most of the entries are self-explanatory, some require additional explanation:

[1/] Zuckerman, Harriet A. Patterns of name ordering among authors of scientific papers: a study of social symbolism and its ambiguity. Amer. J. Soc. 74: 276-291, 1968.

Classification code: assigned in the attainment report, the code indicates the article's research focus. The attainment report recognizes 33 research disciplines which are grouped into 6 broad research catagories: (1) environment, (2) insects and disease, (3) fire and atmospheric sciences, (4) timber management, (5) economics and evaluation, and (5) products and engineering.

Type of publication: we were interested in seven categories: (1) Forest Service research paper, (2) other Forest Service publication, (3) forestry scientific journal, (4) other scientific journal, (5) other forestry journal, (6) proceedings, and (7) other publication outlet.

Affiliation of authors: affiliations were determined by whether the author was employed: (1) within the work unit claiming the publication, (2) by the station claiming the publication, but outside the work unit, (3) by the Forest Service, but not at the station claiming the publication, or (4) outside the Forest Service.

The author data base had one entry for each author. For each author we noted the total number of publications for the 5-year period. For that total we entered the number of publications by:

--Year
--Type of publication
--Author affiliation
--Co-author affiliation code
--Author position in the list of authors
--Number of authors per publication

Findings

Over the five-year study period, the two stations produced 2,896 articles or publications. The annual number of publications produced increased more than 18 percent from FY 1980 to FY 1984:

Year	Station publications: North Central	Northeastern
1980	219	315
1981	281	289
1982	228	295
1983	289	348
1984	280	352
Total	1,297	1,599

The outlets used to publish Forest Service research findings fluctuated greatly over the five years. While proceedings generally accounted for the largest percentage of publications produced in any year, the importance of Forest Service research papers, forestry scientific journals, and non-forestry scientific journals varied significantly (Table 1).

Nearly 60 percent of the publications produced over the 5-year period were multi-author publications, and the number of multi-author publications has been increasing (Table 2). One indication of the increase in multi-author papers has been the increase in the average number of authors per paper. The average number of authors per publication has increased from 1.8 in FY 1980 to 2.1 in FY 1984.

The outlets used to publish research findings varied slightly by the number of authors per publication (Table 3). Multi-author publications appeared more often in non-forestry scientific journals and less often in proceedings than single author publications.

Of the 1,750 multi-authored publications produced over the five-year period, more than 86 percent had at least one author from within the work unit, the remainder were authored entirely by individuals from outside the unit. Unit authors were more likely to coauthor a paper with someone outside the Forest Service than someone within their own unit:

Co-author affiliation	Percent of publications with at least one unit author
Within the unit	34.2
Within the Station, not in the unit	9.2
Within the Forest Service, not in the Station	8.1
Outside the Forest Service	48.5
Number of multi-author publications with at least one unit author	1,512

For co-authors within the Station, the majority were at the same location as the unit author. Most of the collaborations between Forest Service employees were between unit researchers and someone in State and Private Forestry rather than between researchers. Co-author affiliation has fluctuated slightly over the five-year study period with no discernable trends (Table 4).

Discussion

Our first objective in carrying out this study was to identify changes in publication patterns, particularly in regard to co-authorship of publications. We have shown that there has been a significant increase in the number of multi-author publications. We don't know, however, if this reflects a change in the social organization of research, as Zuckerman suggests, or is due to efforts on the part of the Forest Service to increase credit for cooperative research efforts. There has been an increase in the number of publications co-authored by unit and non-Forest Service researchers, however the percentage of multi-author publications co-authored by unit and non-Forest Service researchers has not been increasing. A similiar situation occurs in the case of unit researchers co-authoring with other researchers inside the Station--the numbers and percentages have fluctuated, with no trends evident.

The data bases created for the two stations provide us with baseline data against which future changes in publication patterns can be measured. The publication data base helps us identify changes in publication characteristics such as the number of publications produced, outlets used, number of authors per publication, and affiliations of authors and co-authors. Our next step is to create a third data base, a unit data base, containing much of the same information as the author data base, only for research work units. The unit data base would also include annual budget and staffing information as measures of resources available to the unit.

While the data bases provide a wealth of information on publication patterns, the importance of the information in research management must be determined by those involved in the activity. In the next phase of the study, we plan to interview research managers to determine whether the information is useful, in what format is it most useful, when is it of most use (within what time frame), and what additional information is desirable.

Table 1.--Publications produced annually and distribution among publication outlets, North Central and Northeastern Forest Experiment Stations, FY 1980-1984

Outlet	Year 1980	1981	1982	1983	1984	All years
	percent					
Forest Service research paper	7.5	10.3	5.7	7.6	5.0	7.1
Forest Service other	14.1	17.1	6.3	18.5	16.5	14.8
Forestry scientific journal	11.1	4.4	5.1	4.8	6.3	6.3
Other forestry journal	12.7	9.4	14.2	13.5	12.7	12.5
Non-forestry scientific journal	9.6	14.7	18.9	18.6	17.0	15.9
Proceedings	29.6	19.5	28.4	24.3	28.3	26.0
Other	15.4	24.6	22.0	12.8	14.2	17.5
Number of publications	534	570	523	637	632	2,896

Table 2.--Publications produced annually and distribution by number of authors per publication, North Central and Northeastern Forest Experiment Stations, FY 1980-1984

Year	Number of publications	Number of authors: One	Two	More than three
		- - - - - -percent- - - - - -		
1980	534	48.5	34.5	17.0
1981	570	44.0	33.9	22.1
1982	523	35.4	39.2	25.4
1983	637	35.5	33.6	30.9
1984	632	35.6	36.1	28.3
All years	2,896	39.4	35.0	25.6

Table 3.--Total publications produced by number of authors per publication and distribution among publication outlets, FY 1980-1984

Outlet	Number of authors: One	Two	More than three	All publications
	- - - - - - - -percent- - - - - - - - -			
Forest Service research paper	6.5	7.7	7.0	7.0
Forest Service other	15.1	16.6	11.2	14.7
Forestry scientific journal	4.8	6.6	7.7	6.2
Other forestry journal	11.8	13.2	11.8	12.3
Non-forestry scientific journal	10.6	17.3	21.5	15.7
Proceedings	34.0	18.8	22.0	25.6
Other	17.2	19.8	18.8	18.5
Number of publications	1146	1024	726	2896

Table 4.--Multi-author publications with at least one unit author produced annually, and distribution by co-author affiliation, North Central and Northeastern Forest Experiment Stations, FY 1980-1984

Year	Publications produced	Co-author affiliation: Within unit	Within Station	Within Forest Service	Outside Forest Service
		- - - - - - - - - - - - - -percent - - - - - - - - - - - - - -			
1980	235	37.4	8.1	9.4	45.1
1981	293	34.1	9.6	9.9	46.4
1982	249	37.3	6.8	5.6	50.2
1983	383	36.3	12.3	7.6	43.9
1984	352	27.6	8.0	8.2	56.3
All years	1512	34.2	9.2	8.1	48.5

EVALUATING SCIENTISTS IN CANADA

J.H. Cayford, Regional Director and
C.R. Sullivan, Program Director, Research and Technical Services

Government of Canada
Canadian Forestry Service
Great Lakes Forest Research Centre
Sault Ste. Marie, Ont.
P6A 5M7

The classification system for research scientists employed by the Government of Canada is reviewed and a description for each of the four levels of research scientist is provided. Procedures for evaluating research scientists are discussed under three headings: recruiting, annual appraisal and promotion. The classification standard, the quota allocation, research productivity and the various research scientist committees are discussed and suggestions for improvement are noted.

Introduction

Classification within the federal government is a responsibility of the Treasury Board. Approximately 65 different classifications are recognized. These range from senior executive to research scientist, medical doctor, biologist, technician, secretary and clerk. The classification plan for nearly all employees is position oriented, with the assigned level based on a description of duties rated against benchmark positions. In the case of research scientists, the classification plan is incumbent oriented with individuals rated against a selection standard. The classification process for research scientists is unique within the Public Service of Canada.

Description

The criteria for each level within the Research Scientist Group are based on individual achievement and productivity. Appointment to the Research Scientist Group and promotion within the group are dependent upon the individual's cumulative productivity and demonstrated capacity to function at a higher level.

The basic requirement for entry to the Research Scientist Group is that the individual be involved in planning, conducting, or evaluating research and development in the natural sciences. Positions require the application of comprehensive, in-depth knowledge of concepts, theories and research methods appropriate to a specific scientific field. Positions requiring expertise in managing, coordinating major federal government research programs, or providing advice on these programs are specifically excluded from the Research Scientist Group.

There are four levels within the Research Scientist Group. Level 1 is the normal entry level from university and the appropriate level for the junior scientist who is in the development stage of his career. The scientist must have obtained a doctoral degree or have completed and reported on personal research equivalent to that required for a doctoral degree. At this level he is expected to publish either as an author or as a coauthor, and undertakes projects or studies following established guidelines and under general supervision. Level 2 is the level for the mature research scientist of normal cumulative achievements. The scientist must have completed a substantial number of research or development projects and authored or coauthored a substantial number of scientific papers. Typically, he has established a reputation in a specific field and has the ability to represent his agency on scientific issues related to his area of specialization. He has substantial freedom in identifying, defining and selecting specific problems for study. Level 3 is the level for the mature scientist with cumulative achievement distinctly above average. The scientist at this level must have made contributions to research or development definitely superior in quality or significance to those normally expected from a mature research scientist in the field. Such achievement may be evidenced by authorship of an extensive number of research publications of superior quality or significance, by authorship of authoritative research reviews, by an extensive record of successful technology transfer to industry or other areas of application of scientific knowledge, or by an extensive record of leadership in group projects or programs. Typically, the scientist has attained national or international recognition as an authority in a substantial field of research and development, holds office in professional societies, represents his agency on major scientific issues and carries out assignments in terms of general objectives. Level 4 is the "select" level for research scientists of exceptional attainment. The scientist who is promoted to this level must have a record of exceptional contributions and must have authored an exceptional number of research publications of excellent scientific quality and significance, or have produced highly important patents or designs, or have an exceptional record of successful technology transfer. Typically, he has attained international recognition in a broad field, has served in international delegations and on national commissions and may have received internationally recognized awards of merit. His scientific objectives are defined in general terms, and his assignments demand a high level of scientific originality, coordination and judgment.

Superimposed on the foregoing standards is a Treasury Board quota system. Not more than 5% of research scientists may be classified at level 4, and not more than another 25% may be classified at level 3. Hence, promotion may be denied to an individual meeting the criteria if the quota has been met. Most scientists have a reasonable expectation of attaining level 2; competition increases sharply, however, for levels 3 and 4.

Recruiting of Research Scientists

The recruiting process is designed to attract the applications of high-quality candidates for positions within the Public Service of the Government of Canada. The initial step involves the preparation of a brief, but succinct, job description or summary of duties, together with a statement of qualifications for the position. On the basis of this information, prospective candidates are sought through advertising in national newspapers, in journals such as Science or Nature or in the Forestry Chronicle published by the Canadian Institute of Forestry, and through direct contact with appropriate universities. However, to meet the requirements of the Public Service Commission of Canada (the body that formulates and implements policy on staffing in the Public Service), candidates who are already employees of the Government of Canada must be reviewed first. Prospective candidates are assessed, in relation to the statement of duties and the job description, by means of a personal interview. Often the presentation of a comprehensive seminar is required. Once the successful candidate has been identified, the level and grade are determined in relation to the criteria previously described. Depending on the field of work and the availability of scientists, recruiting may take from 3 to 12 months.

Annual Appraisal

Each research scientist is appraised annually by his supervisor to determine whether or not his performance has been satisfactory. Salary within a research scientist level is determined solely by a fixed schedule. There are between seven and nine pay scales within each level; progression is automatic provided that the performance of the scientist has been judged satisfactory. (It is extremely rare for a scientist to be judged unsatisfactory.)

Once the scientist has reached the maximum salary for his level, he receives no further increment until and unless he is promoted. Salary scales are usually adjusted annually, however to compensate for inflation. When the Research Scientist Group was introduced in the mid-1960s, the annual appraisal incorporated a merit increase. There were no fixed steps within the salary scale, only a minimum and maximum, and each scientist was appraised individually, usually by means of a point rating system for each productivity item, and annual increases were based on a dollar value per point. This system was abolished, at the request of the research scientists, during collective bargaining.

The annual appraisal describes for the scientist his productivity in relation to goals established the previous year, it indicates how his performance and capability are viewed by Management in relation to position level and discipline area, it assesses his ability to contribute to the overall operation and goals of the program outside his direct area of research interest, it provides goals for the current year against which his performance will be assessed, and it encourages him to maintain standard documentation. It also permits comment on human resource planning, including educational leave, sabbaticals, career advancement and aspirations, and proposals for promotion.

Promotion of Research Scientists

Promotion of research scientists is obviously the major issue in the review process. All promotions of research scientists are effective April 1 of each year to coincide with the date specified for granting incremental increases. Recommendations for the promotion of an employee are not contingent upon the salary position in the pay range or the amount of time spent in the level, but strictly on cumulative productivity.

In the annual appraisal, the supervisor must consider whether or not to initiate a recommendation for promotion. If he does, he must prepare the documentation required for this action. Among the information required is basic identification of the candidate (i.e., name, position level, present level, recommended level, current salary, academic qualifications, etc.), career highlights (normally prepared by the immediate supervisor), a section on cumulative productivity, with schedules (the key section, written by the immediate supervisor and/or the scientist), statements on creativity, recognition, influence and nature of assignments, and employment record.

To complete the promotion process a number of Research Scientist Committees have been established. These include a Service Review Committee (i.e., Canadian Forestry Service), a Departmental Review Committee (i.e., Department of Agriculture) and an Interdepartmental Review Committee. Each committee has been delegated specific authorities: for example, promotions from level 1 to level 2 are delegated to the Service Committee, promotions from level 2 to level 3 are the responsibility of the Departmental Committee and promotions from level 3 to level 4 are the responsibility of the Interdepartmental Committee. There may be minor variations depending upon the department concerned and whether or not the scientist is at the maximum salary for his level. The committees are composed primarily of managers, but each has a level 4 research scientist as a member. The committees are interlocking, in that the chairman of a lower committee is a member of a higher committee. It is evident that, the more senior the committee, the further its members are removed from the particular scientist's research field. For example, most members of the Canadian Forestry Service Committee have a general understanding of forest research, but only one member of the Department of Agriculture Committee would have this understanding. On the Interdepartmental Committee only infrequently would there be a member knowledgeable about forest research.

Classification Standard

One of the concerns expressed most frequently about the Research Scientist Group has to do with the small number of levels within the group. As a result of the quota system, at least 70% of the scientists must be in levels 1 and 2. As level 1 is basically a recruiting level, it follows that the vast majority of scientists are in level 2. This level is characterized by a very broad salary scale, with nine steps, and a salary level ranging from $35,000 to $52,500 (Canadian dollars). Hence, a research scientist, once classified at this level, can rise to a very high salary level, merely by maintaining a minimum level of performance. Because of the broad salary range, the annual appraisal may have little significance once a research scientist reaches level 2. Although a salary increment can be disallowed, it is rather difficult to base an unsatisfactory level of performance on only one year's work. Consequently, increments are normally disallowed only in the case of the poorest scientists, i.e., those who have performed unacceptably over a period of years. The few levels identified within the series, coupled with an imposed quota system, may diminish the incentive of research scientists, particularly in level 3. Most of these scientists are aware that they will not progress to level 4. To some extent, the same situation prevails for scientists at level 2. Because there are so few levels, many scientists are evaluated properly in terms of their career productivity only once, namely, when they are being considered for promotion from level 1 to level 2.

Quotas

As indicated earlier, the Treasury Board has developed a rigid quota system for research scientists. In view of the fact that classification of this group is incumbent oriented, the imposition of quotas is seen by most scientists and research managers as a retrogressive step. Treasury Board officials no doubt see quotas as a means of controlling or managing expenditures for salaries of research scientists. It is somewhat difficult to rationalize the use of a fixed quota system, however, as the maturity and productivity of a population affect the percentage of scientists at each level. Indeed, if a quota is applied, it should not employ a consistent formula but should depend on certain criteria developed in relation to the research scientist population. At present, quotas are applied to each government department, regardless of the nature and size of the research scientist population at any given time. At its worst the quota system may well result in the departure of some of our best scientists from government service.

Research Productivity

In theory, all forms of productivity are to be considered in evaluating the performance of research scientists. However, there is a widely held and probably correct perception among both researchers and managers that publication is most important. This emphasis on publication results in unnecessarily large numbers of publications and a cluttering of the literature. There is also concern that some scientists work in areas in which it is relatively easy to acquire publishable data, while others work in areas in which it is difficult. As an example, laboratory experiments on the nutritional requirements of organisms usually provide data for numerous publications, while long-term field experiments on the susceptibility of trees to pests do not. Scientists working in the latter field may therefore be at a disadvantage, when it comes to producing publications, even though the field experiments may be of greater value to forest managers.

The emphasis on publication is probably most pronounced in the level 4 category. Researchers involved in basic research programs leading to publications in top-quality scientific journals are favored. There is also a perception, with which we would agree, that inadequate weight is given to technology transfer, to related but nonresearch assignments, to management of contract research, and to project leadership and supervisory roles. Indeed, it is difficult to prove the impact of a specific type of technology as it may not be used until many years after the research is completed. Special assignments that may have high priority within the research organization may be undertaken by the scientist at his expense in terms of promotion. The current system probably functions best in agencies with programs concentrating on basic research and poorest in mission-oriented agencies.

Research Scientist Committees

Research Scientist Committees as they are now constituted are essentially managerial committees, although one level 4 research scientist sits on each committee, as was noted earlier. Managers are able, to some extent, to assess the quality and importance of work, particularly in relation to organizational objectives, but their ability to do so varies. The review process is not a peer review process and at least some scientists believe that their productivity can be assessed realistically only by their peers. As there is no continuity in committee membership, standards of operating, criteria to be used in assessing each scientist, and value judgments may vary between committees.

Concluding Remarks

We believe that the appraisal and promotion systems work reasonably well for good, productive scientists but marginally productive scientists are usually capable of avoiding the negative impacts of these systems while earning a very comfortable salary. The current review process, incorporating elements of both objectivity and subjectivity, is useful for evaluating the performance of a group of people with diverse talents, objectives and responsibilities. With a variety of outputs ranging from publications on fundamental research in scientific journals to the direct implementation of applied results, it is obviously difficult to develop a completely equitable evaluation process that fairly recognizes the value of contributions by individual scientists. One important factor in achieving an equitable review is to ensure that first-line supervisors develop a close rapport with, and understanding of, individual scientists, their work and their aspirations. It should also be noted that the evaluation of a research scientist through an annual appraisal process cannot be divorced from the topic of program evaluation. While the latter focuses on the effectiveness and efficiency of programs, the link is obvious when the programs being evaluated are undertaken by scientists.

As noted, the current review system does not effectively recognize such activities as technology transfer, liaison, special assignments and project leadership. What is good for the organization may not necessarily be good for the scientist. The most critical problem may well relate to project leadership. Many scientists are reluctant to accept this role, because it interferes with publication productivity. However, the role of project leader provides indispensable management training and is vitally important to the organization. Perhaps what is needed is a subgroup within the classification system that recognizes a dual research-management responsibility.

The process could perhaps be improved through a change in the classification standards that would establish a greater number of research scientist levels, with fewer salary increments. The subdivision of level 2 could be a starting point. Additional levels would permit greater flexibility in recognizing special contributions made by scientists as, for example, in project leadership. Additional levels would also provide more flexibility in classifying scientists. There is also some support for conducting appraisals less frequently. It is difficult to evaluate the performance and productivity of a scientist on an annual basis, especially in a field such as forestry in which much of the research is of a long-term nature. A thorough evaluation might be conducted every 3 to 5 years, and a very simple review conducted annually. There is also limited support, particularly from management, for a return to a salary system based entirely on merit and performance. Such a system could be practicable, however, only if it were implemented on a multi-year basis. However, it seems unlikely that the majority of our scientists would accept a return to a merit system, other than for promotion.

EVALUATION OF THE CANADA-UNITED STATES SPRUCE BUDWORMS PROGRAM--ORGANIZATIONAL AND ADMINISTRATIVE EFFECTIVENESS

Jack E. Coster and Denis Lachance

Director, Division of Forestry
West Virginia University
Morgantown, WV 26505-6125

Head, Forest Insect and Disease Survey
Laurentian Forest Research Centre
Canadian Forestry Service
St. Foy, Quebec G1V 4C7

A joint Canada-U.S. program (CANUSA) was initiated in 1977 to address the spruce budworm problem. An evaluation of the administrative and organizational aspects of the program in 1983 recommended several changes in funding, communications, research planning, and research implementation. The evaluation utilized extensive interviews of researchers, administrators, and users.

The spruce budworm and its close relative, the western spruce budworm, have long been recognized as among the most harmful forest pests in North America. Recent outbreaks in both the eastern and western parts of the continent have infested significant portions of the spruce-fir forests. The problem has persisted for many years despite repeated, extensive aerial spraying programs.

An international program of cooperation in spruce budworm research was initiated in 1977 by the Department of Environment of Canada and the United States Department of Agriculture (USDA). CANUSA is the acronym for the joint Canada-United States Spruce Budworms Program.

A Memorandum of Understanding between these two lead agencies recognized that the pests were socially and economically important to both countries, that the extent of the damage to forests was unacceptable, that a wider base of knowledge was necessary to deal with the problem, and that coordination and cooperation in research and development between both countries would be more effective. The primary objective of the program was to "design and evaluate management strategies for control of the spruce budworms and/or management of budworm-susceptible forests which will assist forest managers to attain management objectives in an economically and environmentally acceptable manner." The Agreement encouraged joint publications of results, exchange of scientists, exchange of methodology and data, and exchanges of biological materials.

Organizational Features

CANUSA's management structure featured joint U.S. and Canadian leadership at several levels. In the U.S., it was headed by the Chief of the USDA Forest Service and in Canada by the Assistant Deputy Minister, Forestry, Environment Canada. A policy committee, The Joint Policy and Program Council (JPPC), provided direct policy guidelines to the two agency heads. The JPPC consisted of four members from each country and had the general purpose of assuring maximum cooperation and coordination. For the operational planning aspects of the program, a Joint Planning Unit (JPU), also with four members from each country, answered to the JPPC. The primary purpose of the JPU was to evaluate budgets, allocate funds to program areas, and review overall progress. The Agreement also provided for a 20-person Advisory Committee that was to serve as a "focal point whereby the concerns of resource managers and other interested groups can be brought to the attention of the Joint Policy and Program Council." The Advisory Committee was never formed. A Canada Program Leader and a U.S. Program Leader were to have day-to-day administrative and technical responsibility for all budworms research in the respective countries.

The Memorandum of Understanding provided for establishment of all the organizational features down through the Program Leaders. Organization below that level was to fit the needs of the individual countries. In the U.S. there were eastern and western program managers at the USDA Forest Service's Northeastern and Pacific Northwest Experiment Stations. In Canada, the program was administered largely by the heads of the six Forestry Research Centres and two Forestry Institutes of the Canadian Forestry Service.

Organization of the program drew on recent previous experience with accelerated forest insect research and applications programs conducted by the USDA. These programs had responded to needs for more effective technology in dealing with the gypsy moth, Douglas-fir tussock moth, and the southern pine beetle (Ketchum and Shea 1977). While several State and federal agencies were involved cooperatively in the earlier programs, CANUSA was the first formal joint international forestry-related program.

Purposes of Evaluation

In late 1982, about 18 months before the program was to end, the JPU and JPPC recommended to the lead agency administrators that a critique of the program be undertaken. The thrust of the critique was to evaluate the effectiveness of the CANUSA organization and administration in accomplishing the objectives of the Memorandum of Understanding. Because many research results would not be implemented for some time after the program's completion, and because many of these results would probably undergo more refinements in the development and applications stages, evaluation of their effectiveness was not to be included in this review.

Future joint programs were also to benefit from the evaluation. The U.S. and Canada have other forestry-related areas that could possibly be the focus of formal research cooperation, e.g. mountain pine beetle control, forest fire research, and silvicultural studies. But before another joint venture was undertaken, the federal agency administrators wanted to know how well the CANUSA organization worked and how it might be improved, if possible.

Evaluation Procedures

Two Canadians and two Americans were appointed to the review team. None of the four had been involved directly in research or administration of CANUSA. The CANUSA Review Team members were Jack Coster, West Virginia University, Kenneth Knauer, USDA Forest Service, Washington, DC, Denis Lachance, Canadian Forestry Service, Quebec City, Quebec, and Kenneth Runyon, Canadian Forestry Service, Fredericton, New Brunswick. Two of the team were forest entomologists, one was a forest pathologist, and the fourth was a forest economist.

The team first met in Washington in November 1982 to begin development of a study plan to be submitted for review and approval to the JPU. Several constraints affected the plan. First was time--the evaluation was to be completed and the final report submitted within three months of approval of the study plan. Second was financial support. Limited funds were available. The team member's institutions gave the needed staff support and only limited funds were available for other costs. And the last important constraint was that the administrators did not want the study to disrupt, unduly, the on-going program and projects. To accomplish the objectives within the temporal and fiscal restraints, therefore, the evaluation used interviews of key administrators, researchers, and users. It was clear that the schedule and budget for the evaluation would not permit using a statistically designed survey. Perceptions obtained from interviews were the basis for an overall assessment of the accelerated approach to forest pest research programs (Allen et al. 1982).

To maximize the information collected within the time available, the interviews were conducted by two-man teams of one Canadian and one American each. Some interviews were conducted individually, others in small groups. The interviews focused on the planning, budgeting, implementation, monitoring, and information exchange functions of the program. The focus on these five management functions was intended to provide a more-or-less consistent framework for interviews. Topics covered in each of the management areas included:

Planning

Goal, objective, and priority setting processes; administrative and technical support provisions; activity and task scheduling; technology transfer plan development; authority and accountability constraints; decision making processes; international and regional cooperation mechanisms; national policy constraints; and roles of the JPU and the JPPC.

Budgeting

National policy differences and effects; development process and constraints; control and criteria for allocation process; visibility of the process; funding adequacy and stability; CANUSA-imposed administrative obstacles; and the role of the JPU and the JPPC.

Implementation

Advisory group formulation and function; working group formulation and function; project management process; adherence to program plans and priorities; role of the JPU and the JPPC, national policy differences and effects; program staffing adequacy.

Monitoring

Role of the JPU and the JPPC in oversight; feedback mechanisms to the JPU and the JPPC; project oversight process; national policy differences and effects; visibility of the monitoring process; fiscal review process; reaction to contractor non-performance; role compatibility between organizational levels within CANUSA; program staffing adequacy.

Information Exchange

Program policy and plan documentation; program status reporting; process for external response to program materials; provision for post-program international cooperation; stimulation of international exchange of information; program staffing adequacy.

The interviewers thoroughly discussed the management functions before the interviews to increase common understanding and a standard list of questions was used by both teams. Individuals were also given the opportunity to provide general reactions to CANUSA organization and administration.

Interviews were conducted at Canadian Forestry Service Headquarters in Ottawa and Provincial Forestry Headquarters in Quebec City, Quebec and Fredericton, New Brunswick. Canadian investigators, forest managers, and pest managers were interviewed during site visits to Canadian Forestry Research Centres at St. John's, Newfoundland, Fredericton, New Brunswick, St. Foy, Quebec, Sault St. Marie, Ontario, and Victoria, British Columbia. U.S. Forest Service and university researchers, and state agency and industry cooperators were interviewed on the Michigan State University and University of Maine campuses. Additional U.S. Forest Service investigators and pest managers were interviewed at the Northeastern Forest Experiment Station in

Broomall, PA and at the Pacific Northwest Forest and Range Experiment Station in Portland, OR. A series of interviews of researchers, investigators, pest managers, and cooperators was conducted at the Western Forest Insect Work Conference in Santa Rosa, CA. USDA Cooperative State Research Service and U.S. Forest Service members of the JPU and the JPPC were interviewed in Washington, DC. Key people with whom the team was unable to meet with otherwise were interviewed by telephone.

When all interviews were completed, the evaluation team reconvened to summarize findings of the interviews, to identify common perceptions and findings, and to weigh issues that had been raised. In all, 138 individuals were interviewed as summarized in the following table:

Summary of Persons Interviewed by the CANUSA Review Team

Category	Canada	U.S.	Total	Percent
Administrators	8	10	18	13
Research Managers	12	13	25	18
Investigators	32	22	54	39
Users	10	31	41	30
Total	62	76	138	100

Findings of the Evaluation

While the evaluation was not to address program accomplishments, certain non-technical achievements and contributions became apparent during the critique. Foremost among these accomplishments were improved communications and personal rapport between the executive administrators of the two federal forestry agencies. This extended to the scientist and manager levels. CANUSA paved the way for cooperation on other forestry-related matters.

Specific institutional goals were accomplished by both countries. In Canada when CANUSA was being proposed, the Canadian Forestry Centres were experiencing cutbacks in staffs and budgets. The extensive spruce budworm outbreaks in the eastern provinces triggered criticism that the federal government was not doing enough to solve the problem. The joint international program reaffirmed the importance of the Canadian Forestry Service and provided a visible federal response to the budworm problem.

Events transpiring in the U.S. were similar to Canada. A major motivator for the Americans was the opportunity to reprogram funds from the on-going accelerated research and development programs that were nearing completion. Loss of the funds to the forestry research budget faced the U.S. Forest Service. CANUSA also provided a U.S. response to growing demands for more attention to the spruce budworm problem. Since the Canadians had considerable information and experience with the problem, an accelerated program in the U.S. that involved Canadians would have credibility with knowledgeable support groups.

CANUSA provided the Canadians impetus for an extensive review of their research. This review, known as the Spruce Budworm Task Force Review, and the follow-up Spruce Budworm Implementation Plan, became a national plan for Canadian spruce budworm research.

Organizational

Three organization-related elements were found to be the subject of strong opinions by many interviewees; these were the roles of the JPU and the JPPC, the roles of the Program Leaders, and the effectiveness of the Working Groups.

Formation of the JPU and the JPPC were motivated by real management concerns. The JPPC was to provide policy-level support and feedback to the program. The JPU, on the other hand, was created to provide technical oversight to the research elements and priorities. There was concern from administrators interviewed that the two policy and review bodies generally were not adequately informed on program matters to be as effective as planned. Pest managers and forest managers also doubted that the JPPC was close enough to user needs and the budworm problem in general to be able to make the best decisions. An opinion of some of the interviewees was that JPPC and JPU existed mainly to satisfy political considerations and not to satisfy program considerations.

The Memorandum of Understanding established Program Leader positions in each country. The language of the Memorandum indicates that these positions were to have broad managerial authority. In practice, the Program Leaders were second-line staff positions within their respective organizations. Although they were expected to be cognizant of national objectives and priorities, and were charged by the Memorandum to "execute the Program", they relied, of necessity, on staff influence and not on direct authority. Other administrators in the program viewed the Program Leader's role as primarily one of information coordination. Both Leader's were, in fact, more active in coordinating international communications than in coordinating or facilitating their respective national programs. One result of this situation was the general lack of coordination, regionally, of the programs within a country. This gave the appearance of four programs; Canada-East, U.S.-East, Canada-West, U.S.-West.

The third organizational finding of concern related to the Working Groups. These groups consisted of forest managers, pest managers, and researchers organized around specific program subject matters such as silviculture, biological control, and economics. They were to meet at least once a year to discuss results of previous work and to coordinate studies for the next year. While the organization and function of these groups appears to be conceptually straightforward, there was considerable dissatisfaction and confusion among users and scientists about their purpose. Some of the complaints about the groups were; that the meetings did not encourage coordination but, instead were often used by funded researchers to sell themselves and their work; that scheduling of the meetings made it difficult to participate in more than one subject; that meetings were too large for meaningful discussion; that the meetings preempted previously established coordination meetings that were of proven value.

Administration

The administration-related elements that were identified to be of concern stimulated conflicting and diverse opinions from the interviewees. Most comments related to the planning process. The planning process was criticized most often for failing to recognize differences between the two countries in administrative procedures and research needs.

Perhaps this perception arose at the beginning of budworm planning when the U.S. Forest Service developed its first plan in late 1974 and early 1975 (the "Denver Plan"). Later, in December, 1977, a modified Denver Plan was discussed in Portland, Oregon and again in Montreal. Canadians were part of both meetings. These plans were developed using the convergence analysis technique (Shea and Bayley 1976), a planning methodology used by the Americans for the three forest pest R&D programs. The technique provided a detailed framework for planning. The Canadians, on the other hand, did not have a detailed long-range budworm research plan at the program's outset.

The Canadian's long history of budworm research gave their researchers insight to specific problems and requirements in budworm research. The scope of those needs was much narrower than the all-inclusive scope in the Activity Schedule that resulted from the convergence analysis technique. Canadian researchers had no experience with the technique, and neither did many American researchers. The interviews indicated that a common opinion was that convergence analysis gave results that were unrealistically broad and naive.

Philosophical differences in the scope of such a program also affected CANUSA administration. It appeared to the review team that Canadians expected the program to concentrate on developing tools for users that could be developed and made available within its five-year time frame. U.S. investigators appeared to expect the program to accelerate the formulation of a spruce budworm management strategy that would incorporate a variety of new and existing tools.

The review team also concluded that differences between the U.S. and Canada in the level of funding for CANUSA affected the extent to which a true joint program was achieved. Canada was already involved in spruce budworm research through its on-going program when the CANUSA agreement was signed. At that time, the U.S. had limited base funds in spruce budworm research. When accelerated funds became available in the U.S., the advantage of great flexibility in research direction and opportunity for bold initiatives were presented. Accelerated funding was not made available in Canada and new opportunities were difficult to develop. The fact that some U.S. money was available for research in Canada did not generally stimulate the Canadian research community to become deeply involved in CANUSA.

Recommendations

On the basis of its findings the CANUSA Review Team made several recommendations on organizational and administrative aspects of CANUSA.

1. Combine the JPU and JPPC functions into a single advisory committee called, for example, an Executive Policy Council. The Council would decide program goals, commit resources, and monitor progress. For technical advice, it would call on experts in its constituency agencies to form a technical committee. The technical committee could be *ad hoc*.

The JPU and JPPC, on reviewing this recommendation, did not agree with the review team. Two review and advisory groups were necessary, they concluded.

2. Program Leaders in each country should report directly to the most senior lead-agency line officer responsible for the country's commitment. The positions must carry authority commensurate with the level of accountability expected.

The JPU and JPPC agreed with this recommendation, saying that the skills of program managers should be equally balanced between managerial and technical abilities.

3. Working groups are an important feature of such technical programs. Form them before the program starts, use them to review and evaluate program directions and priorities in their respective subject matters, and facilitate the dissemination of information within them.

The JPU and JPPC agreed, indicating that the groups should be flexible and adaptable to changing program needs.

4. Planning should be a structured process and bilateral so that both principals feel they have ownership in the plans that are developed. Use the first year of the program for planning; delay the phase-in of project work until plans are complete and the organization and resources are in place.

The JPU and JPPC accepted the team's recommendation.

5. Require that each partner country make

available a special fund to finance the joint program. Supplementary funding, above base-level funding, is necessary to emphasize the departure from "business as usual."

The JPU and JPPC accepted the recommendation, providing that it applies specifically to accelerated programs.

6. In an accelerated program, as opposed to a longer-term or on-going program, focus on projects that are amenable to acceleration and application. Support basic research when necessary to fill knowledge gaps that threaten program success.

The JPU and JPPC agreed with the team, clarifying that the recommendation should not apply to on-going base programs.

7. Begin early to involve users and program beneficiaries in communications and implementation of results. Have an implementation plan as part of the initial research plan for the program and for individual projects.

The JPU and JPPC accepted the team's recommendation here, adding that any modifications of agency standards to emphasize user-oriented publications should not relax general standards of publication quality.

Discussion

One could suggest that the study approach admits elements of subjectivity to enter the evaluation process. Disciplinary and national biases of the team members could certainly influence both the conduct of the interviews and the interpretation of the results. We do not feel this occurred with the CANUSA Review Team. The team recognized the prospects for this and there was some feeling that a formal questionnaire, or survey, should be part of the study approach so that findings would come from a method of data collection besides interviews. Constraints did not permit including such surveys in the study, however.

Careful selection of the review team members would appear to be the best way to obviate the element of subjectivity. Personal characteristics are at least as important as disciplinary representation. Team members must be objective from their disciplinary, regional, and national viewpoints. For an evaluation of this sort, they also must be sensitive to features of organizations that influence the objectives of the program. The CANUSA team members did not have any special training in interview techniques, but some brief formal training would probably be useful.

The study approach accommodated the restraints under which the evaluation was done, i.e. short time availability, funding availability, and minimal impact on the on-going program. The approach provides a way to obtain a comprehensive view of the administrative and organizational machinery of a large program.

One of us (JEC) was involved in an administrative capacity with the Expanded Southern Pine Beetle Research and Applications Program (ESPBRAP), a large accelerated pest programs in the U.S. that preceded CANUSA. The evaluation used for that program (Cleland et al. 1982) and the one reported here for CANUSA are not directly comparable; not in the cost of the evaluations, the time required for their completion, the approach used, the amount of data collected, nor in the objectives of the two evaluations. The ESPBRAP evaluation approach permitted the quantification of program accomplishments using a survey method to obtain clientele assessments of the program's effectiveness. The evaluation also included estimates of the benefit/cost effects of the program. One difficulty with the ESPBRAP evaluation, in fact a difficulty with the evaluations of all the U.S. Forest Pest R&D programs at that time, was that the evaluations were carried out before the efficacy of research results could be properly assessed. It would appear to us that most of the significant recommendations of the evaluations of those programs could have been obtained at less cost and in a more timely manner, with less disruption of the on-going program, using an approach similar to the one used for CANUSA.

As a result of its evaluation, the CANUSA Review Team suggested guidelines for the organization and administration of accelerated international programs. These are:

1. Identify the needs and objectives of each partner and focus the program on needs and objectives shared in common.

2. Recognize the knowledge and resource constraints under which each partner must function and structure the program to capitalize on each partner's strengths.

3. The resources available for the program should be clearly identified early in the planning process and should express each partner's commitment relative to the expected benefits to their forest resource.

4. Create a heightened sense of importance by changing "business as usual" to reinforce the special nature of the program and require participation at the executive policy level to emphasize agency commitment.

5. Establish a focal position or program leader in each country to service continuing international liaison needs and to serve as a program identity figure.

6. Provide the focal position with authority commensurate with the accountability expected of that position.

7. Keep staffs required to manage the program at a minimum and of the same relative size in both countries.

8. Employ existing agency organizational elements as much as possible to accomplish program

Some of the advantages and disadvantages of accelerated research and development programs have been discussed by Allen _et al._ (1982).

Literature Cited

Allen, Douglas C.; Cleland, David I.; Kocaoglu, Dundar F. Accelerated forest pest research and development program--a new approach. Bull. Entomol. Soc. Am. 28: 21-25; 1982.

Cleland, David I., Kocaoglu, Dundar F., Allen, Douglas C., Rose, Dietmar W., Stegman, Charles E., Abara, Jephthah A., Hughes, Jay M., Bell, Thomas R., Mengoli, Monica T. Methods used to evaluate the U.S. Department of Agriculture Combined Forest Pest Research and Development Programs. Bull. Entomol. Soc. Am. 28: 119-128; 1982.

Ketchum, D. E.; Shea, K. R. USDA combined forest pest research and development program. J. For. 75: 404:407; 1977.

Shea, K. R.; Bayley, N. E. A new approach for planning and coordinating of a large project. In: Proceedings, XVI World Congress, Int. Union of Forest Research Organizations, Div. VI. Oslo, Norway. 1976: 304-315.

FORESTRY RESEARCH AND INCOME REDISTRIBUTION[1/]

FOR "Forestry Research Planning and Evaluation: New Frontiers and New Assessment Methods," IUFRO S6.06 and S6.06.01.

David N. Bengston and Hans M. Gregersen

Graduate research assistant and professor
Department of Forest Resources
College of Forestry
University of Minnesota
St. Paul, MN 55108

Technological change resulting from forestry research generates significant economic benefits. However, only some groups within society will gain these benefits, while others could be made worse off. A better understanding of the income redistribution impacts of research and technological change is essential for forestry researchers, forest managers, administrators, and policymakers.

Five major issues relating to the income redistribution effects of research are examined, with examples drawn from forestry and agriculture. It is concluded that income redistribution effects should be explicitly included in evaluations of forestry reseach programs, and that research evaluation techniques should be developed which can address distributional issues.

Introduction

Investment in research has had a significant impact on economic efficiency in the forest-based sector (Hyde, 1983; Risbrudt, 1985, and studies cited therein). Technological change resulting from a successful research effort permits the substitution of new knowledge for scarce resources. But research also entails distributional consequences -- some groups within society will be made better off as a result of a particular technological change, while others could be made worse off.

In U.S. agriculture, decades of rapid technological change have resulted in difficult adjustments for small producers and farm workers. Research decision makers in agriculture have been accused of giving insufficient attention to these distributional impacts. Hightower (1973) documented the frustrations of some with the social consequences of research conducted in the Land Grant University system. In a current landmark legal battle, the University of California is actually being sued for the societal impacts of its publicly supported agricultural mechanization research (Kendrick, 1984; Sun, 1984). It is alleged that innovations resulting from this research have displaced farm workers, contributed to the demise of the family farm, harmed consumers, impeded collective bargaining and caused deterioration in the quality of rural life (Martin and Olmstead, 1985). Regardless of the outcome of this case, agricultural research administrators have been forced to consider more carefully the social impacts of their research.

The income redistribution effects of forestry research have also received little attention in research planning, decision making and evaluation. For example, the Forest Service's RPA planning process does not include these effects as a formal component. The heavy involvement and influence of the public sector in forestry suggest that the distributional impacts of forestry research may have important implications for public research policy.

Forestry researchers, forest managers, administrators and policymakers need to become more aware of the income redistribution effects of technological change in forestry. Knowledge of the distributional impacts of forestry research will better equip decision makers to make informed choices about the types of research to carry out and will help forest managers in their selection of new technologies that will have the social and economic impacts desired by their employers.

The purpose of this paper is to shed some light on the following five major issues related to the income redistribution effects of forestry research:

1. the distribution of research benefits between producers and consumers;
2. the distribution between large and small producers;
3. the distribution between labor and capital;
4. the distribution between geographic regions;
5. the intergenerational distribution of research benefits.

Figure 1 illustrates the relationships among these issues by showing the flow of research benefits among various groups within society. Each of

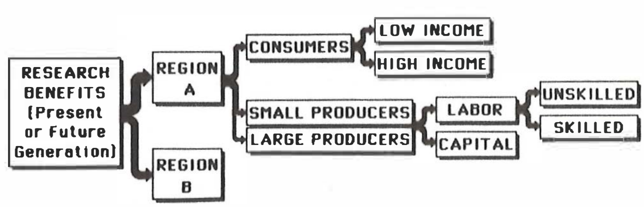

Figure 1. Distribution of research benefits among various groups.

1/ Research supported by the Department of Forest Resources, the Agricultural Experiment Station, University of Minnesota, and the USDA Forest Service and Cooperative State Research Service. The authors thank Dr. Vernon Ruttan and Dr. Allen Lundgren for helpful comments on an earlier draft of this paper.

these distributional issues will be briefly discussed, with examples drawn from forestry and agriculture. But before proceeding, it may be helpful to address the question of how technological advances make themselves felt in the economy. In other words, where do the economic benefits of research that are distributed among various groups come from?

Economic Benefits of Research

Research generates new or modified technologies, including mechanical, biological, chemical and managerial technologies. As these technological innovations are adopted and put into use they affect the processes by which goods and services are produced, allowing a reduction in the amount of inputs needed to produce a given amount of output, such as lumber or recreation services. New technologies can also enable more output to be produced from the same amount of resources. The primary economic impact of research is thus to boost productivity and reduce per unit production costs. Other types of research benefits can be identified, but these are much more difficult to measure.

Technological change can be represented graphically as an upward shift in a production function, as shown in Figure 2. The inputs required to produce output level Q are reduced from X_1 to X_2 following the adoption of new, more efficient technology. Technological change increases the "quality" of certain inputs, making them more productive when combined with other inputs needed in the production process.

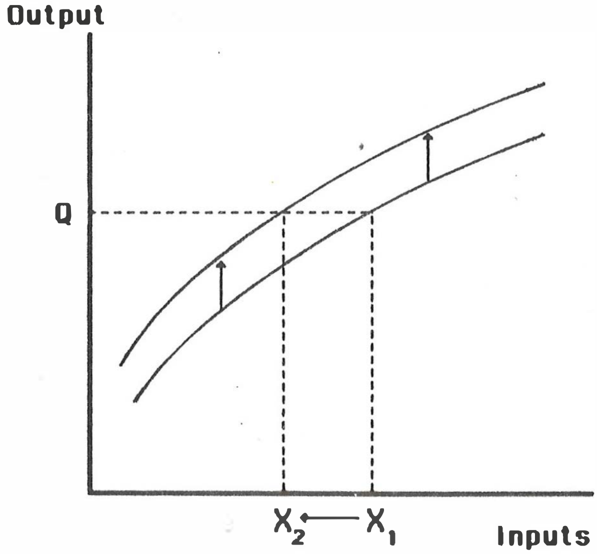

Figure 2. Resource savings due to research: technological change shifts the production function upward, enabling the same output (Q) to be produced with less inputs (X_2).

An example of technological change in forest products is the power back-up roll used in veneer production. Research at the Forest Service's Forest Products Laboratory produced this technology that makes it possible to peel bolts down to a smaller core (Fronczak and Loehnertz, 1982). Thus, the same quantity of veneer may be obtained with a smaller amount of peeler log input, and the per unit costs of producing veneer are reduced. The net reduction in costs represents a resource savings. The following sections examine how the resource savings from forestry research are distributed between various groups in society.

Distribution Between Producers and Consumers

A portion of the research benefits generated by lower production costs for some commodity may be captured by the consumers of that commodity in the form of lower prices. Producers will also share in the gains if the cost savings are not entirely passed on to consumers.

Distribution of research benefits between producers and consumers is determined mainly by the price elasticity of demand for the commodity in question (Binswanger, 1980). Producers benefit most from research on goods and services with relatively elastic demands (e.g., luxury goods). Consumers, on the other hand, will benefit more from research on commodities with relatively inelastic demands (e.g., necessities). Other factors, such as the price elasticity of supply or government pricing policies favoring either group, could also be important determinants of the distribution of research benefits between these groups.

Only one study has examined the distribution of research benefits between producers and consumers in forestry. Seldon (1985) found that consumers captured about 47 percent of the benefits of public research on softwood plywood technology between 1950 and 1980. Plywood producers received the remaining 53 percent. In agriculture, studies have shown that the major share of the gains from research generally accrue to consumers, depending on the commodity. For example, the economic benefits of rice breeding research in Japan were captured mainly by consumers (Akino and Hayami, 1975). Rice is a staple crop in Japan with a low demand elasticity. Research on cotton production in Brazil favored producers, however: about 60 percent of total research benefits were captured by this group (Ayer and Schuh, 1972). Cotton has great export potential and a relatively elastic demand in Brazil.

An obvious implication for forestry research policy in developing nations can be drawn from the fact that consumers benefit most from research on goods with inelastic demands. Forestry research directed towards basic subsistence commodities -- such as fuelwood in many developing countries -- would particularly benefit consumers in these countries. In this case, the impact of research on the distribution of income would be progressive, since poor people spend a larger share of their budget on necessities such as fuelwood and housing than the rich. The poor would experience a larger proportional gain in real income. Research on more price elastic commodities -- such as specialty paper products in many developing countries -- would tend to favor the producers of these products.

Table 1 shows the importance of the elasticity of demand in determining the distribution of research benefits between consumers and producers. The benefits of research on softwood lumber -- a commodity with a relatively inelastic demand -- will be captured mainly by the consumers of lumber. Recreation research, on the other hand, will tend to favor the producers (i.e., suppliers) of recreation services. In this case, benefiting producers more than consumers could have a progressive impact on the distribution of income, if the consumers of recreation services have above average income levels and if rural employment and income are generated by the provision of these services.

Table 1. The importance of the elasticity of demand in determining the distribution of forestry research benefits between consumers and producers.[a]

Forest Commodity	Demand Elasticity	Percent of Research Benefits: Consumers	Producers
Softwood	-.20[b]	79%	21%
Lumber	-.50[b]	63%	37%
Recreation	-1.50[c]	39%	61%
	-2.50[c]	27%	73%

[a] Assumes a given parallel shift in the supply curve for each commodity, and a supply elasticity of 1.0. The equations used to calculate the consumer's and producer's share of research benefits are given by Hertford and Schmitz (1977, p. 155).

[b] These elasticities bracket estimates of the price elasticity of demand for softwood lumber (Adams and Haynes, 1980, p. 16).

[c] These elasticities bracket estimates of the price elasticity of demand for visits to two national parks (Boyet and Tolley, 1966, p. 993).

Distribution Between Large and Small Producers

Producers seldom share equally in the gains from technological change. Two factors are most important in determining the distribution of research benefits between large and small firms. First, large producers are often the early adopters of new technologies because they have greater financial resources and incentives to do so. Early adopters will earn "innovator's rents" or quasi-rents -- short-run profits due to temporarily lower production costs relative to late adopters. Innovator's rents may be the only producer benefits of technological change if the demand for the commodity involved is highly inelastic and adoption of the innovation is widespread (Binswanger, 1980). Early adoption by large producers therefore leads to a temporary regres ive effect on income distribution among producers.

The importance of this factor in determining the distribution of research benefits among producers can be seen in the U.S. forest products industry. Large forest products firms are much more likely to have staffs capable of keeping abreast with new technologies developed through public sector research. Small firms typically lack the scientific, technical and financial resources required to do this. If, as a result, large forest products firms reap innovator's rents, this is an argument for publicly supported research and technology transfer programs to speed up the adoption of innovations among small producers and increase the overall level of competition in the industry (Runge, 1983).

Based on existing evidence, it is unclear whether large forest products firms are early adopters of technological innovations. Globerman (1976) found that large Canadian paper firms did not adopt more rapidly than smaller firms. But Hakanson (1974) found that large companies were earlier adopters of special presses for papermaking. Obviously, more research is needed to answer this question conclusively.

The second major determinant of the distribution of research benefits between large and small producers is the scale bias of new technologies. A technology is scale biased if it reduces the costs of large firms relatively more than small firms. Large producers will gain if technological advances are biased in favor of large-scale operations. An example would be highly capital-intensive logging technologies. Extensive forest landholdings may be required to profitably utilize this technology, which could decrease or rule out adoption by small producers.

The benefits of scale biased technological change accrue mainly to large firms and possibly not at all to small producers, so the impact on income distribution among producers is permanent and regressive. This is a simplification, however, since the second order effects of scale biased technology may benefit small suppliers to large companies. For example, small pulpwood suppliers may prosper from technological progress in large pulp mills.

Distribution Between Capital and Labor

Another important distributional issue is the division of producer benefits among the factors of production (i.e., labor and capital). A portion of the producer's share of research benefits may be captured by workers in the form of higher wages and expanded employment. The owners of capital may also benefit through increased returns to capital.

A key determinant of the distribution of research benefits between capital and labor is the factor-saving bias of technology (Peterson and Hayami, 1977).[2/] Technology may be classified as labor-saving, capital-saving or neutral. Labor-saving (capital-saving) technological change increases the marginal productivity of capital (labor) relative to labor (capital), and neutral technological change does not favor either input. If technology is labor-saving, the demand for labor and total payments to labor will be lower than would be the case for capital-saving or neutral technology. Similarly, capital-saving technology will reduce total payments to capital relative to what these payments would have been with labor-saving or neutral technology.

Technological change in eight U.S. forest products industries has been found to be labor-saving (Stier, 1980). An analysis of the impacts of new forest products technologies on the labor input would therefore likely be of interest to public research decisionmakers. In the sawmilling industry, for example, the adoption of new technologies has significantly reduced the labor force and increased the level of employee skills required (Duke and Huffstutler, 1977). The mechanization of forest harvesting operations has also raised skill levels among workers (Hughes, 1984). Unskilled workers -- whose labor is a substitute for the new technologies -- have no doubt experienced the greatest decline in the demand for their services and have suffered a reduction in income and employment opportunities.

Labor-saving technological change in agriculture has produced similar results on a much larger scale (Day, 1967; Schmitz and Seckler, 1970). But Peterson and Kislev (1981) have argued that the development of labor-saving technologies in agriculture may have been stimulated in large part by labor being pulled out of agriculture by higher wages in the nonfarm sector. This implies a very different response from public research than would be the case if labor has been displaced or forced out of agriculture by new technologies. Research is needed to determine whether labor has been drawn out of forestry by higher earning opportunities elsewhere or has been pushed out by the introduction of new labor-saving technology.

Distribution Between Regions

Since some forestry innovations are suited to a limited range of environmental conditions, the distribution of research benefits between geographic regions is of interest. Consider the simplest case of two regions which supply some forest-based commodity to a single national market. If researchers develop a major new technology that is applicable in one region but not in the other, production costs will decline in the first region and remain the same in the second or go up if there is a loss of market to region 1 and scale economies are relevant. Given a relatively inelastic demand for the commodity, some of the lower production costs will translate into lower consumer prices in the marketplace. The innovating region will capture a larger share of the national market, and the other region will experience a real loss as a result of the technological change.

Many examples of forestry innovations having unequal regional impacts could be cited, such as southern pine plywood and structural particleboard. During the 1960s, the rapid growth of the southern plywood industry -- made possible in part by the development of high speed lathes to peel small logs -- resulted in a reduced market share for Pacific Northwest plywood producers. The current expansion of the structural particleboard industry, primarily in the North Central region, is once again shifting the location of structural wood-based panel production and further affecting the Pacific Northwest.

Intergenerational Distribution of Research Benefits[3/]

Finally, the distribution of gains from research between time periods is an important issue in forestry because of the long production periods compared to other types of economic activity. Simply stated, future generations will benefit from biological research that increases the productivity of tree species with long growing periods. The present generation will benefit from forestry research with a shorter pay-off period, such as wood utilization research.

Figure 3 illustrates the possible effects of investment in different types of forestry research on the price of wood products over time. Heavy emphasis on utilization research would likely slow

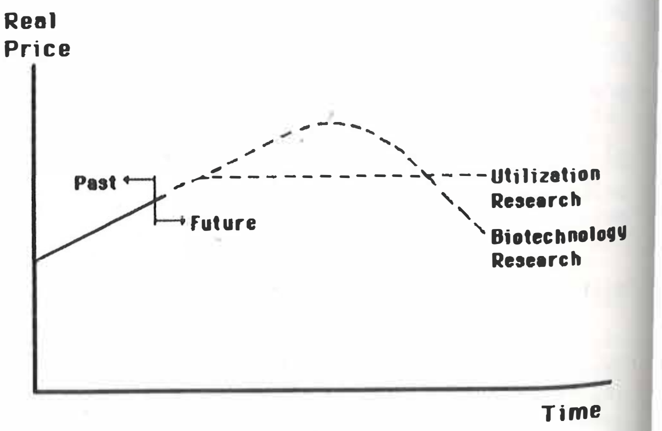

Figure 3. Possible wood products price trends with heavy investment in wood utilization research or biotechnology research.

2/ The price elasticity of input supply is also an important determinant. A more supply inelastic input will capture a larger share of research benefits, all else constant (Ayer and Schuh, 1972).

3/ The authors thank Prof. Paul Ellefson for suggesting this issue.

the rate of increase in prices in a relatively short period of time -- the lag between research and economic benefits may be quite brief. Seldon (1985) found a two year average adoption lag for technological innovations in the softwood plywood industry. Investment in biotechnology research, on the other hand, may substantially increase forest productivity and reduce the price level in the longer run. In this case, research benefits begin at the end of the first rotation of the more productive trees.

Concluding Remarks

Technological change resulting from research has significant impacts on the welfare of consumers, producers, and the growth of national and regional economies. This paper has highlighted some of the issues involved in analyzing the distributional effects of forestry research. Forestry decision makers at all levels need more information about the distributional impacts of research and technological change in the forest-based sector. However, very little empirical work has been carried out in this area.

Two main conclusions may be drawn from this overview of the distributional impacts of research. First, in light of the importance of these concerns, income redistribution effects should be explicitly included in assessments or evaluations of forestry research programs. Second, in order to accomplish this, the forestry research community should devote efforts to developing and improving evaluation techniques which can address the income redistribution impacts of research.

Literature Cited

Adams, Darius M. and Richard W. Haynes. The 1980 softwood timber assessment market model: structure, projections and policy simulations. Forest Science Monograph 22. Washington, D.C.: Society of American Foresters; 1980. 64 p.

Akino, M. and Y. Hayami. Efficiency and equity in public research: rice breeding in Japan's economic development. American Journal of Agricultural Economics. 57:1-10; 1975.

Ayer, H. W. and G. E. Schuh. Social rates of return and other aspects of agricultural research: the case of cotton research in Sao Paulo, Brazil. American Journal of Agricultural Economics. 54:557-569; 1972.

Binswanger, Hans P. Income distribution effects of technical change: some analytical issues. South East Asian Economic Review. 1(3):179-218; 1980.

Boyet, Wayne E. and George S. Tolley. Recreation projection based on demand analysis. Journal of Farm Economics. 48(4):984-1001; 1966.

Day, R. H. The economics of technological change and the demise of the share cropper. American Economic Review. 57:427-449; 1967.

Duke, J. and C. Huffstutler. Productivity in sawmills increases as labor input declines substantially. Monthly Labor Review. 100:33-37; 1977.

Fronczak, Frank J. and Stephen P. Loehnertz. Powered back-up roll -- new technology for peeling veneer. Research Paper FPL 428. U.S. Department of Agriculture, Forest Service, Forest Products Research Laboratory; 1982. 11 p.

Globerman, Steven. New technology adoption in the Canadian paper industry. Industrial Organization Review. 4:5-12; 1976.

Gregersen, H. M., J. Haygreen and I. Holland. Impacts of forest utilization research: an economic assessment. Unpublished manuscript, Department of Forest Resources, University of Minnesota; 1983.

Hakanson, S. Special presses in paper-making. In: The Diffusion of New Industrial Processes, L. Nasbeth and G. F. Ray (eds.). London: Cambridge University Press; 1974: 58-104.

Hertford, Reed and Andrew Schmitz. Measuring economic returns to agricultural research. In: Resource Allocation and Productivity in National and International Agricultural Research, T. M. Arndt, D. G. Dalrymple and V. W. Ruttan (eds.). Minneapolis: University of Minnesota Press; 1977: 148-167.

Hightower, J. Hard tomatoes, hard times: A report of the agribusiness accountability project on the failure of America's land grant college complex. Cambridge, Mass.: Schenkman Publishing; 1973. 268 p.

Hughes, A. J. G. The mechanization of forest harvesting. The Agricultural Engineer. 39(1):15-19; 1984.

Hyde, William F. (ed.). Economic evaluation of investments in forestry research. Durham, North Carolina: The Acorn Press; 1983. 106 p.

Kendrick, J. B., Jr. Agricultural research is on trial. California Agriculture. 38(5-6):2; 1984.

Martin, P. L. and A. L. Olmstead. The agricultural mechanization controversey. Science 227(Feb. 8): 601-606; 1985.

Peterson, W. and Y. Kislev. The cotton harvester in retrospect: labor displacement or replacement? Staff Paper P81-25, Department of Agricultural and Applied Economics, University of Minnesota; 1981. 15 p.

Peterson, W. L. and Y. Hayami. Technical change in agriculture. In: L. R. Martin (ed.), A Survey of Agricultural Economics Literature, Vol. I. Minneapolis: University of Minnesota Press; 1977: 497-540.

Risbrudt, C. D. (ed.). Forestry research evaluation: current progress and future directions. Proceedings of a conference in St. Paul, MN, August 20-21, 1984. General Technical Report. U.S. Department of Agriculture, Forest Service, North Central Forest Experiment Station; 1985.

Runge, C. F. Balancing public and private sector forestry research: rationale and plan of study. In: Economic Evaluation of Investments in Forestry Research, W. F. Hyde (ed.). Durham, North Carolina: The Acorn Press; 1983: 68-85.

Schmitz, A. and D. Seckler. Mechanized agriculture and social welfare: the case of the tomato harvester. American Journal of Agricultural Economics. 52(4):569-577; 1970.

Seldon, Barry J. A nonresidual approach to the measurement of social returns to research with application to the softwood plywood industry. Ph.D. dissertation, Duke University; 1985. 178 p.

Stier, Jeffrey C. Estimating the production technology in the U.S. forest products industries. Forest Science 26(3):471-482; 1980.

Sun, Marjorie. Weighing the social costs of innovation. Science 223(March 30):1368-1369; 1984.

ECONOMIC AND POLITICAL FACTORS INFLUENCING THE DEVELOPMENT OF THE SHORT ROTATION FORESTRY TECHNOLOGY

Anne S. Fege

Staff Assistant, Timber Management Research, USDA Forest Service, Washington, D.C.

Introduction

Fifteen years ago, only a few biologists and silviculturalists were interested in short rotation forestry and in using intensive cultural techniques to maximize yields. Articles on the concept and the potential yields of short rotation forestry first appeared in the 1960's, with emphasis on meeting wood fiber demands using less land and by reducing the time for a return on forestry investments. In the 1970's, public interest focused on short rotation forestry as an alternative source of energy.

In the past decade, researchers from a number of disciplines have become interested in the concept, and are now directing their efforts toward refining silvicultural practices, verifying yields, and evaluating the potential of short rotation energy forestry. Short rotation forestry research has had a clear evolution in scope and direction over the past 15 years, and ways to characterize these changes are described in this paper: history of research support, changes in socioeconomic and technological factors, evolution of field research programs, and conclusions drawn from feasibility assessments. Findings reported in this article were gained from a case study in research evaluation focused on short rotation forestry (Fege and Brown, 1984).

Short-rotation forestry has been defined many ways, and many phrases have been used to describe the concept: short-rotation hardwoods, silage sycamore, minirotation forestry, intensive culture, silvicultural biomass farms, and energy plantations. For this study, the following commonly accepted definition was used: genetically improved planting stock established at spacings of 2 x 2 m or less, managed by intensive cultural techniques to optimize total biomass production, harvested and regenerated at least every 10 years, and used for energy, pulp, or reconstituted wood products.

History of Research Support

Inquiry of the USDA Forest Service (FS), U.S. Department of Energy (DOE), Agricultural Experiment Station Directors (for funds administered by land-grant universities and state funds), and individual scientists produced financial support for short rotation forestry from 1967 to 1982. The list of active researchers was compiled from personal knowledge and search of the Smithsonian Science Information Exchange roster of ongoing research programs. Some universities reported salary allocations as part of the annual budgets, and some included only operating funds available to the scientists. Many industry contributions are not reported since they have often provided support such as land, equipment, and personnel instead of actual funds. In some instances, the short rotation forestry research was part of a larger research project, and the percentage of the funds actually directed toward the concept of short rotation forestry was estimated. In the DOE-supported programs, cost-sharing and contributions approached 25 percent of the Federal contribution (Ranney, personal communication). Funds for the earlier years may be underestimated since prior to the designation of funds for short rotation forestry, some researchers utilized funds within other research projects to install small field trials and/or maintain existing plots. On the other hand, in later years some funds in designated short rotation forestry projects may have been expended on related studies, the maintenance of plots, or the initiation of small trials to test other new concepts. Over the long term, these trends balance each other.

On an annual basis, total expenditures and number of projects were estimated for each agency (Table 1). Note that an investigator sometimes received support from several agencies. There were distinct patterns in the relationships between funding and performing agencies: some funding agencies supported only certain organizations, whereas others supported researchers affiliated with various organizations (Table 2).

As a funding agency, FS has essentially supported its own researchers. However, some funds were awarded as cooperative agreements to universities, particularly by the North Central Forest Experiment Station in Rhinelander, Wisconsin, but these are not identified separately. As a performing agency, the FS received outside funds only from DOE. Forest Service and University of Georgia scientists had established the first short rotation sycamore plots in 1966 in Georgia, as a small part of a larger FS research project. Forest Service scientists at Rhinelander, Wisconsin, conducted their research for about 5 years within a genetics project; a short rotation forestry program was designated in 1976. Scientists in four other projects in the North Central Forest Experiment Station have cooperated in economics, engineering, entomology, and pathology research. Silviculturists at Olympia, Washington, expanded research on *Alnus* in 1981 and *Populus* in 1984, and engineers at Houghton, Michigan, have cooperated internationally to develop short rotation forestry harvest systems. Many other projects have performed research in similar or supportive areas, such as the research on southern hardwood management in Stoneville, Mississippi, and the genetics of *Populus* in Durham, New Hampshire. Of the 80 scientists contacted to evaluate factors influencing research direction, 25 of these conducted their short rotation forestry-related research while employed by the Forest Service (Fege and Brown, 1984).

Fege, Anne S. and Gregory N. Brown. 1984. Research evaluation: Case study of short rotation forestry research, 1966-1982. Final report to USDA For. Serv. North Central Forest Experiment Station for Agreement 23-82-04, St. Paul, MN, 292 p.

Creation of the Energy Research and Development Administration in 1975 and its evolution into DOE in 1977 established new national research priorities for solar energy and renewable resources. In the first few years, while the biomass program was still organizing and defining research needs, several systems studies were funded, among them the study on "Silvicultural Biomass Farms" by the MITRE Corporation defining the conditions for economic production of energy from short rotation forestry (Fege, Inman and Salo, 1979).

The Department of Energy first funded field research programs at the University of Georgia and the Forest Service (Rhinelander, Wisconsin) in 1977. A competitive "Program Research and Development Announcement" was issued in 1978, and funds provided to 25 research groups. The research direction since 1978 has been determined by annual reviews of continuing projects and new proposals and by discussions between the research sponsors, the investigators, and potential users. In 1984, there were 20 research projects, directed toward minimizing costs and maximizing productivity of developing biomass for energy, with focus on species screening and genetić selection, stand management alternatives, equipment needs, fuel characterization, economic evaluation, nutrient utilization, and site productivity.

Agricultural Experiment Station funds are principally from Hatch Act or McIntire-Stennis programs administered by each state, and awarded primarily to investigators at land-grant universities. State funds are also administered through the Agricultural Experiment Station. Funds from industries and other sources have also been awarded to universities.

Land-grant universities have accepted funds from various funding agencies, whereas other universities (other public universities that are not designated as land-grant institutions, such as the University of Washington, and private universities) received no state or industry funds. Industry researchers that provided information for this study received outside funds only from DOE.

Social and Political Factors

History dealt the short rotation forestry concept a difficult beginning. Yield projections made through the mid-1970s were much higher than yields measured from subsequent field studies. These estimates were made by engineers (Szego and Kemp 1973), scientists (such as Ek and Dawson 1976), as well as journalists. The early projections had several effects. Optimism about short rotation forestry (or energy plantations) contributed to the high funding levels in the late 1970s. Since then, both skeptics and prophets have expended considerable effort defending the projections or refuting them, and researchers and administrators have justified research programs that provide realistic yield measurements. When researchers were asked to identify the most important social, political, institutional, or economic changes that have affected the development of short rotation forestry, they most frequently named the "energy crisis," future timber or fiber shortages, the environmental movement, economic factors, and the availability of funding (Fege and Brown, 1984).

Following the "energy crisis" of 1973, politicians and administrators became intent on establishing research and development programs for renewable energy sources. Once the potential of short rotation forestry had been identified and popularized, there was a "bandwagon" effect, with participants ranging from legislators to agency heads to scientists to woodlot owners. 1982 marked the peak year for funding by the FS and DOE, the two principal funding agencies. As energy prices have steadied or declined in the past few years, the political usefulness of and the funds for maintaining a strong renewable energy program have declined. However, as testimony to the projected contribution of short rotation forestry to future energy supplies, the DOE and FS continue to support these programs, at levels that are lower than those several years ago but still retain the integrity of the programs.

In the 1980s, yield projections have faded as a central discussion topic in most regions, as more field information becomes available. The contributions of short rotation forestry to the larger field of forest management may ultimately be subtle rather than direct: for example, greater acceptance of intensive site preparation, recognition of the advantages of planting clonal material, increased understanding of irrigation-fertilization interactions, and development of harvest equipment for small-diameter material. And whereas the interest in short rotation forestry in the 1960s and early 1970s was principally for pulpwood, and in the late 1970s for energy, there is now considerable interest and support in multi-product plantations, to include energy, pulp, and perhaps other forest products.

Evolution of Research Programs

The first publication of the short rotation forestry concept was an article by McAlpine and others in 1966. The first funds were targeted for short rotation forestry in 1967. Since then, there have been well over 500 publications on the subject, perhaps 100 scientists working on the research and development of short rotation forestry, and many more researchers cooperating in these research efforts. Major events are listed in Table 3; selection was based on discussions with researchers and conclusions drawn from the case study (Fege and Brown 1984).

Critical to the development of the concept has been the availability of basic information in a number of disciplines, and the "readiness" of many techniques needed to undertake the research. Researchers could draw on knowledge in tree physiology, genetics, agronomy, horticulture, chemistry, engineering, economics, and many other fields. Techniques for establishing shelterbelt trees, many of them developed in the 1930s, were applied to short rotation forestry plantations. The research on Populus deltoides in the Mississippi Delta region, undertaken by Forest Service and university scientists since the early 1960s, provided some of the early techniques for establishing and maintaining widely spaced hardwood plantations. Intensive weed control and irrigation has been common practice for some woody horticultural crops. Christmas tree growers have developed many techniques for intensive management of conifers.

Short rotation forestry has developed unevenly across the United States, principally because of regional differences in fiber demand, wood supplies, land prices and availability, and energy prices. Intangible differences in the rate of development of short rotation forestry have been created by the differences in the experience and commitment of the individual researchers and managers involved.

Principal Factors

Four research "groups" have been leaders in defining the short rotation forestry concept and in developing research programs. The groups are located in Georgia, Wisconsin, Pennsylvania and the Pacific Northwest. The sites, species, institutional sponsors and performing agencies, and approach and scope of the research programs have been very different.

The Georgia research group, led by Claud Brown, Bob McAlpine, and later Klaus Steinbeck, first published the concept and coined the phrases "silage sycamore" and "short rotation forestry." Their research efforts focused on silvicultural trials and pulping characteristics, and they cooperated primarily with forest industry. Whereas both FS and University of Georgia scientists were involved, this was mostly sequential, with FS research funds provided only from 1968 to 1972, then state funds from 1968 to 1982, and DOE funds from 1977 to 1982 provided to the University of Georgia.

The FS program in Rhinelander was one of the largest short rotation forestry research efforts in the U.S., and offers the best examples of interdisciplinary, inter-institutional research. The advantages offered by a well-funded, directed program are evident in the number of publications and the degree to which the technology of short rotation culture of Populus in the Lake States has been defined since 1970. Much of the success of this program can be attributed to Dave Dawson's and Ed Hansen's encouragement of interdisciplinary studies, and the cooperation between FS, university, and industry. Frequent meetings were held to exchange information, and there was an emphasis on the exchange of study plans and preliminary results among researchers.

The Pennsylvania State University is more typical of small, university-based research programs. Industry was not involved, and studies were not undertaken cooperatively with other research groups. Todd Bowersox maintained a small research effort for many years, essentially unfunded, then developed an interdisciplinary effort with Paul Blankenhorn in 1978 with Department of Energy funds. Research has been conducted only on the northeast Populus clones and on a few sites near the Pennsylvania State University campus, and this has restricted the regional impact of the research.

The Pacific Northwest has had the greatest number and variety of cooperators: Paul Heilman at Washington State University, Dean DeBell and others at Crown Zellerbach, Reinhard Stettler at the University of Washington, DeBell and Constance Harrington at FS, and Linda Dolan at Seattle City Light. Early funds were provided from Crown Zellerbach and the state and were "shoestring" budgets; larger field trials were installed with DOE funding beginning in 1978. The 1980s have brought the production of new Populus hybrids by Stettler, the proposal that Populus be managed in very close spacings similar to forage crops, the establishment of more than 1000 ha by Crown Zellerbach, and renewed interest in Alnus.

Forest industry managers were involved in the conceptualization and in early field trials of short rotation forestry in the late 1960s and early 1970s. Union Camp Corporation, International Paper Company, Armstrong Cork, Mead Corporation, Packaging Corporation of America, Crown Zellerbach Corporation, and other industries have cooperated with researchers in Georgia, Wisconsin, and Washington. Industries throughout the southeastern U.S. have tried intensive management of hardwood plantations, though commonly under longer rotations of 15 to

20 years. Department of Energy funds have gone directly to the BioEnergy Development Corporation in Hawaii, a subsidiary of a sugar producing company. The New York State Energy Research and Development Authority has supported Reynolds Metal Company for some Populus trials. Most field trials established by the North Carolina State University researchers have been on lands owned by industry. Crown Zellerbach Corporation and Weyerhaeuser Company, and most likely other forest industries, have planted Populus and other species in order to evaluate intensive silvicultural techniques.

Whereas the case study was limited to the development of short rotation forestry in the United States, forestry researchers all over the world have evaluated short rotations for fiber and fuel. The Ontario Ministry of Forestry sponsored small field trials of closely spaced Populus hybrids, beginning in the 1960s. In the past 5 years, more than 1000 ha of Populus plantations have been established and intensively managed in eastern Ontario. Researchers in Sweden have assessed the performance of Salix clones in 1- and 2-year rotations and have developed cultural techniques for growing Salix on drained peat land. Screening trials have been installed in Finland and Ireland; results of these and the Swedish trials have been shared in an International Energy Agency monograph (Fege 1981). Field trials of short rotation culture of Populus were established in England and Scotland from 1967 to 1974 (Cannell, 1980). There have also been research programs in Belgium, Ireland, New Zealand, Philippines, and Brazil (Jack Ranney, personal communication). Some European countries have cooperated on short rotation forestry research through the International Energy Agency, principally by sharing information and plant materials, arranging scientific exchanges, and issuing monographs. Other research and development programs for growing trees under short rotations have been supported by the U.S. Agency for International Development and the United Nations, most commonly for providing wood fuel supplies.

Fege, Anne S. 1981. Silvicultural principles and practices in short rotation energy forestry in temperate zones. Monograph prepared for International Energy Agency Agreement on Forestry Energy, Planning Group B, 101 pp.

Feasibility Assessments

The emergence of a new technology is usually followed rapidly by questions about costs and profitability. Short rotation forestry is no exception; in fact, there have been at least 25 studies in the past 12 years to assess the commercial feasibility of short rotation forestry (Table 4). The first economic analysis was published by Dutrow (1971) about 6 years after the concept of short rotation forestry was developed and the first research plots were installed. Since then, a wide range of approaches have been used to meet the studies' objectives within the limits of available data.

Clients

Most of the clients, or financial supporters, for these feasibility assessments have been U.S. government agencies (principally the Forest Service and Department of Energy) with a broad spectrum of research mandates and research beneficiaries. In a minority of instances, feasibility assessments were prepared for decision-makers or clients within the funding agency, with a perceived user or beneficiary of the study's results. The level of involvement of the client varied; in some studies, the client did not participate in defining the objectives.

Forest Service scientists prepared the first assessment (Dutrow, 1971) and four others since then. Four assessments were supported directly by DOE from 1976 to 1982 as part of long-range planning efforts, and one was a planning document for a specific 400-ha pilot plantation that the Department of Energy planned to establish on their land in South Carolina, but did not undertake. In addition, eight assessments were part of the DOE research projects managed by Oak Ridge National Laboratory. Individuals preparing the assessments were part of research teams that had ongoing field trials in short rotation forestry. These scientists were generally confident of the production factors -- costs and yields that are used in the model -- since they had several years of field experience; yet the financial aspects of the models were usually very simple, since economists were usually not research team members.

In total, these clients have invested over $1,500,000 in feasibility studies. The four studies prepared in the late 1960s with Forest Service or McIntire-Stennis funds were estimated to cost $5,000 each. Twelve of the analyses were carried out as part of larger short rotation programs or economics projects. The costs attributable to these studies probably range from $5,000 to $20,000, and a conservative average cost of $10,000 was used in the calculation. From discussions with the investigators and the author's familiarity with the studies at the time they were conducted, costs were estimated for each of the other nine studies, ranging from $100,000 to $300,000, and averaging $160,000 each.

Objectives

These assessments address a broad range of objectives, depending on the client's (or user's) goals and financial situation. Twelve of the 25 studies were interested in the effects of spacing, rotation length, number of rotations, and/or overall yields on the economic feasibility of short rotation plantations. Ten studies evaluated the effect of fertilization, irrigation, land costs, and harvest costs on economic feasibility. Nine studies examined the energy ratios or net energy available; seven of these were funded by the Department of Energy. Seven studies specified the theoretical business entity that would undertake short rotation forestry. Two studies were undertaken by companies that could actually embark on commercial short rotation forestry programs; both studies concluded that the system was currently uneconomical, but would be economical under certain conditions.

Most feasibility studies have been financial models of the production factors, expected costs and revenues, and financial conditions necessary for establishing and operating short rotation plantations. They were often conducted to answer questions about the future "value" or profitability of this new technology to private landowners and forest industries. In some instances, agencies supported these studies in order to evaluate the benefits of continuing their support for short rotation forestry research. Four general approaches were chosen by the investigators: detailed calculation of production costs, determination of financial viability (net present worth or return on investment), analysis of energy efficiency, or creation of a general flexible model. The structure of the models varied greatly, though an annual schedule of input factors, production costs, and outputs was usually prepared, then annual cash flows computed. For feasibility studies designed to evaluate net energy produced or energy ratios, energy requirements for establishment, plantation maintenance, and harvest were estimated and then compared to the estimated recoverable energy. Generally, the energy values were not discounted and the net energy calculations were expressed for each rotation or the entire project life.

Production Factors and Data Sources

Three types of production information are needed for comprehensive feasibility assessments: (1) operations and schedule, (2) production factors (land, labor and capital), and (3) costs. The production operations are the individual activities that are required, planning; leasing or purchasing land; clearing the site, perhaps of timber; building roads, equipment sheds, drainage systems, and irrigation systems, if needed; selecting species and ordering (or growing) planting stock grown from genetically improved or selected seed; and plowing or otherwise preparing the site. There was considerable variability in these management operations and the level of detail included in the models. Each activity occurs on a schedule: some occur once, at plantation establishment; some regularly, such as harvest operations; and some annually, such as fertilization or protection. Production factors can be estimated, principally as amounts of land, labor, equipment, and supplies. Sometimes, measurements made during the establishment of small plantations were used to calculate costs, but most assessments relied entirely on cost estimates from other studies. The last step is to combine these costs on a scheduled basis with the cost of capital, taxes, and financial requirements in order to evaluate the lifetime project cost, cost/ton of product, and/or "profitability" of this venture.

With a few exceptions, these studies included only production costs and expected revenues that could be quantified. Externalities were rarely mentioned for either costs or benefits, yet may include increased wildlife, increased local independence for energy or fiber, socioeconomic changes such as increased employment, and declines in site productivity over several rotations.

Conclusions of Assessments

Conclusions presented in the studies depended principally on the stated objectives. The conclusions varied widely, with relatively little agreement among studies, and no obvious pattern by year of completion. Readers should consider that the process of evaluating costs, benefits, and profitability may have been more important to the clients, potential users, and researchers than the results of the assessments. This was particularly true when a corporation evaluated short rotation forestry, using its own financial assumptions.

The most conservative generalization that can be made from these studies is that short rotation forestry may be economical under very specific financial and production conditions. The profitability of a short rotation forestry venture depends greatly on the site, yields, costs of individual production factors, and prices of alternative fuel and fiber sources. Profitability is more likely when the venture is undertaken by an existing business, whether it is a mill owner needing a reliable source of fiber, a diversified business able to take financial advantage of investment and tax opportunities, or a small non-industrial landowner interested in supplementing fuel supply.

Usefulness of Assessments

Two questions repeatedly surfaced during the study of the individual feasibility assessments, "What overall credibility should be given to this study?" and "What characteristics lend credibility or doubts to the results?" In evaluating the research efforts represented by these 25 feasibility studies, three factors were examined in each model, with respect to the model's usefulness: (1) approach to uncertainty; (2) approach to technological change; and (3) approach to financial assumptions.

There is considerable uncertainty about the production factors, anticipated costs and revenues, and commercial feasibility of short rotation forestry. Yet only seven studies included a sensitivity analysis for costs, and three studies evaluated the sensitivity of costs or economic feasibility to yields. The most useful models are those that extend the geographic and site specificity of the models by assessing the economic feasibility over a range of production costs. The life expectancies of these models is generally inversely proportional to the accuracy with which production costs are reported. Investigators that dedicated much of their effort to ensure accurate cost projections usually sacrificed the applicability of the model to other sites or future investment periods. Far more useful are those studies that evaluate sensitivity analyses and assess the relative importance of various activities within the concept.

Some elements of uncertainty are technical or biological and are often included as separate factors in the model, with probabilities of occurrence assigned to them. Nine of the studies made estimates for protecting the plantation from insect, disease, and fire losses, and one study included insurance premiums in the annual costs. There are great uncertainties in the yields of second rotation crops, from coppice regrowth, and models have usually assumed that yields will be reduced by a fixed percent in each of the subsequent rotations. The site-specific studies prepared by the Mitre Corporation approached the uncertainty aspect by preparing a critical-path analysis. Activities were identified that must be completed before other activities can be initiated.

Future changes in the technology of short rotation forestry are highly likely. And the most narrowly defined models are likely to describe the future short rotation forestry system inaccurately. The broadest outlook on short rotation forestry was taken by the MITRE Corporation in their 1979 technology assessment for the National Science Foundation. Most commonly, a technology assessment begins with a broad description of the technology and the non-technological setting of the system. The assessment focuses on the societal impacts generated by the technology, including identification of the potential impacts, stakeholders, exogenous variables, and policy alternatives. Future decision makers in both the private and public sector will need not only financial analyses but also such macroscopic visions of the changes in the economy and society that may result from the application of some or all aspects of the short rotation technology.

Financial climates within corporations or in the entire economy are likely to change even more rapidly than the technological parameters of short rotation forestry. Generally, those studies undertaken by the business entity that is in a position to make decisions about investing in short rotation forestry (such as BioEnergy of Hawaii and Seattle City Light) have the greatest "usefulness" in the short term. User-input models may be highly useful, if their structure and production factor costs are flexible. Studies that emphasize tax policies probably have the shortest life expectancies, given the history of Federal tax policy over the past 10 years and the diverse state taxation policies.

Development Stages

Mulkay (1977) describes a sequence of development of small research networks, and most of these phases can be easily identified in short rotation forestry research: (1) exploration, with loose definitions of research problems and interpretations of results; (2) concensus about research problems, definitions of variables, and techniques, developed from informal debates and formal publication of results; (3) recruitment, with exponential increases in the number of active researchers and publications; (4) specialization of research groups and individual scientists to explore a wide range of issues and to minimize competition; (5) linear growth in research as opportunities for professional recognition become scarcer; (6) decline, as new problems and observations generate new networks or results appear to have implications in other areas. Mulkay notes that research areas tend to develop in response to early major innovations, and that much of the innovative work is completed before the field acquires most of its eventual membership.

Within Mulkay's model, we would consider short rotation forestry research to currently be in the "linear growth" phase. The "exploration" and "concensus" phases would extend from 1966 to the early 1970s. From 1967 to 1970, total funds and number of funding agencies increased slowly, up to $150,000/year. There were five or less small field trials established annually, and no more than five articles were published annually. During this time period, the "classic" papers by McAlpine and others (1966) and Larson (1969) were published, and the first field plots had been established in Georgia, Wisconsin, Pennsylvania, and Washington. The "recruitment" phase would include most of the 1970s. From 1971 through 1975, more agencies provided funds, and annual funds ranged from $250,000 to $500,000. The nature of established field plots remained about the same, and 11 to 25 articles were published annually. The exponential growth included the recruitment of more than 10 additional research groups as the DOE program expanded. From 1978 to 1982, annual funds totaled $3 to $4 million, largely because of the additional DOE funding. Field plots increased in number, plot size, and total area, and included a greater number of design variables. As the additional researchers entered the field, "specialization" had begun, in terms of species and range of management practices. The "linear" phase had essentially been operating for the past several years. Few researchers entered the field, primarily because no additional funds have been available.

We had looked at the temporal development of this area of research in another way. Short rotation forestry has already proceeded through two early "stages": conceptualization of the idea, and field research programs with small plots. It is now in a third stage, focused on larger field tests. Two fairly distinctive time periods were identified within the second small-plot stage: approximately 1967 to 1975, when five or less field plots were established annually, and 1976 to 1982, when field plots increased in number, plot size, total area, and number of variables (Fege and Brown, 1984). Documentation of the later phases of research and development (larger field tests and pilot testing) within short rotation forestry is more difficult. By asking about the plot size and total area of field trials, the installation of several larger tests since 1978 with DOE or FS funds was verified. Several larger trials were established by industry, including more than 1000 ha of hybrid Populus planted by Packaging Corporation near Manistee, Michigan, in the late 1970s, about 500 ha established in Hawaii in the early 1980s, and more than 1000 ha planted by Crown Zellerbach in the early 1980s. The hardwood plantations established by industries in the southern U.S. in the late 1960s and 1970s could be considered "pilot tests" of a technology that is very similar to short rotation forestry.

Short rotation forestry research can also be characterized by the nature of the investigations. In a general sense, the sequence of research undertaken in most regions of the U.S. has been species screening, evaluation of management alternatives, economic assessments, and genetic selection efforts. Many of these phases have been conducted concurrently, or sometimes in a different order. Research is commonly also characterized as "basic" or "applied", and most of the short rotation forestry efforts are considered applied. Some distinctions have been made in short rotation forestry research relative to the emphasis on energy products or fiber. For field programs, this difference hardly exists, unless bark and wood are separated before biomass estimates are made.

Examination of the 25 feasibility studies shows that knowledge of some aspects of short rotation forestry had increased greatly between 1971 (when Dutrow prepared the first economic assessment) and 1983.. Much more information is known about the physical design and resources required to install and maintain a plantation, based on the accumulation of costs from field trials. However, confidence in the costs per ton and profitability of short rotation forestry has not increased greatly, and the skepticism has not really been eased by the volumes of feasibility reports. The studies have been useful in identifying the field operations and the biological processes that have the greatest affect on profitability of short rotation forestry, and that identification has enabled field programs to focus on the "most critical" factors.

The "next step" in the development of the short rotation forestry technology would be for forest industries, other land managers, or energy industries to establish plantations to test and adapt short rotation forestry to their land management and product objectives. To date, a few companies have established larger trials and evaluated the economics of short rotation forestry within their firm. Most of the conditions limiting the adaptation of short rotation forestry seem to be economic: the general reluctance to invest in more intensive forestry operations, high establishment costs, and current low prices of stumpage and pulpwood. Much of the interest in the 1980s has come from small landowners interested in producing fuelwood for their own use or for resale. Whereas it is extremely difficult to obtain accurate estimates of forestry operations on small landholdings, there have been many extension publications written and distributed on such subjects as growing poplars for fuel. Although each plantation may be only a few ha, the total land area may be considerable, as is the case with small Christmas tree plantations in many regions of the U.S. And there are still some technical uncertainties that will affect the development of short rotation forestry: Can we be assured that harvest equipment will be developed and be commercially available at competitive costs? What are the regeneration patterns of fast-growing species that are not considered "commercial" species? Perhaps short rotation forestry could be considered an idea that arrived "before its time", but only time can judge the wisdom of that statement.

Literature Cited

Cannell, M. G. R. 1980. Productivity of closely-spaced young poplar on agricultural soils in Britain. Forestry 53:1-21.

Dutrow, G. F. 1971. Economic implications of silage sycamore. USDA For. Serv. Res. Pap. SO-66. 9 pp.

Ek, A. R. and D. H. Dawson. 1976. Actual and projected growth and yields of Populus 'Tristis #1' under intensive culture. Can. J. For. Res. 6:132-144.

Fege, A. S., R. E. Inman, and D. G. Salo. 1979. Energy farms for the future. J. For. 77(5):358-360.

Larson, P. R. 1969. Wood formation and the concept of wood quality. Yale Univ. Sch. of For. Publ. 74, 54 pp.

McAlpine, R. G., C. L. Brown, A. M. Herrick, and H. E. Ruark. 1966. Silage sycamore. Forest Farmer 26(10):6-7, 16.

Mulkay, M. J. 1977. Sociology of the scientific research community. In:Science technology and society, a cross-disciplinary perspective, I. Spiegel-Rosing and D. Price, eds., Sage Publications, Beverly Hills, CA, p. 93-148.

Szego, G. C. and C. C. Kemp. 1973. Energy forests and fuel plantations. Chemtech (May):251-264.

Table 1. Estimated short rotation forestry research funds, by funding agency and year.[1]

Year	USFS	DOE	AES	State	Industry	Other[2]	Total
				($1000)			
1967	--	--	--	15	--	--	15
1968	100	--	--	36	--	--	136
1969	100	--	1	43	--	--	144
1970	100	--	2	49	--	--	151
1971	170	--	21	53	--	--	244
1972	170	--	12	65	1	--	248
1973	140	--	20	80	1	--	241
1974	240	--	37	95	1	--	373
1975	290	--	50	110	1	--	451
1976	340	--	102	115	1	4	562
1977	665	507	130	129	5	18	1454
1978	745	2354	174	111	13	14	3411
1979	712	2190	148	136	15	15	3216
1980	770	2764	181	166	50	72	4003
1981	750	3041	112	113	161	97	4274
1982	750	2654	81	113	221	135	3954
1983	790	2820	*[3]	*	*	*	3610
1984	395	3623	*	*	*	*	4018
1985	395	2650	*	*	*	*	3045
Total	7622	22603	1070	1429	457	355	33550
Percent funded by each agency	22.7	67.4	3.2	4.3	1.4	1.0	100.0
Mean award[4] ($1000)	232.4	142.2	12.6	16.2	19.9	25.4	

[1]Abbreviations used: U.S. Forest Service (USFS); U.S. Department of Energy (DOE); and Agricultural Experiment Station (AES), which includes McIntire-Stennis and Hatch funds.

[2]Supporting agencies for "other" included U.S. Department of Agriculture ($133,200); Ozark Regional Commission ($73,500); BioEnergy Council ($30,000), and Gas Research Institute ($118,000).

[3]Data collected only for USFS and DOE for 1983-1985.

[4]Computed as total funds divided by number of project-years funded.

Table 2. Estimated short rotation forestry research funds, by performing and funding agencies and by year.

Performing Agency	Funding Agency[1] USFS	DOE	AES	State	Industry	Other[2]	Total
	($1000)						
Forest Service							
1966-1970	300	0	0	0	0	0	300
1971-1975	1010	0	0	0	0	0	1010
1976-1980	2890	1079	0	0	0	0	3969
1981-1982	1500	554	0	0	0	0	2054
Total	5700	1633	0	0	0	0	7333
Land-grant University							
1966-1970	0	0	2	143	0	0	146
1971-1975	0	0	141	402	3	0	546
1976-1980	23	4302	56	657	51	114	5707
1981-1982	0	3008	79	227	111	211	3636
Total	23	7310	783	1429	165	325	10036
University							
1966-1970	0	0	0	0	0	0	0
1971-1975	0	0	0	0	0	0	0
1976-1980	20	618	173	0	0	10	822
1981-1982	0	318	115	0	0	20	452
Total	20	936	289	0	0	30	1276
Industry							
1966-1970	0	0	0	0	0	0	0
1970-1975	0	0	0	0	0	0	0
1976-1980	0	1816	0	0	34	0	1850
1981-1982	0	1816	0	0	271	0	2087
Total	0	3632	0	0	305	0	3937

[1]Abbreviations used: U.S. Forest Service (USFS); U.S. Department of Energy (DOE); and Agricultural Experiment Station (AES).

[2]"Other" funding agencies included U.S. Department of Agriculture, Ozark Regional Commission, BioEnergy Council, and Gas Research Institute.

Table 3. Major Events in Short Rotation Forestry Research, 1965 to 1984

Year	
Continuing	Fundamental knowledge in genetics, physiology, silviculture, utilization and other disciplines.
1965	"Silage sycamore" concept defined by researchers in Georgia.
1966	Article by McAlpine, Brown, Herrick, and Ruark defining short rotation forestry concept.
1966	Small field trials installed in Pennsylvania.
1967	Small field trials installed in Georgia.
1967	Utilization characteristics of rapidly-grown hardwoods evaluated in Georgia.
1967	Small field trials installed in Washington State.
1970	Small field trials installed in Rhinelander, WI.
1971	Forest Service designated multi-project program at Rhinelander, "Maximum Fiber Yield from Woody Species."
1971	Article by Dutrow assessing economic feasibility of short rotation forestry.
1973	Article by Szego and Kemp on "fuel plantations."
1973	Suitability of short rotation forestry for disposal of mill effluent first tested in Washington State.
1974 and 1975	Conference on intensive culture held at Iowa State University, Ames, IA.
1975	Forest Service hosts meeting in Rhinelander, reporting on five years' progress.
1975	Cumulation of 100 articles published on short rotation forestry.
1976	Article by Ek and Dawson projecting yields for short rotation culture of Populus.
1977	Systems study of energy plantations published by MITRE Corporation, McLean, VA, conducted for the Department of Energy (DOE).
1977	DOE first supported field research, at Forest Service in Rhinelander, WI and University of Georgia.
1978	DOE awarded contracts for 20 projects in short rotation forestry research.
1978	Large field trials established in Georgia (15 ha).
1978	Large field trials established in Wisconsin (10 ha).
1978	1500 ha of Populus plantations established in Michigan by Packaging Corporation of America.
1979	Species screening trials established in Illinois, Michigan, Vermont and the southeastern U.S.
1982	Cumulation of 500 articles published on short rotation forestry.
1982	Cumulation of 25 feasibility studies undertaken or completed.
1983	Over 1400 ha of field plantations established in Oregon by Crown Zellerbach Corporation.
1983	400 ha of field plantations established in Hawaii by BioEnergy Development Corportation.
1985	Large field trials (8 ha) in Alabama previously established by Scott Paper and the North Carolina State University Hardwood Research Cooperative harvested using prototype short-rotation harvester developed in Canada.

Table 4. Feasibility Studies of Short Rotation Forestry

Year of Completion	Principal Investigator and Performing Organization	Client[1]	Reference[2]
1971	George Dutrow, USDA Forest Service	USFS	1
1976	Goerge Dutrow, USDA Forest Service	USFS	2
1971	George Dutrow, USDA Forest Service	USFS	1
1976	George Dutrow, USDA Forest Service	USFS	2
1976	Dietmar Rose, Univ. Minnesota	None	3,4,5,6
1976	Todd Bowersox, Penna. State Univ.	None	7
1976	George Szego, InterTechnology Solar	DOE	8,9
1977	Robert Inman, MITRE Corporation	DOE	10,11,12
1979	David Salo, MITRE Corporation	DOE	13,14
1979	Todd Bowersox and Paul Blankenhorn, Pennsylvania State University	DOE	15,16
1979	Jerry Zavitkovski, USDA Forest Service	USFS, DOE	17
1980	Dietmar Rose, Univ. Minnesota, and Dave Lothner, USDA Forest Service	USFS, DOE	18
1980	David Salo and R. Channing Johnson, MITRE Corporation	DOE	19
1980	Mike Vasievich, USDA Forest Service	USFS	None
1980	Ray Marler, New York State Energy Research and Development Authority	DOE, self	20
1982	Wayne Geyer, Kansas State Univ.	DOE	21
1982	Flora Wang, Univ. Florida	DOE	22
1982	Gary Rolfe, Univ. Illinois	DOE	23
1982	Fred Laing, Univ. Vermont, and Paul Sendak, USDA Forest Service	DOE	None
1982	Jack Ranney, Oak Ridge National Laboratory	DOE	24
1982	Anant Vyas, Argonne National Laboratory	DOE	25
1982	Mittelhauser Corporation	DOE	26
1982	Mary Troy, Hawaii Natural Energy Institute	DOE	27
1982	Mike Morin, Packaging Corporation	self	None
1982	Linda Dolan, Seattle City Light	self	28
1983	John Helmberger, Jr., Univ. Minnesota	self	29

[1]Client: DOE=Department of Energy, USFS=Forest Service, Self=same as performing organization.

[2]References:

1. Dutrow, G. F. 1971. Economic implications of silage sycamore. USDA For. Serv. Res. Pap. SO-66: 9 p.

2. Dutrow, G. F. and J. R. Saucier. 1976. Economics of short-rotation sycamore. USDA For. Serv. Res. Pap. SO-114, 16 p.

3. DeBell, D. S. and J. C. Harms. 1976. Identification of cost factors associated with intensive culture of short-rotation forest crops. Iowa State J. Res. 50:295-300.

4. Rose, D. W. 1976. Economic investigations of intensive silvicultural systems. Iowa State J. Res. 50:301-315.

5. Rose, D. W. 1977. Cost of producing energy from wood in intensive cultures. J. Environ. Manage. 5:23-35.

6. Rose, D. W. and D. S. DeBell. 1978. Economic assessment of intensive culture of short-rotation hardwood crops. J. For. 76(11):706-711.

7. Bowersox, T. W. and W. W. Ward. 1976. Economic analysis of a short-rotation fiber production system for hybrid poplar. J. For. 74(11):750-753.

8. Szego, G. C. and C. C. Kemp. 1973. Energy forests and fuel plantations. Chemtech, May 1973, p. 275-284.

9. Henry, J. F., M. D. Fraser and C. W. Vail. 1976. The energy plantation: Design, operation and economic potential. 172nd National Meeting of the American Chemical Society, Sept. 1-2, San Francisco, CA 24 p.

10. Inman, R. E. 1977. Silvicultural biomass farms. Vol. I, summary. Mitre Tech. Rep. MTR-7347, Vol. 1, 62 p.

11. Inman, R. E., D. J. Salo, and B. J. McGurk. 1977. Silvicultural biomass farms: Volume IV. Site-specific production studies and cost analyses. Mitre Tech. Rep. MTR-7347, Vol. IV, 123 p.

12, Fege, Anne S., Robert E. Inman, and David J. Salo. 1979. Energy far for the future. J. For. 77(5):358-360.

13. Salo, D. J., J. F. Henry, and R. E. Inman. 1979. Design of a pilot silvicultural biomass farm at the Savannah River Plant. Mitre Tech. Rep. MTR-7960, 115 p.

14. Salo, D. J., J. F. Henry and A. W. DeAgazio. 1979. Pilot silvicultural biomass farm layout and design, comparative energetic and cost assessment of irrigation alternatives at the Savannah River Plant. Mitre Tech. Rep. MTR-79W00102, 135 p.

15. Blankenhorn, P. R., T. W. Bowersox and R. E. Weyers. 1982. Energy relationships for selected cultural investments. Forest Sci. 28:459-469.

16. Blankenhorn, Paul R., T. W. Bowersox, C. H. Strauss, L. R. Stover, S. Grado, G. L. Stimley, R. P. Long, and M. L. DiCola. 1982. Net energy and economic analyses for producing Populus hybrid under four management strategies. Unpublished Annual Report to U.S. Dept. of Energy, 99 pp.

17. Zavitkovski, J. 1979. Energy production in irrigated, intensively cultured plantations of Populus 'Tristis 1' and jack pine. For. Sci. 25(3):383-392.

18. Rose, Dietmar, Karen Ferguson, David C. Lothner, and J. Zavitkovski. 1980. An economic and energy analysis of poplar intensive cultures in the Lake States. USDA For. Serv. Res. Paper NC-196, 44 p.

19. Johnson, R. Channing. 1980. Energy from biomass: The implications of methanol from silvicultural farms. Mitre Tech. Rep. MTR-MP-81W8, 69 pp.

20. Marler, R. L. 1980. Development of forest biomass energy estimates for New York, Vol. I. Prepared for New York State Energy Research and Development Authority, Rep. No. 13/ET-RER/79, Rep. No. ERDA 80-31, p. 5-1 to 5-19.

21. Naughton, Gary G. and Wayne A. Geyer. 1982. An economic analysis of cottonwood energy forest plantations. Proc. North American Poplar Council, 19th Annual Meeting, July 20-22, 1982, Rhinelander, WI.

22. Wang, Flora C., John Richardson, Katherine Carter Ewel, and Edward T. Sullivan. 1981. Preliminary energy analysis of utilizing woody biomass for fuel. In: W. J. Mitsch, R. W. Bosserman, and J. M. Klopatek. Energy and Ecological Modelling. Elsevier, NY, p. 673-680.

23. White, T. A., G. L. Rolfe, and Dr. R. Bluhm. 1981. The energy balance intensively cultured autumn olive for biomass production. Ill. Agr. Exp. Sta. Dixon Springs Agricultural Center, Simpson, Ill., DSAC 9:253-259.

24. Barron, William, Robert Perlack, Peter Kroll, Janet Cushman, and J. W. Ranney. 1982. FIRSTCUT: a preliminary assessment model for short-rotation intensive silviculture. ORNL/TM-8566. Oak Ridge National Laboratory, Oak Ridge, TN.

25. Vyas, Anant D. and Sin-yan Shen. 1982. Analysis of short-rotation forests using the Argonne model for selecting economic strategy (MOSES). Argonne National Laboratory Rep. ANL/CNSV-36, 50 pp.

26. Mittelhauser Corporation. 1982. Standard economic analysis methodology for short rotation woody crops. Unpublished report under ORNL/SUB-81/69778/1 MC 410-W, Downers Grove, IL, 28 p.

27. Troy, Mary, Ed. 1982. Hydropyrolysis of biomass to produce liquid hydrocarbon fuels, report on energy tree workshop no. 2. Report produced by Pacific Resources, Inc., and Hawaii Natural Energy Institute, Honolulu, HA, DOE Grant No. DE-FG01-80RA50324, 91 pp.

28. Dolan, L. S. 1984. The cultural treatment of selected species for woody biomass production in the Pacific Northwest. Final report to the U.S. Department of Energy, Grant DE-FG-79-78BP35773.

29. Helmberger, John, Jr. 1983. Economic evaluation of short rotation fuelwood production on Minnesota peatlands: A capital budgeting approach. M.S. Thesis, Department of Agricultural and Applied Economics.

GOVERNMENT RESEARCH AND ECONOMIC WELFARE GAINS: THE CASE OF SOFTWOOD PLYWOOD[1]

Barry J. Seldon[2]
Assistant Professor, Department of Economics, Ohio University, Athens, OH 45701

This paper estimates welfare gains due to public research involving softwood plywood from 1950 through 1980. Due to the applied nature of the research, one might expect large estimates. This is indeed the case: Our least favorable assumptions lead to an estimated internal rate of return greater than 300 percent.[3]

Introduction

The impact of research upon economic welfare has often been measured as the increase in consumer surplus plus producer surplus which can reasonably be credited to research and development (R&D). The problems with this approach are well-documented and will not be discussed here.[4] In this paper, however, we apply methods developed in Seldon (1985a) and described graphically in Seldon (1985b) to determine the average productivity of public research for the entire softwood plywood industry in terms of consumer and producer surplus for the entire period 1950 through 1980. Since we include all the public research over that period, our results should be less biased than the usual case study approach which tends to look at one successful project. The reader should realize that the methods used here credit research for retarding rising costs due to exogeneous increases in the real wage rate and real cost of capital. This is not possible in the usual case study approach. Since this retardation may be substantial, it is difficult to know a priori whether this approach will lead to estimated returns to research which are higher or lower than previous case study estimates. In the industry studied here, the estimated returns to research are much higher than previous case study results for other industries. Perhaps research is more productive than previously believed.

In this study we use consumer and producer surplus as a measure of economic welfare. The concept of consumer surplus as a measure of economic welfare has a long and controversial history. However, Willig (1976) has shown consumer surplus to be a close approximation to the more theoretically precise welfare measures of compensating and equivalent variations in many plausible cases, specifically where expenditures for the good in question are a small part of total economic expenditures and/or where income elasticity is not large. An alternative might be to measure the impact of R&D upon total sales. This would give a measure of the change in value-in-trade rather than value-in-use. This measure, however, would ignore the benefits of research to consumers, and if demand were inelastic would lead to the conclusion that any cost reducing research in a competitive industry was unproductive despite the fact that consumers then would have more income to spend on all other goods. Since many people would argue that consumer welfare is important, we adopt the former more traditional welfare measure.

Finally, we should note that we will concentrate solely upon consumer plus producer surplus (hereafter economic welfare) as our welfare measure. This implicitly assumes that all economic agents are "equally worthy" so that a transfer from one agent to another nets out in the calculation. More specifically, we do not address the problem of income redistribution. While this issue is certainly important if the marginal utility of income decreases in its argument (as the author believes), data availability precludes its consideration.[5]

Supply and Demand in the Softwood Plywood Industry

The methodology used in this study requires the estimation of a supply and demand system in order to obtain estimates of the own price elasticities, the impact of (new process) R&D upon the quantity supplied, and the depreciation of R&D over time. This system is estimated in Seldon (1985a). Extensive discussion of the procedures involved is avoided in this paper, but a brief discussion of the underlying assumptions is appropriate.

First, in order to assure the existence of a supply curve we must assume the industry is competitive despite a 1978 verdict against three plywood producers convicted of a conspiracy to fix prices. The verdict here may well have been

[1]Research supported by the USDA and the Mississippi Agricultural Experiment Station.

[2]The author thanks William F. Hyde, Henry G. Grabowski, and T. Dudley Wallace for helpful comments and suggestions during the course of this research.

[3]The results presented here are adjustments to results reported in Seldon (1985a and 1985b). While the quantitative results are changed to some extent, the qualitative results are not altered. The research studied here appears to have been extremely productive. Changes are due to the use of the all commodities (producer) price index to deflate prices in our final calculations. This is consistent with our econometric equations.

[4]See, for example, Norton and Davis (1981) or Seldon (1983).

[5]For a discussion of the issues involved with research and income redistribution, see Bengston (1984).

erroneous.[6] In fact, the softwood plywood industry seems to closely approximate a contestable market. The products are homogeneous and entry costs are low.[7] If the market is contestable the firms act as competitive firms.

Secondly, the production function is assumed to be Cobb-Douglas. This assumption is fairly standard and the practice is justifiable when the firms face similar price ratios for their factors of production. In any case, the use of a more general functional form is likely to be too demanding of the data given the need to estimate additional coefficients.[8]

Thirdly, it is assumed that private firms spend a fixed fraction of total sales on R&D. This behavior is often noted empirically[9] and can be shown[10] to be optimal for a competitive firm with Cobb-Douglas technology in a dynamic setting under conditions of steady growth.

Fourthly, it is assumed that technology depreciation follows a Koyck lag structure.[11] This implicitly assumes that research does not require extensive capital investment (as was true with most of the public research we examine here). However, we do not assume that research is immediately utilized. That is, the Koyck lag itself has a lag, since research will not be utilized for a while. This reflects the fact that producers have to become aware of research and review its possibilities before they adopt it. The assumption that public research was not utilized until two years after it occurred while private research (independent of public research) was also not utilized until two years after it occurred led to the best fitting regression equations.

In particular, performing the appropriate maximization procedures, solving for the supply equation, and then applying the Koyck transformation (Kmenta, 1971, pp. 259-61) leads to the following general supply curve (although the two year lag on the Koyck term is explicit to simplify notation):

$$(1) \quad q_t = \underline{\beta}(\underline{x}_t^1 - \lambda \underline{x}_{t-1}^1) + \beta_1(p_t - \lambda p_{t-1}) + \beta_2(g_{t-2}) + \beta_3(s_{t-2}) + \lambda q_{t-1}$$

where all variables are in logarithms and

q = quantity supplied

p = own price in real terms

g = government research effort (measured in scientist weeks)

s = proxy for private research effort

$\underline{x}^1$ = a 4x1 vector composed of unity, the real wage rate, real user cost of capital, and the number of plants (so $\underline{\beta}$ is a 1x4 vector of coefficients)

Demand is calculated as[12]

$$(2) \quad q_t = \underline{\gamma}(\underline{x}_t^2) - \gamma_1 p_t$$

where $\underline{x}^2$ = a 5x1 vector composed of unity, the real wage rate in construction, a real interest rate for construction firms, a price index of lumber, and a price index for new construction.

The system was estimated using nonlinear two stage least squares estimation.

Discussion of the econometric estimates will not be pursued here. The equations are presented so the reader will better understand the estimates

---

[6]The reader is referred to Haddock (1982) who specifically alludes to the plywood case and shows how basing-point pricing can arise in a competitive market.

[7]The nature of contestable markets is examined in Baumol, Bailey, and Willig (1982). Plywood grades are standardized by the American Plywood Association in conjunction with the U.S.D.A. The cost of entry into the industry is low: many firms are single-plant, and the cost of a plant in 1962 was reported to be as low as \$1½ million (current dollars) (Fleischer and Lutz, 1962).

[8]On this point see Griliches (1979), especially pp. 95-96.

[9]This is mentioned, for instance, in Mansfield (1968, Chapter 3).

[10]See Seldon (1985a), pp. 42-47.

[11]This is also known as the geometric lag structure. See Kmenta (1971), pp. 474-82.

[12]This demand equation is derived from the supply side of the construction market. Therefore we have no need to include, for instance, the real mortgage rate. The mortgage rate affects the demand side of the construction industry, and its effect is embedded in the price of construction. If we included the mortgage rate in our equation with the price of construction our estimated coefficients would almost surely be inefficient, since that price is almost surely correlated with the mortgage rate. For a discussion of the problem, see Kmenta (1971), pp. 396-99 or any basic econometrics textbook.

of the coefficients of concern. The coefficients used in the remainder of the paper are the own price elasticity of supply (β_1), the elasticity of government research (β_2), the depreciation term on research (λ), and the own price elasticity of demand (γ_1). These are estimated to be:

$$\hat{\beta}_1 = .5011 \qquad \hat{\lambda} = .8679$$
$$\hat{\beta}_2 = .0329 \qquad \hat{\gamma}_1 = 2.6650$$

Hereafter, we drop the "hats" to simplify notation.[13]

The Calculation of Net Welfare Gains

The Benefit Calculation[14]

Using the supply equation (1), it can be shown that, given an equilibrium with price (P_t) and quantity (Q_t), a given level of public research effort (G_t) today will generate an equilibrium price stream beginning two years hence,[15] _ceteris paribus,_ where

$$P^e_{t+2+i} = P_t G_t^{b\lambda^i}; \qquad i = 0,1,2,\ldots$$

Here $b = -\beta_2/(\gamma_1+\beta_1)$, P^e is the equilibrium price realized in the future, _ceteris paribus,_ and capitalized Roman letters indicate the antilogs of lower case Roman letters. It is straightforward but tedious to show that the present value of current public research at any time t in terms of consumer surplus generated over the future is

$$PV^{CS}_t = \frac{P_t Q_t}{1-\gamma_1} \sum_{i=2}^{\infty} (1+\rho)^{-i} (1-G_t^{(1-\gamma_1)b\lambda^{i-2}})$$

where ρ is the appropriate discount rate. The infinite summation does not have a solution in a more elegant form; it is therefore useful to choose some finite time period beyond which the benefits become small enough to ignore. This of course leads to a conservative estimate of benefits. We will sum to i = 12 in what follows.

Following similar reasoning, the present value of current public research at any time t in terms of producer surplus generated over the future may be approximated by[16]

$$PV^{PS}_t = \frac{P_t Q_t}{1+\beta_1} \sum_{i=2}^{12} (1+\rho)^{-i} (G_t^{(\gamma_1-1)b\lambda^{i-2}} - 1)$$

The gross economic welfare gains due to government research at any time t is then simply

$$PV^{EW}_t = PV^{CS}_t + PV^{PS}_t.$$

The Cost Calculation[17]

In the econometric estimation we measure government research effort in each period in terms of scientist weeks as estimated from attainment reports maintained by the U.S. Forest Products Laboratory in Madison, Wisconsin. These reports and the collaboration of two individuals (Herb O. Fleischer and John F. Lutz) who were involved in government softwood plywood research from the 1950's through the mid-1970's allows us to obtain what may be as accurate a time series concerning actual research effort as may be available for any industry.

The cost per researcher week is calculated by multiplying an estimated 1977 cost per scientist year to the Forest Service by an academic R&D price index and converting these figures into weekly costs.[18] These costs, like all other monetary terms in the study, are converted into 1967 dollars. It is uncertain whether the estimated 1977 cost figure includes overhead. Therefore the weekly figure was multiplied by 1.452 to account for local overhead and the overhead of the Washington office.[19]

A multiplier is developed to use for estimating private implementation expenditures induced by government research. The only way to

[13] Our estimate of the price elasticity of demand for softwood plywood indicates that its demand is more elastic than previous estimates for plywood in general (see, for example, McKillop, Stuart, and Geissler (1980) and Rockel and Buongiorno (1982)). Our estimate neither supports nor refutes those estimates for composite plywood elasticities since softwood plywood and hardwood plywood are not substitutes. Therefore it is inappropriate to compare our estimates of the elasticity of softwood plywood demand to those of composite plywood demand.

[14] This section is condensed from Seldon (1985a, Chapter VI). The method was suggested in Seldon (1983) and a graphical exposition is given in Seldon (1985b).

[15] Recall the lag on the Koyck lag.

[16] The present value is approximated since we are limiting the summation to 12 time periods, as mentioned above.

[17] A more detailed discussion may be found in Seldon (1985a, Chapter VIII).

[18] The estimated 1977 cost per scientist year is $100,000 (Callaham, 1981, p. 26), while the basis for the academic R&D price index is Sonka and Padberg (1979, pp. 6-7). The method is similar to Bengston (1982) with a slight difference in the method of extrapolating the R&D index. For details, see Seldon (1985a), pp. 108-12.

[19] Seldon (1985a), p. 112.

do so is to calculate a multiplier based on particular projects with a well defined starting and completion date and for which cost data are available. One such case was found: the case of the powered back-up roll. The project was somewhat different from most of the projects in softwood plywood over the period 1950 through 1980. For one thing, since it involved capital equipment, useful information was not generated each time period as was usually the case.[20] But this "lumpiness" is exactly what helps us identify private costs. Because the new technology which was developed resulted in a new piece of equipment, it is easier to estimate implementation and maintenance costs. Using estimates of the equipment, installation, and maintenance cost, we conclude[21] that the ratio of private expenditures per plant to public expenditures is .26. We therefore calculate total cost as

$$E_t = C_tG_t + .26C_tG_tN_t = (1+.26N_t)C_tG_t$$

where E = social cost of implementing government R&D

C = real cost per government scientist week

G = government scientist weeks engaged in softwood plywood production research

N = number of softwood plywood plants.

Sensitivity to this multiplier is examined by supposing alternatively that the multiplier is .13 (= ½(.26)) and .39 (= 1½(.26)).

Net Welfare Gains

Once the gross welfare gains and costs have been calculated, it is a simple matter to calculate the net welfare gain each period. To be conservative with our estimates, let us assume that gross gains are realized in a lump sum at the end of the period, whereas costs are incurred in a lump sum at the beginning of the period.[22] Then each period the net welfare gain from research is

$$NWG_t = (1+\rho)^{-1}PV_t^{EW} - E_t.$$

and the net welfare gain for the entire 31-year program (1950 through 1980) is

$$NWG_T = \sum_{t=0}^{30} (1+\rho)^{-t}NWG_t$$

where t = 0 indicates 1950, t = 1 indicates 1951, and so on.

The Economic Welfare Gains, 1950-1980

In calculating the returns to public research, we report our results in two ways: We calculate the net economic welfare gains due to public research over the 31-year period and also calculate an internal rate of return. To calculate the net economic welfare gains due to government research over the 31-year period we need to specify a social discount rate, ρ. We report results for three values of ρ: 4 percent, 7 percent, and 10 percent. These rates are used for project evaluation by the U.S. Forest Service, the Water Resources Council, and the Office of Management and Budget respectively. We also perform the calculations for three private expenditure multipliers as discussed above. The net economic welfare gains are presented in Table 1, while the internal rates of return (i.e., the value of ρ which sets the net economic welfare gain equal to zero) are presented in Table 2.

TABLE 1--Net Economic Welfare Gains Due to Public Research in Softwood Plywood, 1950-1980. (values are in millions of 1967 dollars)

Private Expenditure Multiplier	Social Discount Rate .04	.07	.10
.13	4,577.7	2,508.0	1,488.5
.26	4,563.0	2,498.1	1,481.5
.39	4,548.2	2,488.2	1,474.5

TABLE 2--Internal Rates of Return for Public Research in Softwood Plywood, 1950-1980.

Private Expenditure Multiplier	Internal Rate of Return
.13	661%
.26	466%
.39	375%

[20] The projects in softwood plywood over the relevant period in general generated useful information in the course of one time period (a year). This is the reason why it was not found necessary to calculate moving averages of R&D effort as is often done in this literature.

[21] Seldon (1985a), pp. 115-117.

[22] This is overly conservative. Recall that the induced private cost of implementation will not be incurred until later, but our calculation does not discount these future costs. Also note that if demand is less elastic than our figures indicate (and some readers may feel this is the case) our welfare gains are underestimated from the beginning.

Discussion of Estimates and Conclusion

That our results are quite large is an understatement. The reader may suspect that there are multiple internal rates of return, but this is not the case. A necessary condition for this to occur is for the sign of net benefits to change more than once, but this does not occur in our sample under our assumptions concerning cost. The reader might then suspect that something else is amiss in our calculations; perhaps the impact of R&D upon output is overestimated. This, however, does not seem to be the case: From intermediate calculations we may compare an estimated value of the marginal product of softwood plywood research to similar calculations for aggregate agricultural research due to Griliches (1964) and for poultry research due to Peterson (1967). Our results here are very similar to these.[23] Therefore we may presume that our estimated coefficients are not out of line with those found in the agricultural literature.

The reason why we get high results seems to be due to both our methodology and the nature of the government's softwood plywood research. The approach credits research not only with some of the outward shifts in the supply curve (although the case study method credits research with all outward shifts); our methodology credits research also with keeping price lower than it would have been in the absence of that research at all times. The case study method may underestimate returns to research when the supply curve shifts inward due to rising real wage rates or real interest rates, while the present approach controls for these factors and in effect measures not only how research has shifted the supply curve outward, but also how research has retarded rising costs due to these other factors.

In addition, the government research studied here was very applied. Projects were completed (or at least useable results were generated) in short order; usually less than one year. Also, many of the results were applied to production quickly, since (usually) large capital investment was not required. When capital investment is required, implementation is slower since firms will allow existing capital to depreciate for some time before putting the later technology into place. Finally, diffusion was extremely rapid. In contrast to the case of agricultural research, there are few producers to notify and the producers take the initiative in keeping up with developments through frequent telephone calls and visits to the U.S. Forest Products Laboratory in Madison, Wisconsin.

[23]For these comparisons, see Seldon (1985a), p. 128.

In conclusion, we might speculate that while our high estimated internal rates of return may to a large extent be due to the institutional framework of forestry industries, it is possible that the lower estimates of other case studies result from that method's inability to control for inward shifts of the supply curve due to exogeneous factors. Perhaps the productivity of research is greater than previously believed.

Literature Cited

Baumol, William J.; John C. Panzar, and Robert D. Willig. Contestable markets and the theory of industry structure. New York: Harcourt Brace Jovanovich; 1982.

Bengston, David N. Economic impacts of forestry research: The case of structural particleboard in the United States. St. Paul, Minnesota: Unpublished master's thesis; 1982.

________. Income redistribution and the value of forestry research: Issues and approaches. Staff Paper Series Number 41. St. Paul, Minnesota: College of Forestry and the Agricultural Experiment Stateion; 1984.

Callaham, Robert Z. Criteria for deciding about forestry research programs. Washington: USDA Forestry Service General Technical Report WO-29; 1981.

Fleischer, Herb O.; and John F. Lutz. Technical considerations in the manufacture of southern pine plywood. Madison, Wisconsin: U.S. Forest Products Laboratory mimeo; 1962.

Griliches, Zvi. Research expenditures, education, and the aggregate agricultural production function. American Economic Review, 54: 961-74; 1964.

________. Issues in assessing the contribution of research and development to productivity growth. Bell Journal of Economics. 10: 92-116; 1979.

Haddock, David D. Basing-point pricing: Competitive vs. collusive theories. American Economic Review. 72: 289-306; 1982.

Kmenta, Jan. Elements of econometrics. New York: MacMillan; 1971.

Mansfield, Edwin. The economics of technological change. New York: Norton; 1968.

McKillop, William; Thomas W. Stuart; and Peter J. Geissler. Competition between wood products and substitute structural products: An econometric analysis. Forest Science. 26: 134-48; 1980.

Norton, George W.; and Jeffrey S. Davis. Evaluating returns to agricultural research: A review. American Journal of Agricultural Economics, 63: 685-99; 1981.

Peterson, Willis L. Returns to poultry research in the United States. Journal of Farm Economics. 49: 656-69; 1967.

Rockel, Mark L.; and Joseph Buongiorno. Derived demand for wood and other inputs in residential construction: A cost function approach. Forest Science. 28: 207-19; 1982.

Seldon, Barry J. Approaches to investigating the productivity of investments in forestry research. Economic evaluation of investments in forestry research, William F. Hyde (ed.). Durham, North Carolina: Acorn Press; 1983: 23-52.

________. A nonresidual approach to the measurement of social returns to research with application to the softwood plywood industry. Durham, North Carolina: Unpublished Ph.D. dissertation; 1985a.

________. Supply functions and social returns to research. Forestry research evaluation: Current progress and future directions, Christopher D. Risbrudt (ed.). St. Paul, Minnesota: USDA Forest Service; 1985b.

Sonka, S.T.; and D.I. Padberg. Estimation of an academic research and development price index. Urbana-Champaign: University of Illinois Department of Agricultural Economics, No. 79 E-100; 1979.

Willig, Robert D. Consumer's surplus without apology. American Economic Review. 66: 589-97; 1976.

BENEFITS AND COSTS OF CONTAINERIZED FOREST TREE SEEDLING RESEARCH IN THE UNITED STATES[1/]

Robert A. Westgate

Regional Economist, Army Corps of Engineers[2/]
1135 U.S.P.O. & Custom House
180 East Kellogg Boulevard, St. Paul, MN 55101

An economic surplus model is utilized to estimate research benefits resulting from the development of this method of forest tree seedling production. Public and private research expenditures are estimated using screened publication counts and by contacting firms who have undertaken or who are conducting containerized seedling research. Average internal rates of return from investment in containerized seedling research range from 37 to 111 percent, depending on assumptions concerning the percentage price/cost differential between containerized seedlings and bareroot seedlings, and research cost estimates. A sensitivity analysis shows that the rate of return is insensitive to estimates of future production. Results from research evaluations such as this can supply background information for decision-making, provide support for decisions already made (or provide impetus for changing decisions), and give verification or refutation of ideas.

Introduction

There is extensive evidence that research has a significant effect on increases in productivity, which in turn has a positive effect on economic growth and development (Arndt, Dalrymple and Ruttan 1977). A substantial amount of evaluation effort has been devoted to agricultural research and extension to measure these gains (Fishel 1971; Arndt, Dalrymple and Ruttan 1977; Araji 1980; Norton, Fishel, Paulsen and Sundquist 1981). Analyses quantifying the returns from forestry research and few, although at least two studies have been recently completed (Gregersen, Haygreen, Holland and Erkilla 1983; Bengston 1984) and a number of other efforts are underway.

Although research evaluations do not provide easy answers to what should be done, it is likely that they and other forms of technology assessments will play an increasingly important role in the future of publicly supported agricultural and forestry research, due to increasing competition among program activities for limited investment dollars.

The purpose of this paper is to examine the economic impacts of a comparatively recent forestry innovation, containerized forest tree seedlings. The analysis is based on measures of public and private research benefits and costs in the United States.

Background

There have been hundreds of reports on the production of container stock for large-scale reforestation purposes in various parts of the world (Stein, Edwards and Tinus 1975). However, only since 1957 have major research and development commitments and large investments in production facilities have been made in North America (McLean 1959; Walters 1961; Stein, Edwards and Tinus 1975). The drive for containerization in this geographical location was aided by an increased demand for planting stock beginning in the 1950s (McLean 1959; Hahn 1982; Räsänen 1982).

Canadian research and development led the way with the pioneering work of McLean (1959) and Walters (1961). Early experiments began to show some advantages over bareroot seedlings. These advantages included high survival rates, good growth rates, and an excellent potential for mechanization (Hahn 1982). Researchers soon broadened their focus to integrating container systems for handling, packaging, shipping and planting, both in Canada and in Scandinavia where the concept had spread in the early to mid 1960s (Räsänen 1982; Samuelsson 1983). Foresters in the United States did not imitate these efforts until the late 1960s and early 1970s, although Canadian research was being followed there with considerable interest (Ter Bush 1971).

The development of the styroblock system (Matthews 1971; Cayford 1972) and improvements in other systems aided a dramatic increase in the use of containerized seedling (CS) in the 1970s. These dramatic increases did not escape the U.S., where expansion in the Pacific Northwest was exponential; CS production grew from less than a million seedlings in 1970 to a 41.8 million annual seedling production rate in 1974 (Ter Bush 1974).

Recognizing the widespread applications of containerized seedling production technology and the potential embodied in it, an international conference was hosted in Denver by the Great Plains Agricultural Council (Tinus, Stein and Balmer 1974) to review CS research and development.

Progress in the still rapidly developing CS concept was reviewed again in 1981 at two major conferences, one in Savannah and the other in Toronto (Guldin and Barnett 1982; Scarratt, Glerum and Plexum 1982). These conferences addressed the state-of-the-art in the production and use of CS.

[1/] Research supported by the College of Forestry and Agricultural Experiment Station, University of Minnesota and the USDA Forest Service, North Central Experiment Station and Cooperative State Research Service.

[2/] Prior to his current position, he was Research Assistant, Department of Agricultural and Applied Economics, University of Minnesota.

Despite some problems associated with poor technology transfer, container planting today plays an important role in the reforestation programs of Canada, Scandinavia and the United States. Container-stock production is approaching 50 percent of total planting stock production in both Canada and Scandinavia (Scarratt, Glerum and Plexman 1982; Räsänen 1982), although containerized seedlings account for only slightly over 6 percent of total forest seedling production in the U.S. (American Association of Nurserymen in cooperation with the USDA Forest Service 1981).

CS production levels should increase further as the advantages exhibited by the technology gain wider recognition and acceptance. In any case:

> "Containerization brought a lot of technology to the field of reforestation. It also revolutionized the bareroot nursery system. Closely controlled, scientific rearing practices have taught us a lot about the seedlings need for nutrients, environment, and protection. This technology became useful for all seedling production" (Hahn 1982, p. 11).

Methods

Estimation of Research Benefits

Only benefits measurable in terms of savings to consumers are considered in this analysis, the most significant being those due to the lower production costs of CS relative to bareroot seedlings (BR), although economic benefits resulting from the development and utilization of CS technology come from numerous sources. This will insure a conservative estimate of benefits, since the true social returns to this research effort will be understated.

The technique used here to evaluate CS research effort is referred to in the agricultural research evaluation literature as the index number or consumer and producer surplus approach (Norton and Davis 1981; Ruttan 1982).

The index number approach is based on Marshallian concepts of social welfare and cost (Marshall 1920), which in turn follows the definition of consumer surplus forwarded by the French economist Dupuit (1844). The theory behind this approach is that the adoption of technological innovations resulting from research and development reduces the marginal cost of production, increasing production and lowering the market price. A successful research effort can thus be graphically demonstrated by a rightward shift in the supply curve for the product.

The economic surplus model adopted in this study to evaluate CS research benefits follows the methodology employed by Griliches in his 1958 hybrid corn analysis, and more recently by Bengston in his 1984 evaluation of structural particleboard research. The gross annual research benefits (B*) are calculated using the formula:

$$[1] \quad B^* = k\, P_{BR}\, Q_{CS}\, (1-\tfrac{1}{2}\, kn).$$

which can be written as:

$$[2] \quad B^* = (P_{BR} - P_{CS})\, Q_{CS}\, (1-\tfrac{1}{2}\, kn),$$

where k is a percentage measure of the price discount of CS, P is the cost of seedlings (measured per thousand), Q_{CS} is the estimated annual quantity of CS produced, and n is the absolute value of the price elasticity of demand for CS.

Values used to determine the variable k are based on BR and CS price differences in the form:

$$[3] \quad P = [(PR + TH + SP)/SL],$$

where PR represents production costs, TH transportation/handling costs, and SP site preparation and planting costs, and where SL is the survival rate following the model developed by Colby and Lewis (1973). This should give an unbiased estimate of k, since the cost of each system is being compared with the alternative method of achieving the same objective: production of established seedlings. This is an important point, for the CS technology was not widely used in the U.S. at first, and is not now in some areas, due chiefly to the assumed high cost (Guldin 1981a).

Although several studies have been carried out on the economics of CS in the U.S. (Colby and Lewis 1973; Huseby 1973; Goodwin 1975; Guldin 1981a, b; 1982a, b, c, d; 1983; Hahn and Smith 1983), no clear conclusions can be drawn from them, since the majority of these studies are for isolated instances and are not broadly applicable within or between regions. This necessitated using a variety of methods to determine values to be used in the Colby/Lewis model.

Values for P_{BR}, P_{CS} and SL are estimated by contacting knowledgeable persons in industry, in the USDA Forest Service and in universities, and asking them to estimate "average" prices (that included fixed/capital and variable/production costs) and survival rates for the most commonly used BR and CS system in their regions.

Data for the variable TH was taken from Guldin (1983). It was assumed that seedlings are transported to the planting on a daily basis, and that this entails a trip of approximately 50 miles.

Site preparation and planting costs (SP) are estimated from the 1979 Forestry Incentive Program (FIP) data (Risbrudt and Ellefson 1983), the only comprehensive regional statistics available for the variable SP. This should provide a reasonable estimation of average site preparation and planting costs: industry can usually do the job for less, but public agencies report higher costs (Guldin 1982b).

Table 1 ties all the variables together for computation of the variable k using the Colby/Lewis model.

Computing the potential benefits of CS research also requires an estimate of past,

Table 1. The economics of tree seedlings.[a]

Region	PR		TH		SP		SL		Total Cost		k
	CS	BR	CS	BR	CS	BR	CS	BR	CS	BR	
South[b]	35	30	4.12	3.09	123.36	123.36	.85	.77	191.15	204.52	.07
North[c]	80	80	4.12	3.09	123.36	123.36	.85	.85	236.84	23.684	.07[e]
West[d]	120	120	4.12	3.09	323.32	323.32	.895	.7	499.82	621.31	.20

[a] Reported in April 1984 dollars. TH and SP inflated by the producer price index. Measured per thousand seedlings.

[b] Includes AL, AR, FL, GA, KY, LA, MS, NC, OK, PR, SC, TN, TX, VA.

[c] Includes CT, DE, IL, IN, IA, ME, MD, MA, MI, MN, MO, NH, NJ, NY, OH, PA, RI, VT, WV, WI.

[d] Includes AK, AZ, CA, CO, HI, ID, KS, MT, NE, NV, NM, ND, OR, SD, UT, WA, WY.

[e] Although there is no apparent price discount using CS in the North, the extension of planting season made possible by the use of CS stock plays a large role in the continued use of CS. It is assumed here that this advantage, coupled with the flexibility inherent in a CS operation (worth a lot in the North where BR seedlings take 3-4 years to grow), translate into a k value similar to that found in the South: approximately 7%.

present, and future production. Data is scarce mainly because annual U.S. forest planting reports give no indication in their tree planting statistics of the percentage of CS stock used (USDA Forest Service 1983). Additionally the "Directory of Forest Tree Nurseries in the United States," which does include CS statistics from 1975 on, is published only every five years (USDA Forest Service 1971, 1976; American Association of Nurserymen in cooperation with the USDA Forest Service 1981).

However, enough information is available from the last two issues of the Directory, from "Forestation Notes" (USDA State and Private Forestry 1983), and from the proceedings of the 1974 and 1981 symposiums to estimate past and present production. McDonald (1983) can be used to estimate future production to 1990, and unpublished USDA Forest Service data is used to project production beyond 1990 to 2000.

Aggregated results of Q_{CS} production, with future production based on trends implicitly suggested by McDonald (1983), are summarized in Table 2.

Estimates of the price elasticity of demand for seedlings are not currently available. A review of available data on forestry price elasticities indicates that the figures reported by Haynes, Connaughton and Adams (1981) for the elasticity of demand for stumpage would be the best proxy value for CS. This appears reasonable since the number of seedlings planted is a function of the demand for stumpage. Their demand elasticities are quite similar across the regions measured. Haynes[3] suggested a value of -.1 for the overall n value.

This approach should give a fairly good approximation of n. Furthermore, as can be seen from the model for computing research benefits presented earlier; the elasticity of demand has only a second-order effect. A different estimate within a reasonable range would have only a very minor effect on the results.

Estimated research benefits for the U.S. for selected years, based on [1], are presented in Table 3.

Besides the two different cases for Q_{CS}, two different values for k are analyzed: k and ½ k. This range in value is for the purpose of conducting a sensitivity analysis when calculating the rate of return to CS research.

The smallest stream of benefits is produced under the assumption of a lower k and when Case B production estimates are used. The highest estimate of research benefits is obtained with an upper k value and when Case A production estimates are utilized.

Finally, it will be noted that CS benefits are arbitrarily assumed to cease in the year 2000. This assumption should underestimate the rate of return to a certain extent, but from a practical viewpoint, the discounting of returns some twenty years into the future means that this assumption will have only a minor impact on the calculated

[3] Personal communication

Table 2. Estimate of U.S. CS production, 1970-2000, selected years.[a]

Year	Case A[b]	Case B[c]
1970	900	900
1975	63856	63856
1980	120116	120116
1985	155177	138514
1990	188227	154131
1995	191830	155559
2000	195433	156987

[a] Reported in thousand seedlings.

[b] Based on an "optimistic" trend.

[c] Based on a "conservative" trend, generally ½ the optimistic trend on a regional basis.

Table 3. Estimated CS research benefits from production in the U.S. under different assumptions, selected years.[a]

Year	Case A		Case B	
	k = upper	k = lower	k = upper	k = lower
1970	37,953	19,072	37,953	19,072
1975	4,083,460	2,051,971	4,083,460	2,051,971
1980	10,832,715	5,443,010	10,832,715	5,443,010
1985	15,563,440	7,820,205	13,990,636	7,020,942
1990	18,702,422	9,397,417	15,545,332	7,811,137
1995	18,899,680	9,496,488	15,626,419	7,851,862
2000	19,096,939	9,595,558	15,707,506	8,892,589

[a] Reported in April 1984 dollars.

rate of return. Justification for terminating returns at some point could also be based on the likelihood of future technological innovations which might supplant CS.

Estimation of Research Costs

Public and private agencies have supported and conducted CS research in the U.S. since approximately 1969. Costs associated with this effort are estimated following the methodology developed by Bengston (1984). He used screened publication counts as an indicator of public sector research activity combined with information about cost per scientist year, industry estimates of private sector research, and deflation with a cost of research index. This technique is used because the scope and diffuse nature of the CS research effort precludes the use of financial records to account for costs, and published data is not available.

Using publication counts as a basis for estimating research costs in the public sector is not without some criticism. For a start, many public sector researchers try to squeeze more than one publication out of a single research effort by publishing their findings in more than one journal or by breaking down results into a number of small units.

This problem has been dealt with here by including only those publications appearing in refereed journals identified by the abstracting journal Forestry Abstracts rather than counting all articles on CS (for example those appearing in popular or trade magazines, which are not indicative of a unique research effort). This approach should minimize the problem:

> "To the extent that the publications are screened by knowledgeable scientists as a basis for publication, there is some assurance that a publication reflects something called 'knowledge'" (Schuh and Tollini 1979, p. 5).

The issue of research results being broken down into a number of small units remains, although it does not seem to be a significant problem if the publications included using this approach are examined.

Estimating the average number of scientist years per public sector CS research publication is perhaps the largest problem in calculating research costs using the publication count method. Publications per forest science scientist year vary considerably between different subject areas (Whaley and Bell 1982). CS research is classified as forest regeneration research (USDA Forest Service 1982) which is in turn classified as timber management research. Using the estimates given in Whaley and Bell (1982) is inappropriate, however, because this figure includes all publications and not only those appearing in refereed journals. Unpublished data from the USDA Cooperative State Research Service shows that among university forestry scientists, only about 45 percent of all publications appeared in refereed journals on average. Adjusting the 1.8 publications per scientist year figure by this amount, an estimated .8 screened publications per forest scientist year is obtained, which is equivalent to 1.23 scientist years per publication.

The scientist years per publication figure was multiplied by the deflated cost per scientist year ($100,000 in 1977; Sonka and Padberg 1979) and the number of publications to obtain the estimated annual cost of CS research in the public sector, as shown in Table 4.

The first publication of record occurred in 1969 and the number of publications appearing in refereed journals peaked at 15 in 1982. It is rather surprising to see so little publicly supported research in the early years of CS in the U.S.

Publication counts cannot be used to estimate private research costs because the strong incentives for university and government researchers to publish findings are greatly reduced in industry by company policies and security regulations.

Following examples provided by evaluators of agricultural research, estimates of research costs incurred by the private sector are obtained by contacting knowledgeable persons in industry, in the USDA Forest Service and in universities.

The estimated number of scientist years obtained from these sources are multiplied by the deflated cost per scientist year (Sonka and Padberg 1979) to obtain the estimated annual cost of CS research in the private sector, shown in Table 5.

Estimated research costs for the U.S. for selected years are presented in Table 6.

Results

The rate of return to investment in CS research can be readily calculated once benefits and costs are estimated using the internal rate of return (ROR).

A clear majority of research evaluations have used the widely used and understood ROR to calculate a measure of the relative profitability of the research investment. The ROR gives an indication of the highest interest rate at which money could be borrowed to finance the total research budget and still not incur a loss on the investment.

Internal rates of return to CS research in the U.S. under different assumptions concerning research benefits and costs are presented in Table 7. The rates of return range from 37.3 to 111.2 percent, depending on k (the price discount of CS), Q_{CS} (the estimated quantity of CS produced in the future: Case A or Case B) and C* (research cost estimates). The wide range is due to the fact that the various benefit and cost streams under different assumptions diverge fairly widely.

Table 7 clearly shows that the calculated ROR is very sensitive to variations in the estimates for k and C*. C* was varied by plus and minus 33 percent because of the somewhat uncertain nature of the research cost estimates that were employed. It can also be seen that the differences between Cases A and B (Q_{CS}) on the ROR is very small, though an examination of Table 2 indicates the reason: the two cases are not widely divergent.

The average rate of return reported here is substantially higher than the 19 percent long-run average rate of return to agricultural research in the U.S. (Peterson 1971). The most conservative rate of return estimate here, 37%, is also considerably higher than that reported by Bengston (1984) in his example from forest products.

Discussion

The mean internal rate of return range of 54 to 89 percent occurring from this type of forestry research can reasonably be considered a lower bound to research returns.

On the benefit side, the model used to estimate research benefits considers only gains measurable in terms of lower costs of production and hence consumer prices and those resulting from a more flexible operation and the extension of the planting season. This approach ignores other advantages of CS. Additionally, data gathered to

Table 4. Estimated annual U.S. public CS research expenditures.[a]

Year	Number of publications	Estimated cost/S.Y.[b]	Estimated annual public research costs[c]
1969	1	59,066	72,651
1970	1	62,220	76,531
1971	1	65,273	80,286
1972	1	68,732	84,540
1973	0	76,542	--
1974	0	83,150	--
1975	1	88,716	109,121
1976	1	93,727	115,284
1977	4	100,000	492,000
1978	6	110,150	812,907
1979	6	117,883	869,977
1980	4	126,158	620,697
1981	11	135,014	1,826,739
1982	15	144,492	2,665,877
1983	9	154,635	1,711,810
1984	5[d]	165,490	1,017,764

[a] Based on $100,000 per scientist year in 1977 and the R+D price index (Sonka and Padberg 1979).

[b] Cost per scientist 1978-1984 is based on an average annual increase of 7.02 percent over the 1968-1978 period; cost per scientist year beyond 1984 assumes constant (1984) dollars.

[c] Based on 1.23 scientist years per publication.

[d] Five publications per year are assumed after 1983.

Table 5. Estimated annual U.S. private CS research expenditures[a]

Year	Estimated scientist years	Estimated cost/S.Y.[b]	Esimtated annual private research cost
1969	0	59,066	--
1970	2	62,220	124,440
1971	3	65,273	195,819
1972	4.5	68,732	309,294
1973	4	76,542	306,168
1974	7.75	83,150	644,413
1975	7.5	88,176	661,320
1976	10	93,727	937,270
1977	14	100,000	1,400,000
1978	16.7	110,150[b]	1,839,505
1979	15.33	117,883[b]	1,807,146
1980	14.33	126,158[b]	1,807,844
1981	12.58	135,014[b]	1,698,476
1982	13	144,492[b]	1,878,396
1983	13	154,635[b]	2,010,255
1984	12.7[c]	165,490[b]	2,101,723

[a] Based on $100,000 per scientist year in 1977 and the R+D price index (Sonka and Padberg 1979).

[b] Cost per scientist year 1978-1984 is based on an average annual increase of 7.02 percent over the 1968-1978 period; cost per scientist year beyond 1984 assumes constant (1984) dollars.

[c] 12.7 scientist years per year are assumed after 1983.

Table 6. Estimate U.S. CS research expenditures, selected years.

Year	Estimated public research cost[a]	Estimated private research cost[b]	Estimated total research cost
1970	76,531	124,440	200,971
1975	109,121	661,320	770,441
1980	620,697	1,807,844	2,428,541
1985	1,017,764	2,101,723	3,119,487
1990	1,017,764	2,101,723	3,119,487
1995	1,017,764	2,101,723	3,119,487
2000	1,017,764	2,101,723	3,119,487

[a] From Table 4.

[b] From Table 5.

Table 7. Internal rate of return to containerized seedling research in the United States under different assumptions concerning research benefits and costs.[a]

Research costs	Case A		Case B	
	k = upper	k = lower	k = upper	k = lower
- 33%	74.5%	38.4%	74.4%	37.3%
x	89.2	54.2%	89.1%	53.6%
+ 33%	111.2%	74.7%	111.2%	74.6%

[a] Combining public and private CS research.

calculate the variable k does not reflect future technological improvements which should also understate the rate of return somewhat; Colby and Lewis (1973) concluded in their study that:

> "Because the techniques are new, the cost of greenhouse container seedlings should decrease with technological improvements.... On the other hand, nursery procedures have been pretty well standardized, and improvement cannot be expected at the same rate...." (p. 7).

Technological improvements are likely to have an impact on k; thus the choice of the current percentage of the difference in price is likely to understate future benefits to consumers. Finally, spillover effects in all forms are ignored.

On the cost side, the care and assumptions made in estimating CS research costs should result in upper level estimates, and hence conservative measures of the rate of return. For example, using the estimated cost of a government scientist year to value university and private research will overestimate costs if the government pays more per scientist year, as reported by Milton (1972). Additionally, estimates of private sector research were obtained not just from knowledgeable persons in the CS field, but also by contacting each company involved in CS research. Cost data were rounded upwards if there was any doubt about reported figures resulting in high estimates.

Conclusions

Rough data used to quantify containerized seedling research benefits and costs in the U.S. indicate that investment in this area of forestry research is yielding an average annual return of at least 37 percent.

The quality of the data should make one wary of basing decision on these results, although conservative assumptions concerning research benefits and costs were made throughout. An improvement in the quality of the data would make this analysis worth redoing in view of the sensitivity of the results to some of the variables.

It appears, based on the results of this study and those of another recent forestry research evaluation (Bengston 1984), that rates of return to forestry research are at least equivalent with those for agricultural research although, as in Bengston's study, the narrow focus of this study prevents broad policy implications for forestry research from being drawn from the results. It will be interesting to see if these high rates of return hold for other types of forestry research.

Despite problems with poor quality data, rough results indicating the magnitude of returns from research evaluations such as this can supply background information for decision-making, provide support for decisions already made (or provide impetus for changing decisions, if results so indicate), and give verification or refutation of ideas (Gregersen 1983).

> "In the final analysis, the hard choices will be political choices, but political leaders and the public need increased awareness of the arguments, pro and con, concerning the choices that must be made. Those who want to do right should at least be equipped with good information" (Staats 1974, p. 32).

Literature Cited

American Association of Nurserymen in cooperation with the USDA Forest Service. 1981 directory of forest tree nurseries in the United States; 1981.

Araji, A.A. (editor). Research and extension productivity in agriculture. University of Idaho, Department of Agricultural Economics and Applied Statistics; 1980.

Arndt, T.M.; Dalrymple, D.G.; Ruttan, V.W. (editors). Resource allocation and productivity in national and international agricultural research. Minneapolis: University of Minnesota Press; 1977.

Bengston, D.N. Economic impacts of structural particleboard research. For. Sci. 30: 685-697; 1984.

Cayford, J.H. Container planting systems in Canada. For. Chron. 48: 235-239; 1972.

Colby, M.K.; Lewis, G.D. Economics of containerized conifer seedlings. Res. Pap. RM-108. U.S. Department of Agriculture, Forest Service, Rocky Mountain Forest and Range Experiment Station; 1973.

Dupuit, J. De la mesure L'utilite des travaux publics (1844); translation by Barback, R.H. Int. Econ. Pap. 2: 83-110; 1952.

Fishel, W.L. (editor). Resource allocation in agricultural research. Minneapolis: University of Minnesota Press; 1971.

Goodwin, O.C. Greenhouse container seedling production manual. North Carolina Department of Natural and Economic Resources--Office of Forest Resources; 1975 (revised 1976 with addendum); Forestry Note No. 19.

Gregersen, H.M. Assessment of alternative approaches to forestry research evaluation. St. Paul, MN: University of Minnesota College of Forestry; 1983; Final Report Project No. USDA 23-81-17 submitted to U.S. Department of Agriculture, Forest Service, North Central Forest Experiment Station.

Gregersen, H.; Haygreen, J.; Holland, I.; Erikilla, D. Impacts of forest utilization research: an economic assessment. St. Paul, MN: University of Minnesota College of Forestry; 1983; Final Report Project No. USDA FP-81-0395 submitted to U.S. Department of Agriculture, Forest Service, Forest Products Laboratory.

Griliches, Z. Research costs and social returns: hybrid corn and related innovations. J. Polit. Econ. 66: 419-31; 1958.

Guldin, R.W. Containerized southern pine seedling production costs. In: Proceedings of the Symposium on Engineering Systems for Forest Regeneration. American Society of Agricultural Engineers Publication No. 10-81: 279-286; 1981a.

Guldin, R.W. An innovative step in plantation management: planting container grown seedlings. In: Proceedings of the Southern Forest Economics Workshop (Robinson, V. editor); 1981b.

Guldin, R.W. Capital intensity and economies-of-scale for different types of nurseries. In: Proceedings of Southern Containerized Forest Tree Seedling Conference. General Technical Report SO-37. U.S. Department of Agriculture, Forest Service, Southern Forest Experiment Station; 1982a: 87-95.

Guldin, R.W. What does it cost to grow seedlings in containers? Tree Plant. Notes 33: 34-47; 1982b.

Guldin, R.W. Container-grown longleaf pine regeneration costs in the sandhills. South. J. Appl. For. 6: 33-39; 1982c.

Guldin, R.W. Nursery costs and benefits of container-grown southern pine seedlings. South. J. Appl. For. 6: 93-99; 1982d.

Guldin, R.W. Regeneration costs using container-grown southern pine seedlings. Res. Pap. SO-187. U.S. Department of Agriculture, Forest Service, Southern Forest Experiment Station; 1983.

Guldin, R.W.; Barnett, J.P. (editors). Proceedings of the southern containerized forest tree seedling conference. General Technical Report SO-37. U.S. Department of Agriculture, Forest Service, Southern Forest Experiment Station; 1982.

Hahn, P.F. A historical overview of the use of containerized seedlings for operational reforestation--how did we get where we are today? In: Proceedings of the Southern Containerized Forest Tree Seedling Conference. General Technical Report SO-37. U.S. Department of Agriculture, Forest Service, Southern Forest Experiment Station; 1982: 7-12.

Hahn, P.F.; Smith, A.J. Douglas-fir planting stock performance comparison after the third growing season. Tree Plant. Notes 34: 33-39; 1983.

Haynes, R.W.; Connaughton, K.P.; Adams, D.M. Projections of the demand for national forest stumpage by region; 1980-2030. Res. Pap. PNW-282. U.S. Department of Agriculture, Forest Service, Pacific Northwest Forest and Range Experiment Station; 1981.

Huseby, K. A tree seedling greenhouse: design and costs. Montana State University, Agricultural Engineering Department and U.S. Department of Agriculture, Forest Service, Equipment Development Center; 1973.

Marshall, A. Principles of Economics. London: MacMillian & Co., Ltd.; 1920.

Matthews, R.G. Container seedling production: a provisional manual. Information Report BC-X-58. Canadian Forest Service, Pacific Forest Research Center; 1971.

McDonald, S.E. What the next decade holds for U.S. tree nurseries: a prognosis. In: Proceedings of the 1982 Southern Nursery Conference. Technical Publication R8-TP4. U.S. Department of Agriculture, Forest Service, Southern Region; 1983.

McLean, M.M. Experimental planting of tubed seedlings. Ontario Department of Lands and Forests, Division of Research; 1959. Technical Series Res. Pap. No. 39.

Milton, H.S. Cost-of-research index, 1920-1970. Op. Res. 20: 1-18; 1972.

Norton, G.W.; Fishel, W.L.; Paulson, A.A.; Sundquist, W.B. (editors). Evaluation of agricultural research. St. Paul, MN: University of Minnesota Agricultural Experiment Station; 1981. Miscellaneous Publication No. 8.

Norton, G.W.; Davis, J.S. Review of methods used to evaluate returns to agricultural research. In: Evaluation of agricultural research. St. Paul, MN: University of Minnesota Agricultural Experiment Station; 1981. Miscellaneous Publication No. 8: 26-47.

Peterson, W.L. The returns to investment in agricultural research in the U.S. In: Resource allocation in agricultural research. Minneapolis: University of Minnesota Press; 1971: 139-162.

Räsänen, P.K. Containerized tree seedling production and development prospects in Finland and Scandinavia. In: Proceedings of the Can. dian Containerized Tree Seedling Symposium. COJFRC Proceedings 0-P-10. Department of the Environment, Canadian Forest Service, Great Lakes Forest Research Centre; 1982: 9-17.

Risbrudt, C.D.; Ellefson, P.V. An economic evaluation of the 1979 forestry incentives program. St. Paul: University of Minnesota Agricultural Experiment Station: 1983. Bulletin No. 550.

Ruttan, V.W. Agricultural research policy. Minneapolis: University of Minnesota Press; 1982.

Samuelson, H. Containerized seedling systems - a survey of systems in Sweden. Skogsarbeter No. 2; 1983.

Scarratt, J.B.; Glerum, G.; Plexman, C.A. (editors). Proceedings of the Canadian containerized tree seedling symposium. COJFRC Proceedings 0-P-10. Department of Environment Canadian Forest Service, Great Lakes Research Centre; 1982

Schuh, G.E.; Tollini, H. Costs and benefits of agricultural research: the state of the arts. Washington, D.C.: World Bank; 1979. Staff Working Paper No. 360.

Sonka, S.T.; Padberg, D.I. Estimation of an academic research and development price index. University of Illinois, Department of Agricultural Economics; 1979. Staff Paper No. 79E-100.

Staats, E.B. Challenges and problems in the evaluation of governmental programs. Interfaces 5: 25-32; 1974.

Stein, W.I.; Edwards, J.L.; Tinus, R.W. Outlook for container-grown seedling use in reforestation. J. For. 73: 337-341; 1975.

TerBush, F.A. Some observations on container planting in Canada. Tree Plant. Notes 22: 8-12; 1971.

TerBush, F.A. Container boom in the northwest. In: Proceedings of the North American Containerized Forest Tree Seedling Symposium. Great Plains Agricultural Council Publication No. 68: 59-61; 1974.

Tinus, R.W.; Stein, W.I.; Balmer, W.E. (editors). Proceedings of the North American containerized forest tree seedling symposium. Great Plains Agricultural Council Publication No. 68; 1974.

USDA Forest Service. 1971 report of forest tree nurseries in the United States; 1971.

USDA Forest Service. A directory of forest tree nurseries in the United States; 1976.

USDA Forest Service. An analysis of the timber situation in the United States, 1952-2030. Forest Resource Report No. 23; 1982.

USDA Forest Service. 1982 U.S. forest planting report; 1983.

USDA Forest Service Pacific Northwest Region State and Private Forestry. 1983 nursery directory. For. Notes 71: 1-19; 1983.

Walters, J. The planting and gun and bullet: a new tree planting technique. For. Chron. 37: 94-95, 107; 1961.

Whaley, R.S.; Bell, E.F. Health of forest service economic research. J. For. 80: 347-9; 364; 1982.

EMPIRICAL ESTIMATES TO GAINS FROM PUBLIC FORESTRY RESEARCH

William F. Hyde

Associate Professor of Resource Economics
School of Forestry and Environmental Studies
Duke University
Durham, NC 27706

Barry J. Seldon

Assistant Professor of Economics
Department of Economics
Ohio University
Athens, OH 45701

This paper reviews the status of our overall returns to research project, a project which we intend to lead us to a book length manuscript on the topic. It begins with a discussion of the justifications for the public role in research and continues with a summary of our analytical approach. It then turns to our empirical evidence of the benefits from public research in the softwood plywood, sawmill and pulp mill industries and closes with a comment on our remaining work.

Research evaluation has to do with assessing the merits of expenditures on research and development. It is a timely topic in this period of tight budgets for the public agencies which traditionally provide the greatest share of research funding. Furthermore, it will always be an important topic in forestry because the gains from successful research in timber production so often are distant and uncertain, occurring only when timber is harvested--which may be long after the initial research expenditure, let alone the successful research breakthrough.

The U.S. Forest Service and the Cooperative State Research Service, with encouragement from the National Association of Professional Forestry Schools and Colleges, recently initiated support for several research evaluation projects. The purpose of this paper is to show early results of one of those projects. In particular, this paper discusses initial findings in the millions of dollars for the net social benefit obtained from public research expenditures in each of three forest products industries; the softwood plywood industry, the sawmill industry, and the pulp industry. The rates of return on public research expenditures in these industries range upward from 300 percent per annum and the marginal dollar invested in research for these industries may return as much as $20.92.

Private research is important in forestry but this paper is more concerned with the policy questions surrounding public research expenditures. We begin by reviewing the potential justifications for a public research presence and then outline our method for evaluating the social benefits and costs in the three industries. Our method is consistent with a considerable body of literature in agricultural economics and our results in the three industries are consistent with agricultu al results which suggest that we, as a society, have underinvested in research over the years. (See Ruttan (1980) for a survey.) Indeed, our results, and those generally found in the agricultural literature, suggest that the returns to public research investments are much greater than the returns anticipated from many marginal investments in other timber management activities. These results, however, raise questions for further analysis. In particular, the natural questions have to do with whether we can expect these high returns to continue for these three and other timber utilizing industries and with whether we can expect similar high returns for the more biologically oriented research in the timber growing industry. A concluding section discusses these questions and outlines the needs for further analysis.

A Role for Public Research Investments

There is much general comment about the role of public agencies in forestry research. There is even considerable discussion about whether there should be a role. Perhaps the gains to forestry research are all captured in the market by private firms capable of conducting their own research. To our minds, there is little clear thought devoted to this topic. Therefore, it may be useful to outline the rationale for this kind of public market intervention.

There are four valid economic arguments or cases justifying public research effort. First, private investors may be unwilling to invest in research projects where the research is risky or where there is a long time horizon between the initial expenditure and the anticipated final payoff from implementing the research breakthrough. This may be true even where the research investment is efficient, i.e., it has a positive present net worth, benefit/cost ratio greater than one, or rate of return greater than the firm's guiding rate of return. Tree improvement investments are an example of forestry research investments with a long delay until eventual payoff and which may be efficient.

The remaining three cases are usefully made with reference to supply functions S and S' and demand function D in Figure 1. Research breakthroughs cause a decrease in production costs--or a downward shift in the supply function from S to S'. the market clearing price-quantity relationship shifts from p_1,q_1 to the lower price and greater quantity shown by p_2,q_2. Consumers gain p_1abp_2. Producers lose p_1acp_2 but gain Ocb which may or may not be sufficient to make the aggregate impact on producers positive. In all cases, aggregate social gains (the sum of consumers' and net producers' gains) exceed research costs. Since gains to private investors (producers) may not by themselves exceed costs, then private investors may have no incentive to invest and a public presence may be both necessary and justifiable on basis of the positive net social gain.

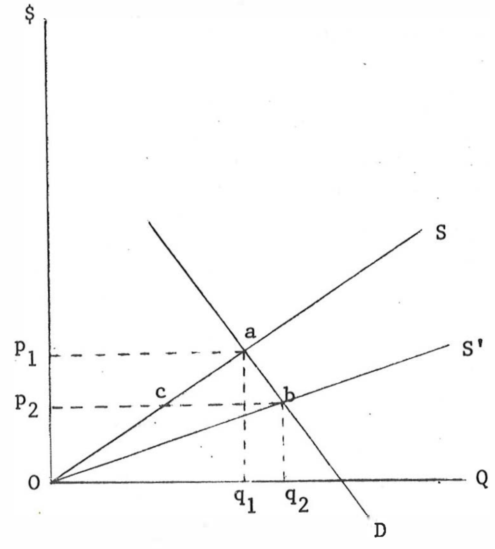

Figure 1. Consumer and Producer Returns to Research

In one case, the demand function is relatively inelastic (close to vertical), as in Figure 1, and the supply function is relatively inelastic (close to horizontal). Virtually all benefits accrue to consumers; producers may even be net losers. In this case, private industry is unlikely to conduct research. The sawmill industry is characterized by demand and supply functions similar to these. In a second case, producer gains are positive but there are many firms in the industry. Dividing the aggregate gains among each firm does not leave a sufficiently large gain to cover the research investments if each firm has to conduct its own research and, thereby, duplicate each other firm's investment. The lumber and furniture industries may be examples of this case and, for the timber growing industry, the North Carolina State University/Industry Tree Improvement Cooperative may be an example of one remedy.

The patent system attempts to generate a return on private research investments in another example for this latter case, but it does not always succeed. When rapid duplication prevents its success by causing dissipation of all private gains, then we have the fourth case justifying a public role in research. Successful patenters can sell rights to their patents for a value less than or equal to area Oab in Figure 1, thereby forcing the market to remain at the old equilibrium p_1,q_1. If, however, other firms can quickly copy the innovator by designing something similar, then the patent will be worthless. The market shifts to a new equilibrium at p_2,q_2 and all producers share the gain equal to area Ocb. The innovator with the original patent receives nothing. Thus, there is no incentive to the innovator if someone else can copy the innovation quickly and inexpensively and no private investor will innovate. The power back-up roller in plywood mills could be an example of this final case. It is a simple but very effective idea, developed at the publicly-funded Forest Products Laboratory, which places uniform pressure on logs being peeled for plywood. Once this idea became known many firms would have been able to develop the technology for themselves. There might have been no return for a private innovator.

These have been arguments why there may be justification for public involvement in a specific research area. They are not automatic justifications for all public research. Each case of public research must be examined on its own merit in terms of these four justifications. If it appears to have merit, then it must still pass the test of social efficiency. That is, the research investment in question must still obtain a return which, at the margin, equals or exceeds that anticipated for other investments, public or private.

Returns to Research in Three Forest Products Industries

We examined returns to public research in three timber utilizing forest products industries. This section briefly reviews the general method used in all three analyses and then considers our results.

The method depends on an understanding of Figure 1. We begin by estimating demand and supply functions for each industry directly. We estimate supply functions for an initial year of inquiry both with (S), and in the absence of (S'), any research inputs. We then estimate supply functions for each subsequent year which are functions of a given first year research input. The gains to research conducted in any given year are the differences between the initial year's supply in the absence of research and the supplies from subsequent years given first year research inputs (and adjusted for changes in other inputs). We find that research gains increase from year to year until the industry fully assimilates the technical breakthrough due to the initial year's research. In subsequent years the benefit from the technical change depreciates--as new research breakthroughs replace old--and the supply function shifts upward until it eventually coincides once more with the initial year supply function S. All measurable gains from initial year research have been played out. Total gains to initial year research are the sum of the differences between initial and subsequent year supply, measured for all years and properly discounted. [1/]

Our supply regressions permit determination of both average and marginal returns to research. (Previous literature could not determine marginal returns from supply analysis. Rather, marginal

[1/] It is also important to separate public from private research and research from development. Each is an input to the full shift in the supply function from S to S'. We wish to measure only that portion of the supply shift due to public research. The methods we applied for quantifying each of these inputs vary from industry to industry according to available data. Seldon (1985) provides full detail for the most complex case.

returns were determined from production functions found in an altogether different manner.) The accounting in each of our annual supply functions for year-to-year adjustments of other input costs, wages for example, permits us to claim improved accuracy. For example, rising real wages shift the supply functions upward and require offsetting research gains which are unaccounted for in the previous research evaluation literature. The inclusion of this offsetting research gain explains why our returns to research are generally greater than found in the agricultural literature.

Despite this methodological improvement, our analysis continues to have two important biases, one conservative and one of undetermined impact. Our analysis provides a conservative estimate of the total gain because it does not consider any depreciation in the initial supply function over time (the equivalent to deteriorating capital and outdated research). It overlooks, for example, the fact that the initial supply may have been partly due to an insect control strategy which the insects have long since overcome. Additional research may have been necessary to discover new strategies against the insects in order to continue producing even at the old levels. Our analysis fails to display the gains from such research (known as maintenance research), yet it includes their associated costs.

The analysis has an undetermined bias caused by its failure to consider the effect of research in one industry on the markets facing all other industries. This failure creates conservative estimates if the greater effect is on industries which produce complements to the product of our focal industry. It yields generous estimates if the greater effect is on industries which produce substitutes. The net impacts cannot be known without a general equilibrium model.[2/]

From this methodological background, let us turn to the preliminary results from our analyses of public research expenditures benefitting three forest products industries: softwood plywood, sawmills and pulp mills. Table 1 summarizes these results. They all refer to the period 1950-1980.

Our results for the sawmil industry remain incomplete until we determine one input in our analysis, a satisfactory private R&D expenditure multiplier. Our results for the pulp industry remain incomplete depending on estimation of a pulp demand function. We anticipate completing both of these, and the full results for two more industries, paper and wood preservatives, by the end of the summer.

The IRR is the rate of return which would just equate costs with benefits. The VMP is the return on the last public research dollar invested. Clearly, these two measures and the net economic welfare measure all strongly support the argument that public research in softwood plywood has been highly productive. In fact, the IRR and the VMP are both considerably in excess of what is generally expected for either marginal private or public

[2/] Bengston (1984) makes some progress on this problem in his examination of dispersion of structural particleboard research impacts.

Table 1: Measures of Public R&D success

	SWPW	Sawmills	Pulp
IRR	375%		
VMP[a/]	$20.92		
Net economic[a/] welfare due to public research	$1474.5 million		
Public research factor share	17%	7.5%	9.0%

[a/] 1967 dollars, 10% social discount rate

investment. We can safely assume that the Office of Management and Budget reviews very few investments from any public agency which approach this level of productivity. Furthermore, it should be clear that considerable expansion of the research effort in the SWPW industry would have paid off in larger social gains yet. Or, in other words, considerable expansion in public SWPW research would have been possible before the IRR fell to levels comparable to marginal investments in the rest of the U.S. economy or until the VMP fell to the point where a dollar invested yielded a discounted dollar returned.

Our fourth measure of impact is the factor share of public research. This is the portion of total output which is due to public investments in research and development. Total output of SWPW would be much lower and total output of lumber and pulp would be somewhat lower were it not for a history of public research. The smaller public research factor share in sawmills may be explained by a slower rate of technical change in this unconcentrated industry. The smaller public research share in the pulp industry may be compensated by a larger private R&D share. It is reasonable to anticipate that large firms and concentrated industries (both of which characterize pulp) spend more on private research, therefore have larger private factor shares.

These results compare well with those from agriculture and the economy as a whole. Ruttan (1982) surveyed the agricultural research literature and found returns generally ranging from 30 to 110 percent. These would increase in the direction of our SWPW returns if they were corrected for changing costs of other non-research factors of production. (This is a methodological improvement new to our analysis.) Denison (1974) found that 48 percent of aggregate U.S. productivity for the years 1929 to 1969 was due to R&D. This is larger than our range of 7.5 - 17 percent. It suggests that forestry may have lagged behind the economy as a whole in research investments. If this is the case, then we should not be surprised to see very high rates of return (as in SWPW) in those cases where there was forest research investment.

Conclusions and Implications for Further Analysis

These results raise two important points. One has to do with their general implications for public research funding and the other has to do with the limits of their implications and, therefore, with identifying important questions which are, as yet, unasked.

The basic conclusion is that there have been very large net social gains from previous public expenditures on research programs intended to benefit these industries and consumers of their products. The gains have been considerably in excess of the 5 to 25 percent range which usually guides marginal decisions of private investors. They have also probably far exceeded the gains from any changes in national forest timber management where a 10 percent rate of return is usually considered good. We can conclude that considerably more investment could have been made before the return on public research investments would have fallen to the level of marginal private or marginal national forest investments. Therefore, social gains were available to further public research investments in these three industries. Wise public policy would have permitted the necessary expansion in public agency research expenditure.

These results do not tell us, however, about the efficiency of all public research investments in other forest industries in the past and they are only suggestive of the efficiency of public research investments in the future--even for these three industries. For example, it may be that there would have been social gain from additional research in the softwood plywood industry in the 1960's, but that additional research in this industry today would only substitute for output in the structural particleboard industry and, therefore, yield no further social gain.

In order to learn whether these results are unusual or representative we need to evaluate research impacts in several other industries. In particular, we need to examine research in biological forestry. Because of the long time periods involved in growing timber and because of data differences, such examinations may be more difficult than those of research for timber utilizing industries. We might also try to obtain a measure of the gains to the aggregate of all public forestry research. This measure would save us from the potential mistake of claiming that all research gains are comparable to those from what may be a few outstanding examples.

Finally, we might attempt to anticipate gains to future forest research investments. Bengston (1984) did this for one industry, structural particleboard. If we can do this for several research projects or several industries, then we will be able to suggest with greater conviction (1) whether wise public policy includes expanding future research budgets and (2) a priority ranking for allocation within those budgets. These are the questions on which we are focusing our further research as we attempt to evaluate research in several other forest products industries.

Acknowledgements

This research was funded by the USDA Forest Service and the Cooperative State Research Service. Gary Lindell and a steering committee chaired by Douglas Richards provided invaluable advice. Melinda Sallyards, Peter Brucato, and Thomas Haxby provided research assistance.

Literature Cited

Bengston, David N. Economic impacts of structural particleboard research. For. Sci. 30(3):685-697, 1984.

Denison, E. Accounting for United States economic growth, 1929-1969. Washington, D.C.: Brookings Institution. pp. 131-137, 1974.

Ruttan, Vernon W. Bureaucratic productivity: the case of agricultural research. Pub. Choice 35:529-47, 1980.

Ruttan, Vernon W. Agricultural research policy. Minneapolis, Minnesota: University of Minnesota Press. 367 pp., 1982.

Seldon, Barry J. A residual approach to the measurement of social returns to research with application to the softwood plywood industry. Unpublished Ph.D. dissertation. Durham, N.C.: Duke University. 179 pp., 1985.

Burns, Denver P., comp. Proceedings IUFRO: Evaluation and planning of forestry research. International Union of Forestry Research Organizations (S6.06-S6.06.01); 1985 July 25-26; Fort Collins, CO. NE-GTR-111. Broomall, PA: U.S. Department of Agriculture, Forest Service, Northeastern Forest Experiment Station; 1986. 156 p. https://doi.org/10.2737/NE-GTR-111

Contains 23 papers presented in six technical sessions on forestry research management planning and evaluation. Primary topics focus on nontraditional views and sources of information and emerging technologies affecting forestry research; methods for identifying research needs and strategies required for implementation; and research evaluation at the individual, program, and organizational levels.

☆U.S. GOVERNMENT PRINTING OFFICE: 1986-605-026/40011

Headquarters of the Northeastern Forest Experiment Station are in Broomall, Pa. Field laboratories are maintained at:

- **Amherst, Massachusetts, in cooperation with the University of Massachusetts.**
- **Berea, Kentucky, in cooperation with Berea College.**
- **Burlington, Vermont, in cooperation with the University of Vermont.**
- **Delaware, Ohio.**
- **Durham, New Hampshire, in cooperation with the University of New Hampshire.**
- **Hamden, Connecticut, in cooperation with Yale University.**
- **Morgantown, West Virginia, in cooperation with West Virginia University, Morgantown.**
- **Orono, Maine, in cooperation with the University of Maine, Orono.**
- **Parsons, West Virginia.**
- **Princeton, West Virginia.**
- **Syracuse, New York, in cooperation with the State University of New York College of Environmental Sciences and Forestry at Syracuse University, Syracuse.**
- **University Park, Pennsylvania, in cooperation with the Pennsylvania State University.**
- **Warren, Pennsylvania.**