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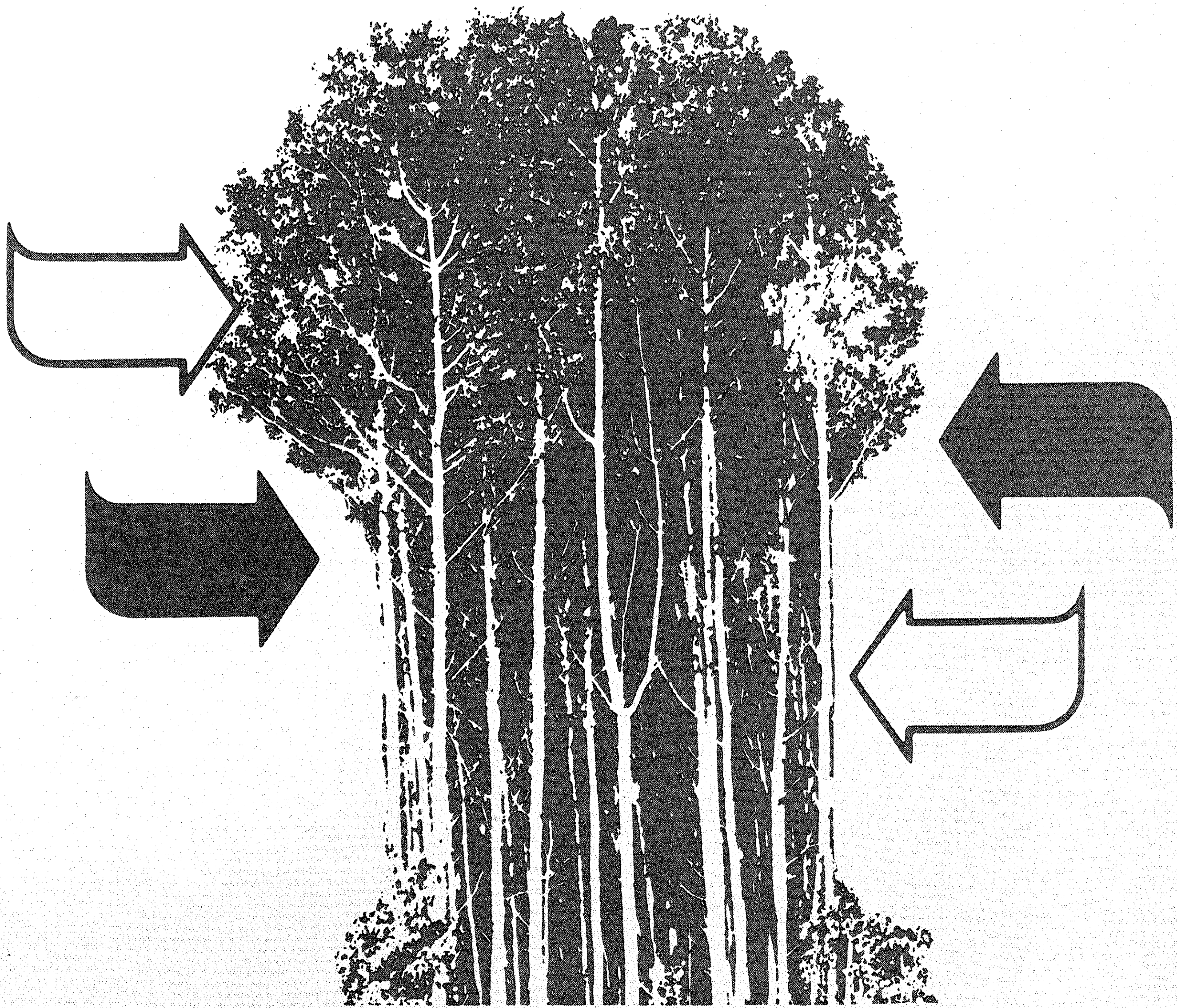
# Alternative Approaches to Forestry Research Evaluation: An Assessment



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# ALTERNATIVE APPROACHES TO FORESTRY RESEARCH EVALUATION: AN ASSESSMENT

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This publication is based on the Final Report for Project Number 23-81-17, "Assessment of Alternative Approaches to Forestry Research Evaluation," submitted to the North Central Forest Experiment Station in December 1983 by principal investigator Hans Gregersen and research assistants David Bengston, Jim Olmstead, and Robert Westgate (Gregersen *et al.* 1983a).

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# INTRODUCTION

Forestry research evaluation is a relatively new subdiscipline of evaluation research.

Fishel (1981) has identified three uses of evaluations: (1) education, (2) resource allocation, and (3) policymaking. Until recently, most forestry research evaluations have been used primarily for resource allocation or policymaking. A recent move toward more quantitative, objective evaluations has resulted in a greater use of these evaluations for education.

In forestry research evaluation we deal mainly with three potential user groups: (1) policymakers (legislators or high-level decisionmakers), (2) research administrators, and (3) researchers. These three user groups parallel those found in other disciplines. For example, the Commission of the European Communities also recognizes three levels of user groups: policy, managerial, and scientific (Boggio 1982). Although there are similarities among users of research evaluations, each evaluation must be tailored to fit the user's particular needs and situation.

This report is basically a review of the field of research evaluation. It begins with an evaluation framework that will facilitate organization of material presented later. This is followed by a discussion of research evaluation approaches from agriculture, industry, and other sectors. Examples of forestry research evaluations are then presented. It concludes with a summary of our findings and recommendations for future research.

Although we have introduced a number of approaches, this is not a cookbook for forestry research evaluation. Rather than providing detailed descriptions of methods, we have offered a framework that can aid the reader in analyzing the methods and case studies discussed. We have focused on the process, the philosophy of designing and choosing an appropriate approach, and the factors to consider in carrying out an evaluation.

The authors of this report have undertaken several major forestry research evaluations; thus, the analyses and recommendations are based on practical experience (Bengston 1984, Westgate 1985, Gregersen *et al.* 1983b). This experience indicates that the real challenge in a research evaluation is to develop an evaluation that blends the real-world constraints of data limitations, uncertainty, and conflicting objectives into a result that is useful.

# AN EVALUATION FRAMEWORK

Hans Gregersen[1] and
Allen Lundgren[2]

The problem in conducting this research was not one of finding enough evaluation material, but of digesting, organizing, and synthesizing it to make it relevant and useful for forestry policymakers, research administrators, and other potential users. The framework that follows should help the reader analyze the methods and case histories discussed later. Of many possible frameworks, we know that this one works, and that it will aid the user in developing a useful evaluation approach for a specific problem. It is based on the following steps (fig. 1): (1) Establishing the evaluation objectives, (2) identifying the evaluation measures and criteria, (3) selecting an analytical model, and (4) interpreting and presenting the results. Each element is discussed in detail below.

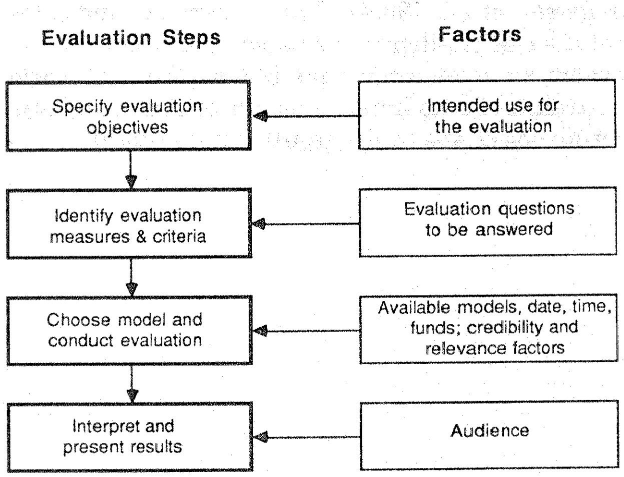


Figure 1.—*A research evaluation framework: critical steps in the evaluation process and factors affecting each step.*

[1]*The author is a Professor, Department of Forest Resources, College of Forestry, University of Minnesota.*

[2]*At the time this chapter was being prepared the author was a Forest Economist, U.S. Department of Agriculture, Forest Service, North Central Forest Experiment Station.*

## Establishing the Evaluation Objectives

Evaluations can be undertaken for a variety of reasons and with a variety of objectives—in most cases, one research evaluation can serve a number of objectives. However, when starting out it is important that the evaluator and client agree on the reasons for conducting the evaluation. In addition to the education, resource allocation, and policymaking uses discussed earlier, there may be another consideration: the client's "hidden agenda"—i.e., whether the evaluation will eventually be used in an advocacy or nonadvocacy situation. All considerations must be clear to the evaluator and client.

In addition to the objectives of the evaluation, the objectives of the research must be known, since they help determine relevant evaluation measures. The following research objectives might be encountered in applied forestry research (Fedkiw 1981, Callaham 1981):

- Reduce costs of production.
- Increase quality of goods or service.
- Reduce uncertainty in production or consumption.
- Increase quantity per unit time, resource, etc.
- Increase variety or opportunity access (e.g., new products).
- Prevent loss (maintenance research).
- Increase stability of production or consumption system.
- Redistribute costs or benefits of forestry activities.

Serious problems can arise if research objectives are defined too narrowly, since the evaluator cannot credit to the project benefits from accomplishments not identified in the objectives. The evaluation may indicate that the project was unsuccessful when in fact it was very successful. This point has been discussed by Fasella (1982) as the "Christopher Columbus effect." Columbus set off to find a new route to the Indies and failed; but one could hardly call the mission a failure. Evaluations need to be flexible, with objectives that encompass any byproducts of a research project.

## Identifying the Evaluation Measures and Criteria

The next step is to formulate questions to be answered by the evaluation. These questions help determine evaluation methods and criteria. For example, a policymaker asks for information related to the economic efficiency of a project. Because the policymaker's questions deal with economic efficiency, the evaluation must include measures of economic worth. In this case, the evaluation questions can be translated quickly into appropriate evaluation measures.

Evaluators do, of course, ask many noneconomic questions. Patton (1982) has identified 33 sets of questions asked by evaluators (table 1). Only a few of them can be answered by strictly economic evaluations. Noneconomic evaluation questions are less easily associated with specific evaluation criteria and measures. The process of identifying noneconomic criteria and measures illustrates the iterative nature of the evaluation process and the fact that most evaluations involve a series of successive approximations.

## Selecting an Analytical Model

Several sources list and describe evaluation techniques, including Porter *et al*. (1980), Schuh and Tollini (1979), and Salasin *et al*. (1980). But before an evaluator becomes immersed in the menu of potential evaluation models, it is important to remember that two actors are involved in choosing the evaluation technique: the evaluator and the client. The evaluator may be primarily concerned with the technical characteristics of the evaluation—data availability, reliability, reproducibility, etc., while the client, or decisionmaker, may be more concerned with relevance and credibility of results. These two main categories—technical considerations and client considerations—are in turn influenced by legal, political, and other constraints. They can be further subdivided as indicated in figure 2, which lays out the main factors affecting the choice of an evaluation approach.

*Technical Considerations*

Technical considerations focus on (1) constraints imposed by the client or evaluation situation, and (2) individual model characteristics. Constraints are principally dollars, time, and data availability. Four

Table 1.—*Patton's (1982) evaluation question sets*

| Question set |
|---|
| Does the program meet minimum standards for accreditation or licensing? (Accreditation evaluation) |
| What services should clients be receiving? To what extent are current services appropriate to client needs? (Appropriateness evaluation) |
| Who knows about the programs? What do they know? (Awareness focus) |
| What is the relationship between program costs and program outcomes (benefits) expressed in dollars? (Cost/benefit analysis) |
| To what extent has a specific objective been attained at the desired level of attainment (the criterion)? (Cost-effectiveness evaluation) |
| What information is needed to make a specific decision at a precise point in time? (Decision-focused evaluation) |
| What happens in the program? (No "why" questions or cause/effect analyses.) (Descriptive evaluation) |
| To what extent is the program effective in attaining its goals? (Effectiveness evaluation) |
| Can inputs be reduced and still obtain the same level of output or can greater output be obtained with no increase in inputs? (Efficiency evaluation) |
| What are the inputs into the program in terms of numbers of personnel, staff/client ratios, and other descriptors of levels of activity and effort in the program? (Effort evaluation) |
| To what extent is this program able to deal with the total problem? How does the present level of services compare to the needed level of services? (Extensiveness evaluation) |
| The evaluation is conducted by people outside the program in an effort to increase objectivity. (External evaluation) |
| How can the program be improved? (Formative evaluation) |
| To what extent do individual clients attain individual goals on a standardized measurement scale of 1 (low attainment) to 5 (high attainment)? (Goal attainment scaling evaluation) |
| To what extent have program goals been attained? (Goals-based evaluation) |
| What are the actual effects of the program on clients (without regard to what staff say they want to accomplish?) (Goal-free evaluation) |
| What are the direct and indirect program effects on the larger community of which it is a part? (Impact evaluation) |

(Table 1 continued)

Table 1.—Continued

| Question set |
|---|
| Program staff conduct the evaluation. (Internal evaluation) |
| What happens to the program and to participants over time? (Longitudinal evaluation) |
| Was the evaluation well done? Is it worth using? (Meta-evaluation) |
| What do clients need and how can those needs be met? (Needs assessment) |
| How does this program population compare to some specific norm or reference group on selected variables? (Norm-referenced evaluation) |
| To what extent are desired client outcomes being attained? What are the effects of the program on clients? (Outcomes evaluation) |
| What are participants actually able to do as a result of participation in the program? (Performance evaluation) |
| How effective are staff in carrying out their assigned tasks and in accomplishing their individual goals? (Personnel evaluation) |
| What are the strengths and weaknesses of day-to-day operations? How are program processes perceived by staff, clients, and others? What are the basic program processes? How can these processes be improved? (Process evaluation) |
| What are the characteristics of specific and concrete products produced by or used in a program? What are the costs, benefits, and effects of those products? (Product evaluation) |
| Are minimum and accepted standards of care being routinely and systematically provided to patients and clients? How can quality of care be monitored and demonstrated? (Quality assurance) |
| What routine social and economic data should be monitored to assess the impacts of this program (e.g., health statistics, housing statistics, employment statistics)? (Social indicators) |
| Should the program be continued? If so, at what level? (Summative evaluation) |
| What information is needed and wanted by decisionmakers, information users, and stakeholders that will actually be used for program improvement and to make decisions about the program? (Utilization-focused evaluation) |

characteristics of each model or technique are particularly important in the selection process: (1) reliability, (2) relevance, (3) relative cost, and (4) data requirements.

Reliability is determined by the theoretical soundness of the model, reproducibility of the results, objectivity, and acceptance by the scientific community. Model relevance is concerned with two factors: the extent to which the approach can generate results by the time they are needed, and the extent to which the model provides answers to the evaluation questions. Relative cost is the cost of using the model in relation to the available budget.

The question of data needs and availability is a critical one. Models differ dramatically in data needs. In the case of forestry, models that depend heavily on production and growth time series data, which are generally unavailable, may have to be discarded. Schuh and Tollini (1979) concludes that, "...simple approaches which are less demanding in terms of data are more useful than more complicated procedures which have to be based on more precarious data."

*Client Considerations*

As discussed earlier, the major impact of the client on the evaluation process is in determining objectives. We've discussed the three uses of research evaluation: (1) resource allocation, (2) policymaking, and (3) education. The evaluation technique is also affected by whether the evaluation will be used in an advocacy or nonadvocacy situation.

Another major consideration is the client's criteria for use. Although the relevance of the evaluation is important in determining whether an evaluation is used, some would argue that the overriding consideration is credibility. A 1976 report of the House Subcommittee on Oversight and Investigations states that the most significant factor in the acceptance of a benefit-cost study is the name of the sponsor (Anonymous 1980). Thus, the question of credibility is critical.

Credibility is influenced by four factors:

1. The perceived independence of the evaluator. For example, the research evaluation program of the Commission of the European Communities (CEC), stressed that "the independence of the assessments was considered essential to ensure objectivity and so to provide credibility" (Contzen *et al*. 1982).
2. The understandability of the evaluation approach.
3. The consistency of the evaluation results with other evaluations. However, too many favorable evaluations, with no negative ones, can decrease credibility.
4. The degree of agreement with the decisionmaker's intuitive judgment and beliefs. Along similar lines, the more the approach and results are accepted by the decisionmaker's trusted advisors or analysts, the more credible the evaluation.

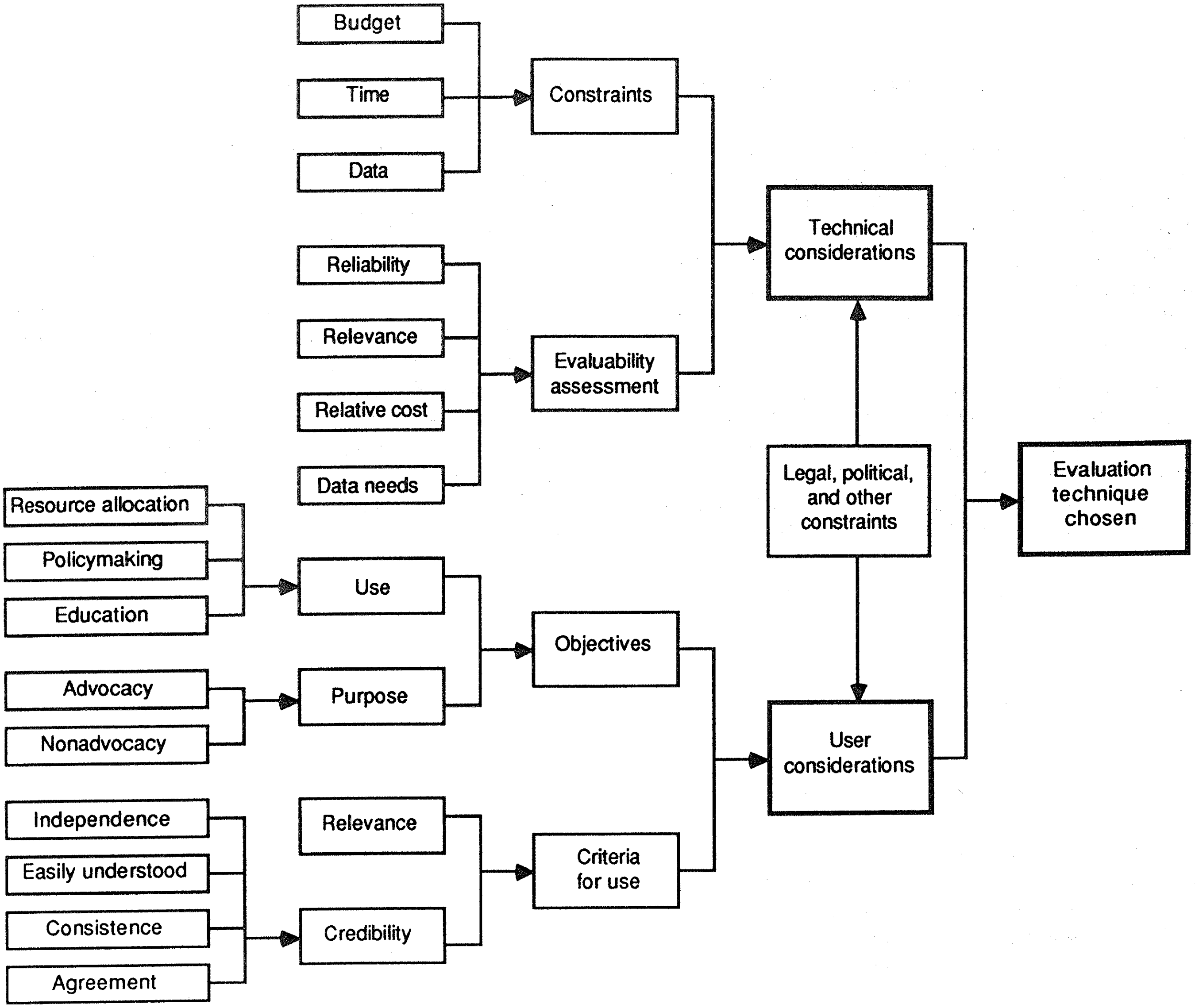


Figure 2.—*Factors affecting the choice of an evaluation approach.*

## Interpreting and Presenting the Results

Results of evaluations are often misinterpreted. Correct interpretations depend on good evaluations—i.e., those which consider the relevant dimensions of the situation. For example, the fact that a number of isolated research evaluations turn up with quite high rates of return to research does not mean that there is a general underinvestment of funds in this field of research. One can infer nothing about overall program funding from a review of a few isolated project evaluations. Further, most evaluations measure a very specific impact of a project along one narrow dimension (e.g., economic efficiency). One has to be careful not to broaden the interpretation to include dimensions not covered by the model.

## RESEARCH EVALUATION: LESSONS FOR FORESTRY

Forestry research evaluators need not operate in a vacuum; there is a large body of literature in agricultural and industrial research evaluation, among other areas, to which they can turn for guidance. By studying examples from other fields, we can assess the potential usefulness of the various approaches or combinations of approaches for forestry research evaluation.

The point about combinations of approaches bears some emphasis. In most forestry research situations, some combination of quantitative and qualitative evaluation tools is needed, especially in research areas not dealing directly with market-priced products. As will be discussed later, 75 percent of agricultural research evaluations deal with a narrow range of commercial field crops. There are relatively few examples of quantitative evaluations that address other types of agricultural research.

This chapter, which covers a wide array of approaches to research evaluation, is arranged in three sections. It begins with a review of agricultural research evaluation, where efforts have focused heavily on methodology. This is followed by a review of industrial research evaluation, where the approach has been much more pragmatic. The final section of the chapter discusses other approaches to research evaluation not readily placed in disciplinary categories.

# AGRICULTURAL RESEARCH EVALUATION


## David N. Bengston[3]


A substantial amount of research has been done on the economic evaluation of agricultural research and extension (R&E). Four major symposia have been held in the United States and one in Canada to discuss various aspects of "research on research" (Fishel 1971, Norton *et al*. 1981, Davidson 1973, Arndt *et al*. 1977). Agricultural research evaluation has provided guidance and focus for research on forestry research evaluation. The primary objective of agricultural research evaluations has been development of methodology or, if we follow Fishel's breakdown, education. Approaches have been developed to evaluate returns to investments in agricultural R&E, to investigate other effects of research, and to efficiently allocate research resources. These techniques have been applied in scores of studies on several types of research at various levels of aggregation. In addition to the many empirical studies, the literature abounds with critiques of existing evaluation methodologies and theoretical investigations aimed at extending or refining current approaches.

The approaches developed to assess the economic impacts of investments in agricultural R&E range from simple scoring models to sophisticated production function analyses. Following Schuh and Tollini (1979), these methodologies are divided into two main categories: *ex post* techniques (which attempt to evaluate past research) and *ex ante* techniques (which attempt to evaluate proposed research projects or programs). The major methodologies that fall into these categories are as follows:

*Ex post*

a. Consumer and producer surplus
b. Production function
c. Allocative efficiency
d. Public relations

*Ex ante*

a. *Ex ante* benefit-cost
b. Simulation models
c. Mathematical programming
d. Scoring models


[3]*At the time of this research, the author was a graduate assistant, Department of Agricultural and Applied Economics, University of Minnesota.*


Each evaluation approach has unique characteristics that determine its suitability for a specific situation. Five of the methods yield rates of return to R&E investments: consumer and producer surplus, allocative efficiency, *ex ante* benefit-cost, production function, and simulation models. Mathematical programming and scoring models are intended to be used in allocating resources. They provide a ranking of projects or determine an optimal mix of research activities. The "public relations" approach (Peterson 1969), while not a formal evaluation technique, is included in this section because of its long history of use.

Other informal research evaluation methods are routinely used by research decisionmakers. For example, the professional reputation of a researcher is a frequently used criterion for allocating research funds at the project level: "Probably the best predictor of the outcome of an individual project is the recent 'track record' of the scientist proposing it" (Peterson 1978). These types of evaluation methods will not be considered here, although they probably figure prominently in research management decisionmaking (Gold 1969). Since the contribution of agricultural research evaluation to forestry has been primarily methodological, we will focus on methods rather than case studies.

## *Ex Post* Methods

*Consumer and Producer Surplus*

Of the agricultural R&E evaluation methods which yield rates of return, the consumer and producer surplus (or index number) approach has the longest history of use. The seminal work of Schultz (1953), Griliches (1958), and Peterson (1966) employed this technique, and it has been widely used and further refined in subsequent studies.

The basic idea in this approach is that the adoption of technological innovations resulting from research reduces the marginal cost of producing the commodity in question, and this in turn increases the amount produced and lowers the market price (fig. 3). In the figure, the shift in aggregate supply

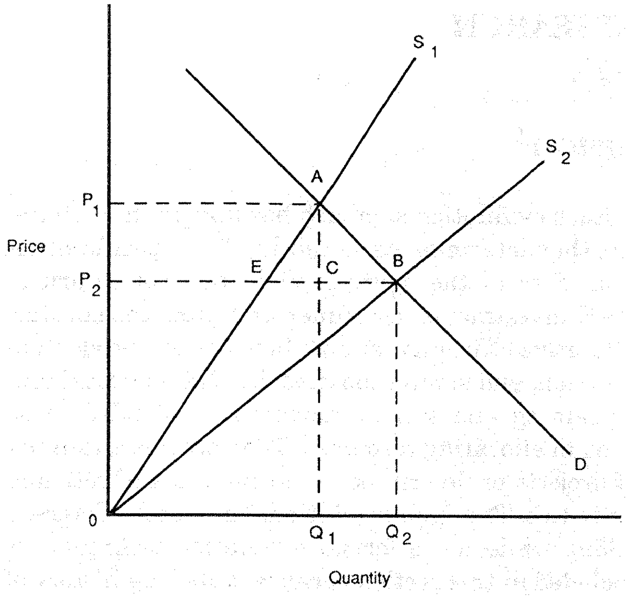


Figure 3.—*Generalized conceptual framework for the evaluation of research benefits using the consumer and producer surplus approach.*

from $S_1$ to $S_2$ causes a change in consumer surplus, represented by the area $P_1$ $ABP_2$. Producer surplus is the area EOB minus $P_1$ $AEP_2$. Producer plus consumer surplus is the total change in economic surplus, which is equal to the area AOB.

Although a good deal of theoretical controversy surrounds the concept of economic surplus, the analysis of consumer and producer surplus by practitioners is widespread.

*Production Function*

The use of the production function technique to analyze the effects of research on agricultural production dates back to early studies by Tang (1963), Griliches (1964), Latimer (1964), and Peterson (1967). The idea underlying this evaluation approach is that research can produce an increase in the quantity of a good produced with a fixed amount of traditional inputs. Another way of saying this is that research causes the production function to shift upward. The contribution of research to productivity growth is assessed by fitting a production function (usually a Cobb-Douglas specification) with research as an independent variable. Regression analysis makes it possible to isolate the effects of research on production, while at the same time controlling for the use of other inputs that have an impact on productivity.

Since research conducted in a given year does not have an immediate impact on productivity, a time lag for the impact of expenditures on output must also be specified. Evenson (1967) has shown that an inverted V-shaped distribution with a mean lag of 6½ to 7 years is most appropriate for production-oriented agricultural research.

Proper specification of the research or the R&E variable is vitally important in this evaluation method. Three main approaches have been used in specifying the research variable: the annual expenditure on research, the level of adoption of an innovation resulting from research, and number of research publications. Griliches (1964) was the first to use the annual expenditure on research, and it has been adopted in most other production function research evaluations. Even though data availability makes this approach appealing, using some measure of research output (which is an input of the production process) would be more correct than using research expenditures (which is an input of the research process). Measuring research output can be problematic in many cases, however, so the assumption is made that research expenditures represent a good proxy for research output.

Another method of specifying the research variable is to use a measure of the adoption of a technological innovation resulting from research. For example, Sim and Araji (1980) used the acreage harvested of wheat varieties bred by the Western Agricultural Experiment Station system as a measure of research output. Specifying the variable in this manner is theoretically more sound, but it limits the evaluation to types of research for which the output is easily measured.

The third specification is to use the number of publications in scientific journals resulting from publicly funded research as a proxy for research output. However, this ignores the question of actual adoption. See Evenson (1974) for an example of this approach.

*Allocative Efficiency*

Allocative efficiency, or the adjustment to disequilibrium approach, has been used by Huffman (1974, 1977) to estimate the value of improved information resulting from agricultural extension. In Huffman's model, extension contributes to allocative efficiency by speeding up the rate of adjustment to disequilibria in the use of nitrogen fertilizer. An adjustment equation is specified in which changes in the use of variable inputs to optimal levels are related to extension:

> The novelty of the model is that the adjustment coefficient is specified as a

vector of environmental variables, including extension. The variable adjustment specification of the dynamic model permits a test of the hypothesis that agricultural extension enhances adjustment to changes in economic incentives, which is one aspect of allocative efficiency. Closing the disequilibrium gap increases farm profit (Huffman 1978).

The allocative efficiency approach is capable of analyzing the adoption of new inputs or technology produced by research, although it apparently has not been used for these purposes. It also holds some promise for evaluating the returns to some types of social science research for which improved information is the main output of the research effort. Data requirements for this evaluation approach are moderately heavy. Huffman's model requires data on anticipated farm input and output prices, fixed and variable input use, and environmental variables affecting input use, including extension.

*Public Relations*

The "public relations" approach (Peterson 1969) has been used more widely than any other research evaluation method. It usually consists of a nontechnical description of what the outcome of a research effort has been, or what the expected outcome will be. This approach is used most often for *ex post* evaluations, but it can also be used for *ex ante*. Some public relations evaluations include quantification of benefits from a research effort, but no attempt is made to determine whether benefits exceeded costs.

Advantages of this approach include its low cost, complete flexibility, and accessibility to the general public. The main disadvantages are that it gives no information to help allocate research resources more efficiently, and it gives a somewhat inaccurate view of the research process by making it appear that producers are the main beneficiaries of agricultural research and that virtually all research is successful. See Fletcher (1937) and Salisbury *et al*. (1973) for examples of the public relations approach.

## *Ex Ante* Methods

Ex Ante *Benefit-cost*

The use of *ex ante* benefit-cost analysis for evaluating agricultural research is a relatively recent development. Fishel (1971) designed a model which calculates benefit-cost ratios and internal rates of return to proposed research projects. This model is "a computer-based, generalized structure for collecting and processing information relevant to resource allocation decisions under situations characterized by a high degree of uncertainty" (Fishel 1971). Other *ex ante* benefit-cost research evaluations have been much simpler, usually focusing on single research projects (Araji and Sparks 1975) or commodity-specific research programs (Araji *et al*. 1978, Barker 1981).

This evaluation method is conceptually analogous to the consumer and producer surplus approach. The main difference is that in an *ex ante* benefit-cost study the effects of research on productivity (and hence on consumer and producer surplus) are estimated and projected into the future rather than being observed from past data. These projections are most frequently obtained from interviews or questionnaires given to research scientists. The scientists are asked to give their subjective judgments about such questions as the expected time requirements for a research project, probability of success, size of benefits, rate of adoption of results, and costs of conducting the research. This information is then used to calculate benefit-cost ratios and internal rates of return for current and prospective research projects.

One obvious criticism of this methodology is that it relies heavily on subjective estimates provided by scientists about the costs and expected success of their research, which would likely be biased upwards. Postmortems of *ex ante* evaluations of industrial research proposals reveal that actual costs and returns often vary considerably from what was predicted (Carter and Williams 1958, Marshall and Meckling 1962, Gold 1964).

Castro and Schuh (1977) avoided this problem by using secondary data to estimate projected impacts and costs of agricultural research in Brazil. This study was also unique in that it concentrated on the growth and distributional effects of research rather than on rates of return.

*Simulation Models*

Simulation models are to the research evaluator what pilot models or experiments are to a natural scientist. An engineer builds a prototype of a machine to see how it behaves. Similarly, a research evaluator constructs a mathematical model to capture the salient features of the research process to assess its behavior.

In a typical evaluation simulation model, changes in productivity are attributed to lagged values of public investments in research and to other factors thought to influence productivity. The coefficients used for the variables in the model usually come from production function studies.

Fishel's computerized *ex ante* benefit-cost model, which could also be considered a simulation model, was the first application of this technique to the evaluation of agricultural research. Other simulation models used for this purpose have only appeared in the past 8 or 9 years (Pinstrup-Andersen and Franklin 1977, Lu *et al*. 1978, White *et al*. 1978).

The same basic criticism leveled at the *ex ante* benefit-cost approach is applicable to the simulation approach. Subjective estimates of variables such as research time requirements, costs, probability of success, and probability of adoption of results must be fed into the model. The validity of the output of a simulation model will depend on the accuracy of these estimates. Additional data requirements of this approach are highly variable, depending on what questions are of interest. If the effects of research on such concerns as the environment, employment, nutrition, economic policy, trade, or income distribution are important, a great deal of additional data must be incorporated into a simulation model.

*Mathematical Programming*

Mathematical programming has been used to a very limited extent in agricultural research evaluation to investigate the optimal allocation of research funds. This approach requires an explicit elicitation of the decisionmaker's goals and preference function, which comes at a relatively high cost in terms of a research administrator's time. Given this information and estimates of expected costs and benefits of the research program or project, the output describes the utility-maximizing allocation of the research budget.

There have been only two attempts to apply this methodology to agricultural research evaluation. Cartwright (1971) designed a model to determine the optimal allocation of resources in agricultural economics research in a university department. This model has never been put into practice and remains in the conceptual stage of development. Russell (1977) developed a mathematical programming model to determine an optimal portfolio of publicly funded agricultural research projects. Russell's model was applied to several research programs in Scotland with satisfactory results.

*Scoring Models*

Scoring models provide a ranking of alternative research projects or problem areas. Shumway defines this approach as one "which formally incorporates the decisionmaker's subjective trade-offs and decision criteria into the model framework" (Shumway 1973). He also provides an excellent review of ranking and scoring approaches used in industrial and agricultural research. This technique is useful primarily as a research management tool: It provides a basis for allocating research resources at various levels.

One of the first applications of the scoring model approach to agricultural and forestry research was done under the aegis of the National Association of State Universities and Land Grant Colleges and the U.S. Department of Agriculture in 1966. This study used a scoring model to arrive at "estimates of socially desirable levels of publicly funded agricultural research in 1972 and 1977 in each of 91 problem areas" (Williamson 1971). The research classification system that was developed in this study is now being used in the Current Research Information System (CRIS) of the U.S. Department of Agriculture.

Other scoring models have been developed to evaluate agricultural research, each addressing slightly different aspects of the problems involved in selecting an optimal research portfolio (Mahlstede 1971, Shumway and McCraken 1975). Some use simple procedures, while others use more complicated techniques to determine rankings, such as the Delphi approach. Data requirements are therefore highly variable, ranging from individual qualitative assessments of the expected benefits and costs of alternative projects to extensive group-determined estimates of project costs, benefits, weights attached to various research goals, etc. Most of the models require large inputs of expensive labor (scientists are usually the evaluators of the research projects). Scoring models are subject to the same criticism as all *ex ante* approaches: The biased opinions of people with a vested interest in the outcome are central to the evaluation.

## Limitations of Agricultural Research Evaluation[4]

The sheer number of agricultural research evaluations is impressive—I have identified 85 studies

[4]*For a more detailed discussion of the topic see Bengston (1985).*

(Bengston 1985b). Despite the large number, two evaluation techniques have been overwhelmingly favored by researchers—the consumer and producer surplus and production function approaches account for nearly three-quarters of all evaluations.

The preponderance of these two approaches is due to several factors. First, since these methods were used in the first economic evaluations of agricultural research, they have undergone more development and refinement than other techniques. Second, both of these approaches are based on theoretical foundations and methods of economic analysis that have been applied (if not universally accepted) for a long time. Finally, there is a common perception among research evaluators that the information generated by these approaches is robust (Arndt and Ruttan 1977) and the type of information produced is relevant to research decisionmakers.

In agricultural research evaluation, crop production research is by far the dominant category. Animal production research has received very little coverage, even though significant shares of State Agricultural Experiment Station research budgets are oriented towards livestock. Most studies not related to production evaluate returns to agricultural extension, not research.

Why is it that research evaluators have focused on production-oriented research so heavily? A main reason is that the outputs are easier to measure than those from other types of research. For example, research results that produce higher product yields are more easily measured in economic terms than research results in the form of information, a main output of social science research.

Evaluation of research on most major agricultural commodities has been reasonably complete, while coverage of less important products has been scattered. The small number of evaluations of mechanical innovations is somewhat surprising in light of the key role that advances in agricultural mechanization have played in the productivity growth of American agriculture (Hayami and Ruttan 1971). A likely explanation is that the bulk of research on mechanical agricultural technology takes place in the private sector, and the evaluation literature is almost exclusively concerned with public sector research.

There has been only one evaluation of a "service product" other than the returns to extension studies. Hayami and Peterson (1972) examined the returns to public information services—the statistical reporting of agricultural commodities. Several evaluations of research on intermediate agricultural production processes have been carried out. For example, Araji and Sparks (1975) looked at potato storage research, and Duncan (1972) evaluated pasture improvement research.

Finally, the extent of coverage of agricultural research evaluations may be judged by the objectives of the research that has been evaluated. If we accept that the overall goal of all applied research is improvement in human welfare, this can be broken down into four basic objectives:

1. To improve economic *efficiency in production*—i.e., produce the same output with fewer resources, or more output with given resources.
2. To improve *efficiency in consumption*—i.e., use something longer or get the same utility by consuming less of some good or service, etc.
3. To improve the *quality* of goods or services in final consumption.
4. To "improve" the *distribution of benefits* from consumption or the *distribution of costs* of production.

It should be stressed that improvement in quality as a goal of research must be determined at the final consumption level to be meaningful. Without this distinction the difference between improvement in efficiency of production and improvement in quality becomes blurred.

Using these four objectives to classify the agricultural research that has been covered in the evaluation literature, it is clear that efficiency in production is the primary or sole objective of nearly all the research evaluated. This is not particularly surprising, since crop and animal production have been the dominant types of research looked at. But even among the nonproduction studies there have been very few evaluations of research with objectives other than efficiency in production. This is undoubtedly due to the difficult methodological problems encountered in evaluating certain types of research.

The body of literature employing the techniques of agricultural research evaluation is large. For a variety of reasons, application of these techniques has centered on crop production research, and the consumer surplus and production function approaches have been heavily favored. In light of the experience in agriculture, application of existing evaluation methods to forestry research will likely be governed by two main factors: data availability and problems in quantifying the value of forestry research outputs.

Data problems are endemic to the process of evaluating research from an economic perspective. But difficulties in obtaining the data required to evaluate forestry research will be greater than in the case of agriculture. Data on farm inputs, acres of individual crops harvested, etc. are routinely collected and reported in various State and Federal publications in the U.S. In addition, fairly reliable estimates of economic parameters such as the price elasticities of supply and demand are available for many agricultural commodities. Because this is not the case in forestry, evaluation techniques with heavy data requirements, such as the production function and allocative efficiency approaches, are unlikely to find widespread applicability.

Evaluation methods with relatively low data requirements are more promising in forestry. These include the consumer surplus approach and the *ex ante* benefit-cost and scoring model approaches. Simulation models are also a possibility, since data requirements vary considerably depending on the exact nature of the model that is developed. Other possibilities include hybrid models incorporating elements of existing evaluation methods designed to address the unique aspects of forestry research.

The application of economic evaluation methods will likely be limited for the near future to forestry research that produces outputs easily measured in economic or monetary terms. This is the case in the agricultural literature, where studies of production-oriented research dominate and studies of research results involving nonmarket, nonpriced outputs are almost nonexistent. The forestry literature to date has concentrated on forest products and utilization research, where research outputs are more easily quantified economically than in other areas.

Some economists are working on ways to evaluate other types of research, such as social science research (Norton and Schuh 1981), but these efforts currently remain in the conceptual stage. Until basic advances are made in the tools available to measure the economic impacts of research, forestry research evaluation is likely to concentrate on research that increases technical efficiency in production, such as product utilization and genetic improvement research. Research that results in increased allocative efficiency—such as forest management research—might also be amenable to economic evaluation. In conclusion, an understanding of what has been done in the evaluation of agricultural research and a clear recognition of its limitations are essential prerequisites to finding appropriate and usable evaluation methods for forestry research.

# INDUSTRIAL RESEARCH EVALUATION

**Jim Olmstead**[5]

While agricultural research evaluations have focused both on methodology and its application, industrial research evaluations have been confined primarily to studies of methodology, with few case studies or examples of its application. Also, the methodology developed strongly reflects the real-world constraints of conducting research within organizations trying to exist in a competitive market economy.

American firms spend billions of dollars each year on research and development (R&D). Rettig *et al.* (1975) explains that firms are willing to make this investment because R&D helps meet the firms' objectives of:

1. Providing a steady if not increasing flow of profits.
2. Maintaining competitive positions in present areas of business activity.
3. Improving existing market positions.
4. Entering new markets.
5. Ensuring continued existence of the corporate entity.

R&D accomplishes this by providing:

1. Useful outputs such as process improvements, improvements in the quality of existing products, and new products.
2. Staff that can aid other groups within the firm and serve as the firm's technological intelligence unit.
3. A progressive image for the firm.

Although the relationships between R&D and the firm are easily understood, conceptualizing the relationships in a manner conducive to analysis and planning is an elusive task. As an article in a recent *Harvard Business Review* states:

> "One of the most difficult corporate functions to analyze and assess is research and development. Partly for this reason, few corporate relationships are more consistently troubled than that between R&D and general management . . . the problem is clearly in communication . . . one reason is a failure on the part of general management and research management to agree on what the R&D process is" (Merten and Ryv 1983).

The bottom line for research and development can only be determined within the context of the firm. The results of R&D, in and of themselves, are unlikely to earn much for the firm; rather, profits occur when an innovation is transferred from the laboratory to the marketplace. This activity involves a host of actors and actions. The process is well illustrated by Merrifield (1976) with his concept of the "R&D pipeline"—costs for bringing an innovation to market are incurred at various locations in the firm, not exclusively in the research and development laboratory. Horesh and Kamin (1983) support this concept with their finding that the cost of R&D is only approximately 50 percent of the cost of bringing an innovation to the market.

Mansfield (1982) has determined that on average the probability of an R&D project resulting in an economically successful product or process is 12 percent. This probability takes into account the potential technical and commercial risks. The likelihood of a project being a technical success from the laboratory standpoint is much higher—one survey puts it at 50 percent. However, the commercial risk (the possibility that a new product or process will not merit commercial introduction or application, or that if it does it will not be an economic success) is often much higher.

The preceding discussion has shown that the bottom line for R&D depends as much or more on commercial considerations as technical considerations, and that the actual R&D costs are but a part of the total costs of a marketable innovation. For these reasons the financial evaluation of R&D begins to take the shape of venture analysis. According to Cangemi and Weil (1982), R&D management is moving toward a more systematic, holistic approach, with R&D considered in the context of the firm.

[5]*At the time of this research, the author was a graduate research assistant, Department of Forest Resources, College of Forestry, University of Minnesota.*

## Evaluation in Theory

The literature on industrial R&D evaluation deals overwhelmingly with techniques in theory, as opposed to techniques in practice. The techniques, which concentrate almost exclusively on project or problem selection, include front-end analysis, formative evaluations, impact evaluations, and program monitoring.

Hundreds of methods for selecting R&D projects have been put forth in the past 30 years or so. As a result, literature reviews have been numerous; some examples include Baker and Pound (1964), Cetron *et al.* (1967), Moore and Baker (1969), Souder (1972a, 1972b), Augood (1973), Baker (1974), Clarke (1974), Albala (1975), Baker and Freeland (1975), Rettig *et al.* (1975), and Gibson (1980).

As might be expected, there is little agreement among reviewers on how to categorize the many methods of project selection. One format, moving from less to more complex, is as follows:

1. Checklists and profiles.
2. Ranking methods.
3. Scoring methods.
4. Profitability indices.
5. Optimization models.
6. Risk and decision analysis.
7. Staged approaches.

*Checklists*

In a checklist model, projects are subjectively rated against a simple list of criteria chosen for their significance in achieving project success. The rating of each criterion is indicated by a descriptor such as acceptable or unacceptable. Profiles are basically the same as checklists except that the results are displayed graphically.

Although checklists and profiles are simple, problems may arise in the selection of decision criteria (Augood 1973). Nevertheless, checklists and profiles have achieved some acceptance in the chemical process industry, where users feel that such methods introduce a desired level of formality without demanding more data and specification than are readily available (Souder 1978).

*Rankings*

In a ranking model, the decisionmaker simply compares projects with one another (Clarke 1974). An example is the Q-sort method, described by Souder (1978) as the simplest and most effective process for ranking a set of R&D projects. The method includes the following steps:

1. Each decisionmaker is given a deck of cards each bearing the name of one project. Cards are sorted according to some specified criterion.
2. The deck is then divided into two piles; one high level, one low level, based on the criterion.
3. Cards from each of the two piles are selected to form a medium-level pile. There are now three piles.
4. Cards from the high-level pile and low-level pile are next divided into two piles each: very high and high, and very low and low. There are now five piles.
5. Finally, the selections are surveyed and rearranged if needed.

Gibson (1980) identifies one disadvantage of ranking models as that they do not require decisionmakers to reveal the bases of their judgments. Experienced managers, however, often feel that they "know" more about their areas of responsibility than they can articulate. Critics also argue that ranking models do not exclude irrelevant and emotional items and do not force considerations of all rational factors. However, managers skilled in management politics do not see this as a problem. Finally, Gibson notes that the most telling argument against ranking models is the lack of reproducibility from manager to manager. Thus, rankings are very personal and could hinder rather than help the decisionmaking process.

*Scoring Methods*

Scoring methods (also referred to as rating methods) compute an overall project score based on ratings assigned to relevant decision criteria for each project. The higher the score, the more desirable the project. Albala (1975) points out that scoring methods may differ in: (1) the degree of detail or number of decision criteria, (2) whether the numerical values assigned are weighted or unweighted, and (3) whether the numerical values are based on qualitative descriptors (e.g., outstanding = 0.1, very good = 0.8), quantitative descriptors (e.g., annual sales potential less than $25,000 = 1, between $25,000 and $100,000 = 2, etc.), or both.

Scoring methods force both criteria and weightings into the open. Gibson (1980) feels scoring methods are superior to ranking methods, but points out that managers may try to beat the system by writing proposals to fit the scoring methods. Analysts may attempt to thwart such attempts by designing more complex systems, but the merit of simplicity is then lost.

*Profitability Indices*

Many methods center on economic or profitability criteria. In general, such indices try to consolidate into a single value the various quantitative elements normally found in conventional economic evaluations. Most of the methods use probability ratios (expected profits to expected costs). They may also include probability of success or risk factors, and costs may be limited to R&D expenses or include full commercialization costs. One of the earliest and often-cited economic indices is that of Olsen (1955):

$$\text{Value of new project} = (\text{IR})(\text{Pt})$$

where:

IR = Value of process savings for 1 year, or 3 percent of new product sales for 5 years, or 2 percent of sales value of improved products for 2 years, depending on the criterion of the organization, and

Pt = Probability of technical success.

*Optimization Models*

Constrained optimization models seek to optimize some economic function subject to specified resource constraints. They employ some form of mathematical programming, such as linear, nonlinear, integer, or dynamic. Souder (1972a) has presented a bibliography of many such models. Although there are a number of optimization models available, there is no evidence that they have been used by managers (Gibson 1980).

*Risk Analysis*

Risk and decision analysis models are based on the concept that rational decisionmakers will select those projects having the highest expected value scores (Souder 1978). It requires that each possible outcome, along with its probability and value to the decisionmaker, be established in advance. Risk analysis is typically based on a simulation analysis of input data in distribution form and provides output distributions of such factors as rate of return and market share (Moore and Baker 1969) (fig. 4).

The most likely level of lifetime profits for Project 2 is greater than for Project 1 ($150 million vs. $100 million); so some managers would select Project 2 without further analysis. However, if a manager considers the risks inherent in both projects, she/he may make a different decision. Project 2 has a 0.10 probability of earning no profits and only a 0.40 probability of earning its most likely level, while Project 1 will always earn some profit and has a 0.80 probability of earning its most likely level. While the gambler may stick with the original selection of Project 2, the risk averter would be inclined to choose Project 1.

Decision theory models are attractive because they help to clarify the available strategies and the potential risks, regrets, and tradeoffs, and they are based, at least in part, on a management or policy oriented thought process. There is little evidence that they are put into practice.

*Staged Approach*

At least three authors (Albala 1975, Souder 1978, Gibson 1980) have advocated an eclectic approach to project selection which attempts to match the selection tool to the characteristics of the decision situation at hand. Known as a staged approach, it is based on two key considerations: the "real-world" aspects of decisionmaking and the changing nature of risks and costs as projects move closer and closer to commercialization. Souder (1978) has contrasted the "real-world" with the idealized view of decisionmaking. (See next page.)

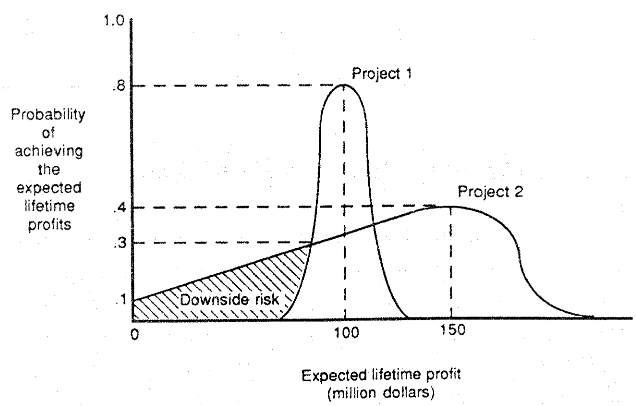


Figure 4.—*An example of risk analysis (Souder 1978).*

| Idealized World | Real World |
|---|---|
| 1. A single decisionmaker, in a well-behaved environment. | 1. Many decisionmakers and many decision influencers, in an dynamic organizational environment. |
| 2. Perfect information about candidate projects and their characteristics; outputs, values and risks of candidates known and quantifiable. | 2. Imperfect information about candidate projects and their characteristics; outputs and values of projects are difficult to specify; uncertainty accompanies all estimates. |
| 3. Well-known, invariant goals. | 3. Ever-changing, fuzzy goals. |
| 4. Decisionmaking information is concentrated in the hands of the decisionmaker, so that he has all the information he needs to make a decision. | 4. Decisionmaking information is highly splintered and scattered piecemeal throughout the organiziation having all the information needed for decisionmaking. |
| 5. The decisionmaker is able to articulate all consequences. | 5. The decisionmaker is often unable or unwilling to state outcomes and consequences. |
| 6. Candidate projects are viewed as independent entities, to be individually evaluated on their own merits. | 6. Candidate projects are often technically and eoncomically interdependent. |
| 7. A single objective, usually expected value maximization or profit maximization, is assumed and the constraints are primarily budgetary in nature. | 7. There are sometimes conflicting multiple objectives and multiple constraints, and these are often noneconomic in nature. |
| 8. The best portfolio of projects is determined on economic grounds. | 8. Satisfactory portfolios may posses many noneconomic characteristics. |
| 9. The budget is "optimized" in a single decision. | 9. An iterative, recycling budget determination process is used. |
| 10. One single, economically "best," overall decision is sought. | 10. What seems to be the "best" decision for the total organization may not be seen as best by each department or party, so that many conflicts may arise. |

## Evaluation in Practice

Compared with the literature on theories or models for project selection, the literature on the practice of project selection is scanty. Dean (1968) and Gee (1971) have reported on surveys of project selection practices in the United States, and Allen (1970) on practices in Great Britain. Additional information can be found in occasional reports on practices employed at individual firms, such as Wolff's (1980) study of project selection at Uniroyal. Further, scattered comments appear in the R&D management literature.

In writing about selection practices, researchers have focused more on criteria and people involved than on analytic tools; in fact, descriptions of methods are almost nonexistent. Porter (1978) found that the same methods used for project selection are used for postevaluation. The methods involve calculating a probable returns ratio of profits to R&D costs. Uniroyal uses a scoring model that considers the probabilities of technical and commercial success and the organizational resources required to commercialize the project (Wolff 1980). Finally, the British survey mentioned earlier reported that of their 112 respondents (Allen 1970):

- Ninety-six used net present value, payback period, or average rate of return on capital invested when assessing the commercial viability of projects.
- Ninety-nine estimated the probability of commercial success, technical feasibility, and probable development costs as part of their project evaluation procedure.

- One-third used either mathematical models, weighted checklists, or project ranking procedures as part of their project assessment procedure.
- Twelve had a numerical weighting system on their standard evaluation forms which could be used to form a picture of the weak and strong points of a project by drawing a profile line connecting the weights given to the project attributes.

The previous discussion has dealt almost exclusively with project selection. There is a small body of literature dealing with postevaluations, which are conducted after the prime responsibility for a project has moved from the research group to other parts of the organization. Postevaluation attempts to determine how well the research organization is doing in contributing to the company's commercial objectives (Collier and Gee 1973).

According to Porter (1978), Mobil Oil's research division sees five major benefits to postevaluations of R&D: (1) motivating researchers, (2) identifying high risk business objectives, (3) demonstrating research productivity, (4) identifying productive research areas, and (5) increasing confidence in predictive evaluations.

Collier and Gee (1973) endorse postevaluations because they believe that without a valid method for evaluating R&D, it is difficult to know objectively if other procedures for improving the research process are really effective—in their words "It's difficult to determine the effort of the independent variables unless you can measure the dependent variable."

Although relatively little has been written about evaluating the consequences of industrial R&D (Patterson 1983), there are descriptions in the literature of how some companies evaluate research effectiveness. At Mobil, research return ratio (RRR) is used to predict the value of research (Porter 1978). The RRR compares the value of accomplishing a research objective with the cost of doing it. Alcoa has used an indicator called the economic benefit ratio to evaluate R&D on a quarterly basis (Patterson 1983). Laboratory performance is measured by using the ratio of accumulated present values for accomplishments maturing during the quarter to laboratory expenditures for that quarter. Alcoa has identified several benefits from this evaluation effort:

1. It helps to legitimize R&D in financial rather than technical terms.
2. It provides feedback to decisionmakers.
3. It stimulates researchers to pursue research with financial benefit to the company.
4. It makes researchers more aware of the importance of implementation of results, and gets them more involved in the process.
5. It points out where most of the benefits are coming from, thereby helping management determine where to concentrate attention.
6. It creates a more favorable climate for longer range work and funding by demonstrating a mastery of near-term technology.
7. It improves project selection criteria.

Currently, Alcoa sees three major drawbacks to the effort:

1. Sales projections are difficult to make.
2. The synergistic effect of accomplishments often defies accurate separation of benefits.
3. Capital costs of implementation have been ignored.

Collier (1977) has described Borg-Warner Corporation's first 3 years of experience with a two-step R&D evaluation procedure which seeks to determine how well the research department is creating technologically based business opportunities for the company. Step one evaluates how well research has met previously agreed-upon objectives. Step two evaluates the potential value of completed research projects. The interesting feature of this approach is that it estimates the total financial gain represented by research accomplishments. How much of this gain the firm might realize depends on others in the firm.

## Limitations of Industrial Research Evaluation

Regarding evaluation of industrial R&D in theory, Clarke (1974) notes, "Obligingly, the same people who design the models also tell us why they are not used." One comprehensive critique, (Baker 1974) identifies seven limitations to using the project selection techniques discussed above:

1. Inadequate treatment of project and parameter interrelations with respect to both benefit contribution and to resource utilization.
2. Inadequate treatment of uncertainty as it impacts on benefit measurement and parameter estimation.
3. Inadequate treatment of multiple, interrelated decision criteria which have no common, natural underlying measure.
4. Inadequate treatment of the time and variant property of the parameters and criteria and the

associated problem of continuity in the research program and staff.

5. A restricted view of the problem which (a) portrays a once-a-year investment decision rather than an intermittent stream of investment alternatives, (b) does not include such attributes as timing of the decision, generation of additional alternatives, and recycling, (c) does not recognize the diversity of projects along the spectrum from basic research to engineering, and (d) views the problem as a decision event rather than as a hierarchical, diffuse decision process.
6. No explicit recognition of the importance of individual R&D personnel.
7. The inability to establish and maintain balance in the R&D program.

Mansfield (1982) adds that many of the models fail to recognize that R&D is essentially a process of buying information. Even unsuccessful projects can provide valuable information; therefore, the real task is to allow for a series of decisions under uncertainty. He goes on to point out that project selection often rests on overly optimistic, unreliable estimates of benefits and costs that reflect both the uncertainty of the undertaking and the desire to "sell" projects to top management.

Although there are few descriptions in the literature on how firms evaluate their project selection processes, generalizations can be made. Quantitative evaluations are done most frequently at the development stage of R&D, for two primary reasons: First, developmental work accounts for the major share of industrial investment in R&D. Second, because development is closer to the commercialization stage of innovation, the risk and cost attributes are easier to estimate. Reliable estimates of both risk and cost are keys to quantitative evaluation. At the exploratory research stage, evaluation consists of comparing proposal attributes with recognized criteria. Almost all evaluations assess benefits from the firm's innovations rather than R&D per se. For this reason, the market looms large in the method described.

# OTHER APPROACHES TO RESEARCH EVALUATION

**Robert A. Westgate and David N. Bengston**[6]

There are two other approaches to research evaluation that do not fit neatly into our discussions of agricultural or industrial research evaluation, but are relevant to the topic. These are the peer review approach used by the Commission of the European Communities, and technology assessment.

## Peer Review—Commission of the European Communities (CEC)

The CEC is involved with a variety of projects dealing with European cooperation. It became seriously interested in formal evaluation of research during the early or mid-1970's. Although CEC research does not deal directly with forestry, it is involved in a number of related fields, including energy, environment, raw materials, and agriculture.

In June of 1978, the CEC organized a seminar on the evaluation of R&D (CEC 1979a, 1979b). Research evaluation approaches used in different sectors and in different countries were reviewed and discussed at that time. Based on discussion at the seminar, the CEC developed an evaluation strategy and approach for its projects. This evaluation system was tested on a number of cases, with the results reviewed and assessed at a second conference (Boggio and Gallimore 1982).

The heart of the CEC approach is the traditional peer review model. Within this approach, the CEC recognizes two types of evaluations: (1) internal evaluations, conducted during research program execution, and (2) external independent evaluations, conducted following program completion.

The CEC evaluations attempt: (1) to assess scientific and technical quality of the research, (2) to evaluate the effectiveness of program management, and (3) to determine the practical contributions of program results. Evaluation teams consist of three to six experts, chosen for their expertise and independence concerning the program being evaluated. Evaluations are divided into four phases: (1) orientation of panel members to the research program being evaluated; (2) reaching agreement on evaluation methods, organization, and criteria; (3) actual evaluation of research quality and impacts; (4) final report and recommendations. During the actual evaluation, panel members review contract proposals, study plans, progress reports, final reports, related publications, meeting minutes, seminar proceedings, statistical data, and other materials supplied by researchers and administrators. Analysis is supplemented by interviews and results of questionnaires sent to contractors and, in some cases, users of results.

In analyzing its process, the CEC found that the peer evaluation approach has been generally well accepted, reconfirming the need for independent, outside evaluation (Contzen *et al.* 1982). The choice of experts is critical, as is timing. In the case of an ongoing research program that involves phases, the evaluation should be done early enough to allow input into extension or revised project decisions. Efforts should be made to strengthen the quantitative aspects of the evaluation process. More effort needs to be devoted to evaluating the socioeconomic impacts of research.

The research evaluation approach of the CEC is still evolving. As stated by the Director General for Science, Research and Development of the CEC: "We are in a continuing learning process" (Fasella 1982). He further states:

> We are convinced, in fact, that "peer review" is extremely valid for establishing the scientific and technical merit of the results, for assessing the efficiency of the management and, of major importance, for comparing the results with the original objectives of

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[6]*While working on this study, the authors were graduate research assistants, Department of Agricultural and Applied Economics, University of Minnesota.*

> the programme. However, as we proceed from "review" to "evaluation," i.e., we try to establish the "value," the "usefulness" of our results, the opinion of "peers" is no longer sufficient and must be integrated with what could be called appreciation by "users" (Fasella 1982).

The CEC approach is extremely practical and provides some very useful evaluation information. However, in and of itself it does not provide sufficient information by which to judge the value of research. Some evaluation of impacts based on response of users is needed as well. In the case of market-priced outputs, such response shows up in market prices and sales. In the case of research involving nonmarket goods and services, or intermediate technical processes, some other indication of user valuation is needed. Further discussion of variations on the peer review process can be found in Salasin *et al*. (1980), Kennedy (1977), Cleland *et al*. (1982), and Knoppers (1979).

## Technology Assessment (TA)

Technology assessment may be defined as policy analysis designed to systematically explore the impacts on society that may accompany the introduction of new technology. The underlying idea is that technology has social consequences which need to be evaluated, and that public policy decisions are based on many evaluation criteria. For discussions of TA methodology, see Coates (1974), Porter *et al*. (1980), Armstrong and Harman (1980), and Lee and Bereano (1981).

In contrast to most forms of policy analysis, TA has been advanced by policymakers and analysts in Federal government (O'Brien and Marchand 1982). Congressman Emilio Daddario introduced the idea of technology assessment with his proposal in 1966 to create a congressional staff that would monitor technological developments for government policymakers (Daddario 1970). The Office of Technology Assessment (OTA) was established by Congress in 1972 and remains the major producer of TA's today. Technology assessment in the private sector has been quite limited (Coates and O'Brien 1982).

Several problems often limit the effectiveness of TA. The first relates to the timeliness of information; because decisionmakers often require information quickly, and because TA is a time-consuming process, decisionmakers' need for information often exceeds analysts' ability to provide it (O'Brien and Marchand 1982). Another limitation is the inherent tradeoff between the broad scope of a TA and the depth to which the technology can be analyzed (Sundquist *et al*. 1982a, 1982b). A broad assessment may produce a great deal of information about potential impacts of an emerging technology, but may not be sufficiently detailed about key decision criteria to be useful to policymakers. The danger is that technology assessments without carefully delineated objectives and boundaries can end up being a mile wide and an inch deep.

Finally, the political environment in which TA takes place has the potential for limiting its effectiveness in decisionmaking. The control of technology involves sensitive issues that touch many interest groups. Some have asked whether TA's do indeed contribute to consensus building or whether they merely fuel political conflicts. O'Brien and Marchand (1982) have commented:

> Assessment of technologies thus is actually extremely fragmented, inexorably incremental, largely uncoordinated, and contingent on the dynamic political pressures of multiple institutions and organizations in the public and private sectors.

Yet, technology assessment, with its flexible methodology, has the potential to provide research policymakers with the types of information they require. In designing studies, care should be taken to avoid the pitfalls discussed by Martino and Lenz (1977) and Berg *et al*. (1978).

Technology assessments have the greatest potential impact on research decisions made at a fairly aggregate level. Individual research projects or small programs are probably inappropriate for this type of analysis, because they typically have more narrow objectives than those addressed by TA. In addition, the resources needed to carry out a TA are likely to be prohibitive at this level.

The use of TA in setting research priorities has received some attention in the literature—e.g., evaluating research results and improving allocation of research funds (O'Brien and Marchand 1982, Coates 1982)—and its application to research management has been discussed (Koppel 1978, 1979). The application of TA to research decisionmaking, however, has been limited, making it an important area for further research.

# FORESTRY RESEARCH EVALUATIONS


## Hans Gregersen and Christopher D. Risbrudt[7]


In arguing for an annual allocation of $40,000 for testing the physical characteristics of various species of timber in 1893, Bernard Fernow remarked:

> The economy secured to the country at large by this work may best be comprehended from the statement that by the simple demonstration of the value of "bled" timber for building purposes, an increase in the price of the product of nearly 1,000,000 acres in the Southern States has been secured, involving in the assumed appreciation of at least 2,000,000,000 feet, B.M., of lumber, which, if appreciated by only $1 per 1,000 feet, represents a savings of $2,000,000 in value (USDA 1893).

Thus reads a report on one of the earliest forestry research evaluations. Through the years, similar statements regarding the value of forestry research have been made. Although many have not been thought of as "research evaluations," they fit the term in all respects.

For this study we reviewed some 60 forestry "research evaluation" documents, choosing examples to illustrate the current state of the art. Let's begin by looking at some examples of forestry program evaluation. Program evaluation is closely related to research evaluation, and methodologies often overlap. Although the field of forestry research evaluation is relatively young, there is a tradition in forestry of program evaluation. One of the first examples of the latter was Lundgren and King's (1966) evaluation of financial returns from forestry tree improvement programs. In this evaluation, Lundgren and King calculated internal rates of return for long-term and short-term tree improvement programs for red pine and jack pine. They were interested in answering these questions: How much genetic improvement is needed to justify the program test approach? How does one determine which of the species could be improved at the greatest profit? How much return can be expected in tree improvement programs?

In another program evaluation, Risbrudt and Kaiser (1981) evaluated the U.S. Department of Agriculture, Forest Service sawmill improvement program using a cost-benefit approach to determine the relationship between the cost of the program and the benefits resulting from higher yields or improved efficiency in sawmills.

Evaluations of the USDA Combined Forest Pest Research and Development Programs bridge the gap between program evaluation and research evaluation (Cleland *et al.* 1979, Cleland *et al.* 1982, Rose 1983). In the primary evaluation (Cleland *et al.* 1979), three separate approaches were used and then combined in an overall evaluation. The first approach was a benefit-cost evaluation, which provided the evaluators with a basic measure of economic efficiency. The second was a clientele assessment survey, which queried knowledgeable individuals on the effectiveness of the Combined Forest Pest Research and Development Program. The third was an "acceleration evaluation," in which an expert panel compared the expected accomplishments under accelerated effort with those under normal or base-level funding. The third approach is a traditional "with and without," or incremental, evaluation.

An evaluation conducted by the General Accounting Office (1972) was one of the first initiated outside the forestry community to focus specifically on the impacts of forestry research. Evaluators interviewed national forest managers to determine the extent to which they applied 10 innovations resulting from Forest Service research. The evaluators found very little use of the innovations in the field.

In a second study of forestry research initiated outside the forestry community, the U.S. Department of Agriculture, in cooperation with the Office of Management and Budget, interviewed the users and beneficiaries of 15 Forest Service research projects to ascertain whether the research being undertaken was appropriate for the Federal govern-

[7] *At the time this chapter was being prepared, the author was a project leader, U.S. Department of Agriculture, Forest Service, North Central Forest Experiment Station.*

ment (USDA 1982). The strongly stated conclusion was that the Forest Service research was appropriate.

Early research evaluations initiated within the forestry community were used primarily for long-range planning and policymaking. Methods ranged from quantitative models providing information for resource allocation decisions (Bethune and Hargreaves 1968, Bethune and Clutter 1969) to survey assessments (Fedkiw 1981, Callaham 1981) and descriptions of how a process works (Kennedy 1977, Moeller and Shafer 1981).

In the early 1980's, political and economic changes helped generate increased interest in forestry research evaluation in the U.S. Department of Agriculture and at universities across the Nation. This resulted in a series of studies aimed at developing quantitative methods to evaluate forestry research. Although many of these studies have focused on research methodology, they have also been used in resource allocation or policymaking.

Gregersen *et al*. (1983b) analyzed the impacts of seven major innovations in forest products utilization research. For this study, the evaluators used an *ex ante* benefit-cost approach. The study methods are outlined in figure 5. Even with a very conservative assessment of benefits and an extremely liberal estimate of costs, the calculated rate of return on the investment in forest products utilization research was on the order of 20 percent.

Using an economic surplus approach, Bengston (1984) evaluated returns to structural particleboard research. Benefits were measured only in terms of savings to consumers, ensuring a conservative estimate. Public and private research costs were estimated using industry information, publication counts, publications per scientist, and estimates of costs per scientist. Average internal rates of return ranged from 19 to 22 percent. A sensitivity analysis indicated the results were insensitive to estimates of research costs. Marginal rates of return were from 27 to 35 percent, suggesting that further investment in this type of research would produce attractive rates of return.

Using an approach similar to Bengston's, Westgate (1985) evaluated the impacts of containerized forest tree seedling research. He calculated internal rates of return ranging from 37 to 111 percent, depending on (1) the quantity of containerized seedlings produced in the future, (2) estimates of research costs, and (3) the price discount of containerized seedlings. The internal rate of return was very sensitive to assumptions made for the latter two factors.

Seldon (1985) developed a nonresidual supply function approach to estimate returns to southern

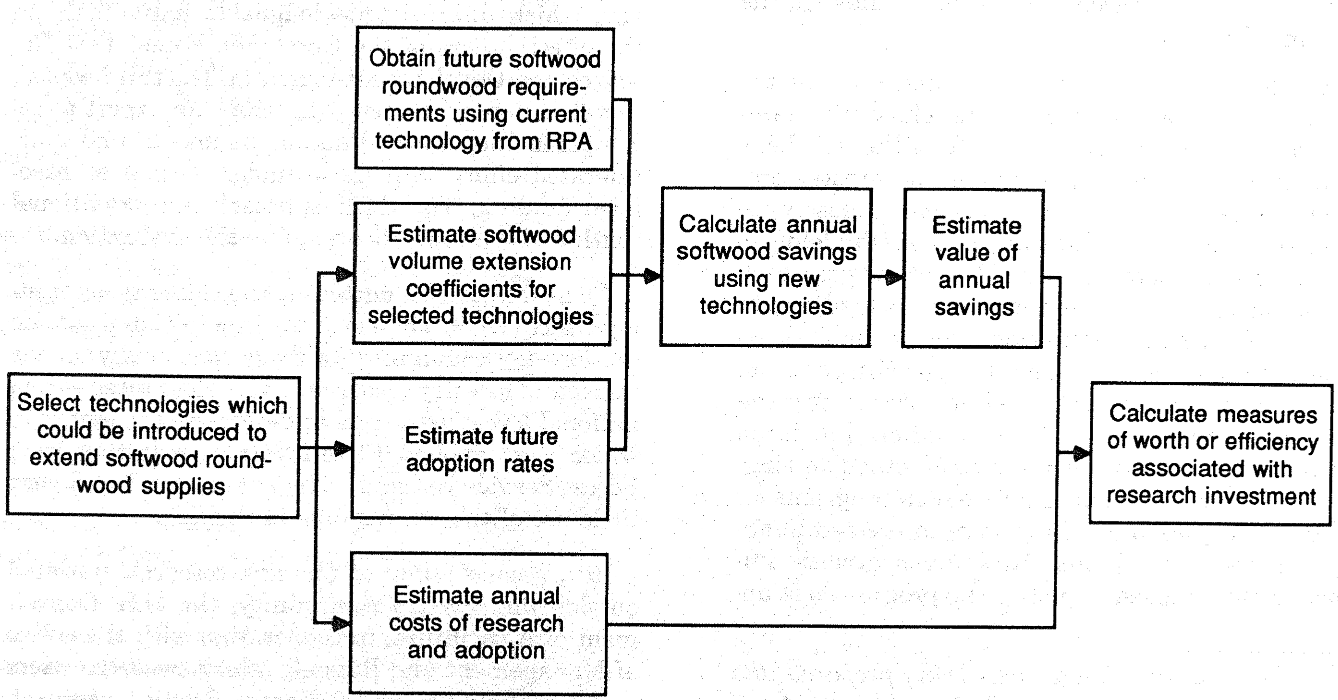


Figure 5.—*Evaluation of utilization research.*

softwood plywood research. He explained the high internal rate of return—422 percent—by the fact that public softwood research was applied research quickly adopted by softwood plywood producers.

In a second evaluation by Bengston (1985a), the rate of return to U.S. lumber and wood products research from 1942 to 1973 was estimated to be 40 percent. After adjusting for interindustry technology flows (allowing for innovations developed in other industries but used to increase productivity in the lumber and wood products industries), the rate of return was calculated at 34 percent. Bengston also adjusted for changes (such as quality) in intermediate inputs, a factor previously ignored.

Bare (1985) evaluated the Regional Forest Nutrition Research Project administered by the University of Washington's College of Forest Resources, and found real after-tax return rates of 9.3 to 12.1 percent. The range in values was caused by differing assumptions concerning the percentage of acreage fertilized annually that can be attributed to the research cooperative.

While the value of new information has received attention in agricultural research evaluation (Hayami and Peterson 1972, Huffman 1978), it has only recently been investigated in forestry. Chang (1985) found that use of a growth and yield model developed for upland oak in New England rather than the Midwestern model used previously resulted in an increase in land expectation value of nearly $50 per acre. By estimating the extent of application of the new model and the costs of research, Chang calculated a benefit-cost ratio in excess of 16:1. Where new information does not lead to significant changes in management practices, the returns would probably not be as large.

The Canadians are also active in research evaluation. Babcock (1973) provides the following observation:

> In a nutshell, all the methodology does is to permit a systematic quantification of tangible and intangible benefits from Canadian Forest Service research, according to national objectives, and ties the estimated benefit to the cost of doing the work. The results of program evaluation are a *guide* to decisionmaking and not a *decisionmaker*.

Within this framework, Babcock discusses research evaluation criteria, definitional programs and projects, and selection of a panel of experts to develop weightings for criteria and ratings for projects. The panel approach to project selection has also been used by the Western Forest Products Laboratory in British Columbia (Kennedy 1977).

Taylor (1973) tried to develop monetary benefit measures for several Canadian forestry research projects, but succeeded in only one of the cases chosen. He states that serious problems with quantifying research benefits in forestry stem from lack of readily accessible data and long production periods.

A review of forestry research evaluation is found in the proceedings of a workshop held recently in St. Paul, Minnesota (Risbrudt and Jakes 1985). A second proceedings, from a forestry research evaluation conference organized by the International Union of Forestry Research Organizations (IUFRO), is also available (Burns 1986). At a more conceptual level, the collection of papers contained in Hyde (1983) provides an overview of research evaluation thinking in the forestry community.

Many of the most useful research evaluations are those done inhouse, and not meant for public scrutiny. The U.S. Department of Agriculture, Forest Service conducts such formal reviews or evaluations of research projects and programs on a regular basis. These normally consist of field visits by Washington Office staff and other specialists or users. Their purpose is primarily to obtain information for decisionmaking.

Other evaluations of university research programs are carried out by the Cooperative State Research Service (CSRS). These reviews or evaluations consist of formal visits to cooperating universities by a review team appointed by the CSRS. Personal judgment, based on review of records and interviews, is the main approach used.

One Forest Service approach, the design of which has not been published, was used to evaluate costs and benefits associated with 1985 RPA research program alternatives (Shafer and Davis 1983). It is a first attempt to evaluate the total Forest Service research effort, and will certainly be subject to major adjustments and refinements. The approach combines use of expert panels with a number of innovative analytical techniques. Although tailormade for the Forest Service, the basic underlying benefit-cost model is the familiar one used in many evaluations. As with so many evaluation or planning exercises, the key challenge is generating or locating acceptable data.

No discussion of research evaluation literature in forestry would be complete without mentioning the seminal papers of Lundgren (1981, 1982, 1983, 1986). He has provided the guidance and insight necessary for expanded efforts to understand and undertake research evaluations in the field of forestry. Although not written to help administrators plan and budget research programs, they do provide useful guidance for setting research evaluation priorities.

Most of the evaluations discussed above have been formal (although not necessarily quantitative) evaluations, undertaken as major assignments. The Forest Service is often called upon to quickly produce research evaluations related to policy issues. For example, several years ago it was asked to identify the extent to which the Federal forestry research effort addressed the concerns and problems of nonindustrial private forest owners. Forest Service staff specialists classified each research project in their respective technical areas into one of six main categories. The results of these classifications were summarized as proportions of line item appropriations for the fiscal year 1983 budget, and then summed to produce a classification profile for the current Forest Service research program. Although lacking in sophistication, this and similar studies have provided practical guidance for administrators charged with making research management decisions.

In summary, most of the quantitative approaches to forestry research evaluation have been variations on the widely used benefit-cost approach (e.g., Gregersen *et al*. 1983a; Rose 1982; Porterfield *et al*. 1975; Taylor 1973; Cleland *et al*. 1979, 1982; Shafer and Davis 1983). But another common approach involves use of opinions or judgments from groups of experts, clientele groups, or other types of panels. This is comparable to the peer group review process found in many other fields. This approach is discussed by, among others, Babcock (1973), Kennedy (1977), Cleland *et al*. (1979, 1982), and Ohman and Skok (1977).

The remaining evaluations either do not fit into neat categories or else fit into the general category of "descriptive" evaluations—i.e., personal judgments, descriptions of the research itself rather than its value, and so forth. A number of rather simple approaches to research evaluation have been used by forestry research administrators and policymakers alike.

Most of the published evaluations have been conducted to provide information for research planning, budgeting, and related activities. Recent studies, although useful in decisionmaking, have been conducted primarily to test research evaluation methodology. Although the field of forestry research evaluation may be characterized as in its infancy, recent work demonstrates that it is moving into a new, more sophisticated phase.

## SUMMARY

**Pamela J. Jakes**

Forestry research evaluation is not significantly different from evaluation of other subjects. Many so-called "research" evaluations, however, are evaluations of social or organizational innovation processes, rather than evaluations of research. The value of research is latent, a value realized through use of its results after they leave the laboratory. Standing alone, research can only be evaluated in terms of norms of the culture which produced it. The factors that cause research to be pulled or pushed out into the world, and the factors that determine its rate of spread and adoption, thus have great bearing on the actual benefits derived.

The distinguishing feature of research as an evaluation subject is the uncertainty of the magnitude of benefits to society resulting from it. This uncertainty increases as one moves from developmental to applied to basic levels of research.

Research evaluation needs in forestry are diverse, due in part to the extremely broad scope of forestry research. Needs vary with discipline (e.g., production vs. social science or trade research), type of research (basic, applied, or developmental), evaluation questions (e.g., resource allocation, justification), and state of research (e.g., preappraisal, monitoring).

While forestry research is broad-based in scope, it is highly concentrated in terms of execution. Roughly two-thirds of U.S. forestry research is done by the U.S. Department of Agriculture, Forest Service and eight private firms. Useful evaluation techniques and the results of evaluations could, therefore, be applicable to a major share of forestry research decisionmakers.

Models or techniques to quantitatively or qualitatively evaluate research have been developed and applied in a great number of disciplines. But few evaluators have attempted to integrate evaluation tools from different disciplines. Little evidence exists of cross-referencing from one area to another, for example, from agricultural economics to evaluation research or to industrial research evaluation. This lack of interaction is certainly partly due to the nature of the U.S. academic reward system, but it may also be partially explained by how evaluators view their efforts: Is it applied social science research or a social service function? This distinction is seen in the evaluation research literature. For the social scientist, disciplinary tenets loom large in the search for valid statements, tenets which are not capable of integration with other disciplinary paradigms.

Thus, one of the main problems with research evaluations is not the availability of models and techniques, but rather the lack of specificity of objectives and purposes, lack of administrative support (or market for research evaluation results), and lack of adequate data. Attention needs to be devoted more to the process of research evaluation—to the ways in which objectives are specified, relevant data are generated, support is obtained, and appropriate models and techniques are chosen to meet real-world constraints.

While all decisionmakers use evaluations, very few use formal, rigorous evaluations. Informal evaluations appear to serve mainly as background information for shaping future program directions. From the industrial and evaluation research literature, we find that decisionmaking processes are often not "evaluation friendly"—that is, they do not lend themselves to the injection of specific information at specific points in time. Clear decisions of the "go/no go" variety are rare; rather, decisions are made incrementally. An evaluation which collapses all aspects of research into a single number is mainly limited to specific uses, such as *ex post* justification. Such evaluations fail to provide useful information on evolving, ongoing programs. To the extent that an evaluation seeks to facilitate an open and knowledgeable decisionmaking process, it needs to be less sectarian and judgmental and more inclusive.

It is evident that credibility is a key factor in determining the ultimate usefulness of a research evaluation. Credibility relates to such factors as who sponsors the evaluation, who carries it out, the clarity of the results, the perceived level of uncertainty surrounding the results, and the agreement of the results with decisionmakers' views and previous information. This does not mean that the only evaluation worth doing is an external evaluation. Internal evaluations can be very beneficial, especially in terms of learning from the evaluation process itself. Also, trust develops through active participation.

What is needed in the way of future research on forestry research evaluation? It is essential that the questions of who makes decisions and what types of decisions are made in the forestry research system be studied in much greater detail. It is only possible to design relevant evaluations when we know how they are used in the decisionmaking process.

Some case studies now exist in forestry to indicate the usefulness of certain research evaluation approaches. However, other alternatives need to be explored. For many types of forestry research evaluation, a broad interdisciplinary approach is needed, and combinations of quantitative and qualitative evaluation tools should be tried. Impacts from the transfer of research results across national boundaries should be assessed.

Some specific topics for future research on research should include:

- The link between research planning and evaluation and technological forecasting.
- Technology assessment and research planning.
- Evaluation measures other than economic efficiency.
- The link between international technology transfer and research progress in the United States.
- What we can learn from research failures, or research that does not accomplish its intended task.
- How research directions change over time in relation to budgets and the general economy.
- Marketing approaches to research evaluation.
- How the potential size of user populations for given research outcomes could provide useful information for research decisions.
- New ways to identify promising areas for future forestry research.

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Reviews research evaluation techniques in a variety of fields and assesses the usefulness of various approaches or combinations of approaches for forestry research evaluation. Presents an evaluation framework that will help users develop an approach suitable for their specific problem.

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