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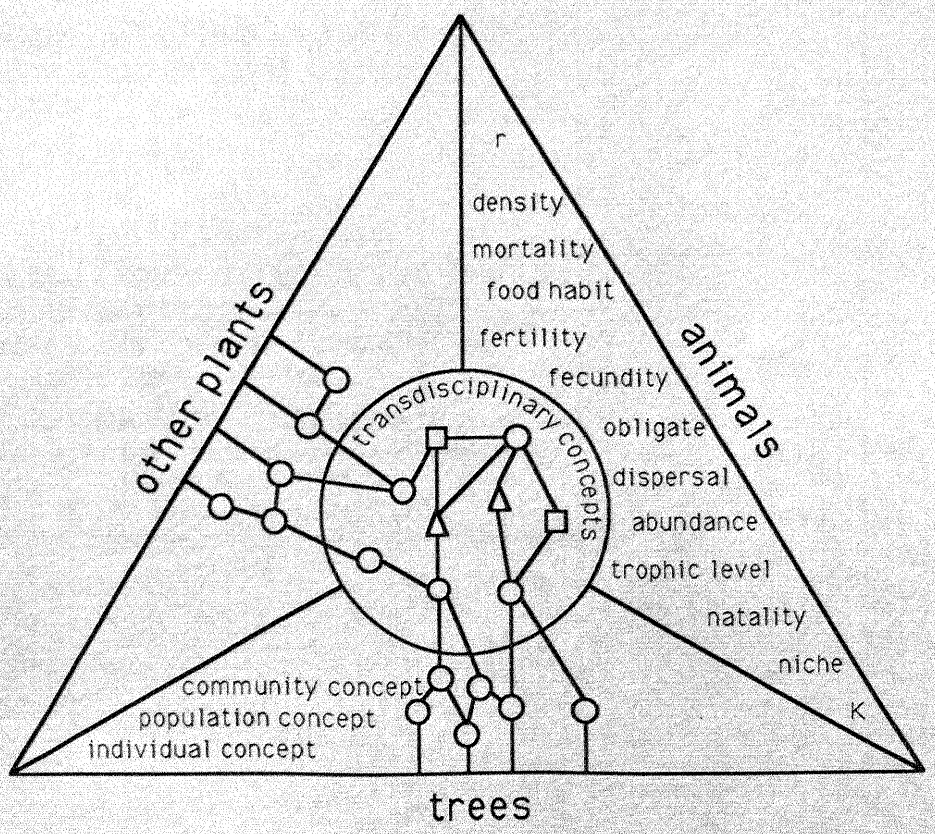
Discovering New Knowledge About Trees and Forests



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DISCOVERING NEW KNOWLEDGE ABOUT TREES AND FORESTS

Selected Papers from
a meeting of IUFRO Subject Group S6.09:
Philosophy and Methods of Forest Research

held at

Michigan Technological University
Houghton, Michigan, USA 49931
August 19-23, 1985

Hosted by: Department of Forestry
Michigan Technological University

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PREFACE

Subject Group 6.09 — Philosophy and Methods of Forest Research — was organized at the 1981 IUFRO World Congress in Kyoto, Japan. It is the newest subject group in Division 6 of IUFRO, and one of the youngest of all the Divisions. Two important reasons for organizing such a group were spelled out at the Kyoto meeting: 1) to improve the quality of scientific research done on trees and forests, and 2) to enhance the state of forestry as a science. The primary goal of the group during its early years of existence has been to identify persons with a strong interest in philosophy of science and scientific research methods and to help others to know who shares their interests. One method for accomplishing this is to bring the identified persons into contact, either directly through participation in small meetings such as the one reported on here, or indirectly through the publication of the papers, or abstracts of papers, delivered at the meeting.

The 1985 meeting of 6.09 was not large compared to the meetings of some subject groups, but it was marked by a high level of interest and enthusiam for the subjects being discussed. When the meeting was being planned, I did not anticipate publishing the papers. During the meeting, a number of participants asked that a proceedings be prepared. Some who gave papers did not prepare their comments in written form. I have included the abstracts here that were distributed at the meeting so that others may know of their interests. I omitted abstracts submitted prior to the meeting if a paper was not presented. My apologies to the participants for taking so long to get the papers published.

I thank Dr. Ed Frayer, Dean, School of Forestry, Michigan Technological University, who kindly offered the support of the Department in organizing the meeting.

A special thanks to the Division of Education and Public Service at Michigan Tech, especially to Ms. Sue Bucheger, who did a find job of managing the details of meeting prepartion and made my first experience at such a task a thoroughly enjoyable one. I am especially thankful for the bright sunshine that arrived wednesday afternoon for our picnic on the shores of Lake Superior during an otherwise rainy week.

I also thank Dr. Robert Hann, Director of the North Central Forest Experiment Station at the time the meeting was held, for allowing me to work nearly full time on preparations following my return from Michigan Technological University, and for granting permission for a number of North Central employees to attend the meeting.

Finally, I thank the participants who travelled the long way to Houghton, and who shared their views on a diversity of topics. Those who prepared manuscripts for this document have my special gratitude.

Rolfe A. Leary
St. Paul, Minnesota
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GRADUATE INSTRUCTION IN THE RESEARCH PROCESS

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Abstract: Most new graduate students arrive ill-prepared for initiating and carrying out their first research project. The old rules that guided them to success and high grades in undergraduate courses are no longer of paramount importance, but rules for performing the new task —research—are poorly defined. In addition, ideas about research and researchers derived from the published literature may lead to unrealistic expectations, wasted time, conflicts with advisors, and needless discouragement. By formalizing activities related to initiating research, and by giving students a clearer and more realistic view of the research process, a research orientation course or seminar can facilitate the successful transition from undergraduate to graduate school. Fundamental goals for an introductory research course should include making research a less intimidating prospect, providing a sequence of tasks that minimizes procrastination and provides direction, giving encouragement for the students' initial efforts, providing criteria for making informed decisions and judging performance at critical stages in the research, and helping students develop realistic expectations of themselves and their advisors.

INTRODUCTION

New graduate students, especially new master's degree candidates initiating their first research projects, have unique problems and needs as they strive to meet the challenges presented by graduate school. Serious consideration of these problems and needs is an essential step toward developing improved methods of teaching the research process.

As we are all well aware, the quantity and quality of information about the research process available to graduate students from their departments, colleges, and individual research advisors lacks uniformity. In fact, the research training environment to which graduate students are exposed is so variable that their ability to do independent research would probably differ greatly even if all of them started with equal ability and motivation. Some time ago I discovered that one basic principle explains most events in life. This principle is embodied in Stock's Second Law: All things are random. More than we care to admit, this law applies to graduate education and research, especially the first research project undertaken by a new graduate student. The luck of the draw too often pertains in, for example, choice of graduate schools, advisors, and thesis topics.

In addition, many unwarranted assumptions are made about the advisor/advisee relationship and the research training environment. A few of the more universal and illogical of these assumptions lead to poor quality research training, and hinder development of a truly effective, formalized, systematic approach to research instruction. In spite of these obstacles, however, many departments have research courses or seminars, but their content, format, and emphasis varies widely — more widely than does, for example, the content of a basic silviculture or forest entomology course. Guidelines or even textbooks appropriate for research methods courses are not widely available. While professors might agree on, say, the content of an introductory statistics course, there seems to be little agreement on the content of a course on the even more important and fundamental subject of research. To understand why research training is so variable and especially why some research training is so poor, I'd like to explore some of the fundamental and erroneous assumptions that hinder open communication and exchange of ideas on research methods instruction.

ASSUMPTIONS

Assumption (Myth) #1
Knowing that they have been screened for appropriate undergraduate coursework and a minimum GPA, we assume that *graduate students are well-prepared for graduate school*, including initiating a research project, given a bit of guidance by an advisor.

Most educators are aware that to best teach new behavior, students must be provided with clearcut criteria or standards for judging their performance at the new task at various stages in the learning process, not just at the time of the exam or thesis defense. The best teachers provide these criteria, thus permitting more of their students to do well. Learning becomes a less random, better directed process.

But graduate students arrive at graduate school not knowing what the appropriate performance criteria for research are. The performance criteria that they learned to excel at as undergraduates — studying for grades in a structured setting — no longer pertain in any really important way in graduate school, but the criteria for the new task — research — are ill defined. In research, for example, there is usually no clearly prescribed sequence of tasks, and there is little formal or regular evaluation of progress. These differences between undergraduate courses and research are not minor. New graduate students think that the game simply gets harder when in truth they are playing a whole new game with lots of new rules. All the skills learned to get A's are not particularly useful, *but the students don't know that.* Therefore, early graduate training in the research process should focus, at least in part, on the way the new game is played. Without some orientation in this regard, students can become discouraged, frustrated with their advisors, and waste time. (In some cases they quit, and the university may lose considerable time, money, and other resources invested in those students.) They will not have a realistic perspective of this new adventure and will not be able to efficiently and effectively get at the job of learning to do research.

Assumption (Myth) #2
We also tend to believe that *all professors are good* (or at least adequate) *research advisors.* Clearly this is not true, but the way students select advisors, and the way professors select advisees, often seem to be based upon this assumption. The guidance received by the student, based on the one-to-one relationship between advisor and advisee, varies from excellent to abysmal. The relationship between having earned a Ph.D. and being a skilled advisor is as tenuous, perhaps, as the relationship between being able to produce a child and being an effective parent. An additional set of skills must be learned. A minority of individual professors are, through talent or, more often, hard-earned ability, excellent research counselors and instructors. Most of the rest are wise advisors to at least some of their students some of the time. I'd like to briefly itemize a few obstacles that arise at one time or another in the advisor/advisee relationship. These obstacles contribute to the "randomness" of research training and can serve to emphasize the problems inherent in a system that too often relies almost entirely on research instruction by individual advisors.

- **Unrealistic (overly high) expectations for guidance**. Having been guided by their undergraduate teachers, many graduate students expect their advisors to take the lead, not knowing that it is they who must take the lead in the graduate program.
- **Personality differences**. Researchers range from creative and impulsive to systematic, from authoritarian to democratic. Conflicts can arise between an advisor and student with basic differences in research or leadership style.
- **Excessive faith in trial-and-error learning**. Most researchers learned a great deal of their craft through trial and error. As graduate students, they were thrown in the water and learned to swim, so they believe (to some extent perhaps correctly) that this is a good way to sort out the qualified from the unqualified researchers of the the future.
- **Heavy work load/not enough time**. Unfortunately, many faculty members (perhaps especially the most competent

and professionally involved ones) are genuinely too busy to take the time they would like to take to work with each of their students.

•**Increasing dependence on learning by osmosis**. New assistant professors usually spend a great deal of time working out the nuances of research with their first graduate students. However, after doing this a few times, less and less effort may be put into it. Advisors begin to assume that subsequent generations of graduate students will absorb the appropriate "street smarts" from their predecessors once they've gotten the ball rolling.

•**Fading memories**. In most researchers, memories of the anxieties, questions, and self-doubts that they felt about their own early research efforts tend to fade as they gain self-confidence. It's harder to recall the initial fears and problems once you've mastered the game.

•**Overemphasis on methods**. Finally, even when advisors make a real attempt to communicate with their advisees, it is much too easy to focus on the methods. Often a student will come to an advisor with a difficult and usually ill-defined intellectual problem and the two will end up rummaging in a drawer looking for a different kind of lens or making phone calls about some piece of equipment, the real intellectual issue set aside because neither the student or the advisor could easily deal with it. Fortunately, some activities routinely required in graduate programs, such as oral exams and seminars, provide a degree of formalized group discussion of the intellectual aspects of the research process.

Assumption (Myth) #3

The research process is logical and straightforward, and thus easy to teach.

The fact of the matter is that research only appears logical and straightforward after it is tidied up for publication. The more coherently a scientist writes, the more an illusion of order can be created from disorder. Although this practice has many important benefits for the dissemination of knowledge to the scientific community as a whole, students having read about research in textbooks or journal articles enter graduate school with a stereotyped and unrealistic notion of what research is all about.

Students doing research are regularly faced with discrepancies between the model of research provided by texts and teachers and the realities of how research is actually conducted. This tends to make the students feel that much of what they are doing is wrong because it does not match the published research accounts that they accept as performance standards. The discrepancies between what they think they should be doing and what researchers actually do can be disorienting and discouraging.

The idealized concept of research and the writing style required by journals suggests that research moves in an orderly, step-by-step manner from background theory and observation to problem definition. A hypothesis is formulated and tested, then accepted, rejected, or modified. Finally, the new information is added to the body of background theory used to do further research.

Actual day-to-day research differs a great deal from this simplified model. Actual research consists of a number of closely related activities that overlap continuously, rather than follow a prescribed sequence. In most research there is no clear end point. Because of time and reporting constraints, an end point must be imposed upon an essentially endless process. (According to Lanier's Axiom, a research project is never completed: it is merely abandoned when the ratio of cost — in dollars or effort — to results becomes less favorable than that of another project or enterprise.) Beginning researchers are also unaware of the amount of change and revision involved in research, since they see only the final product in the literature. They also often do not know that a research subject is often chosen for nontheoretical reasons, such as the availability of support funding, and only afterwards is it given a theory-based justification.

Finally, one of the largest differences between research as it is described in the literature and actual research is that actual research involves many additional activities that are rarely, if ever, mentioned in publications. These activities include logistics (the management of money, people, facilities, and

time), intellectual activities (such as those involved in the generation of ideas or hypotheses), and communication. Writing and speaking coherently and eloquently about research are not automatically derived from doing research (not even from doing very good research), but they are an extremely important part of the process.

DISCUSSION

By formalizing activities related to initiating research, a research orientation course or seminar can give students a clearer and more realistic view of the research process and facilitate their successful transition from undergraduate to graduate school. Fundamental goals for graduate research instruction should include: making the prospect of research less intimidating; providing students with a sequence of tasks that minimizes procrastination and provides direction; giving encouragement for the students' initial efforts; providing students with criteria for making informed decisions and judging their performance at critical stages in the research; and helping students develop realistic expectations of themselves and their advisors.

Practical aspects of research are particularly important in such instruction. In addition to the standard information on research history, philosophy, and methods, an introductory course can also include information on research management (e.g., planning, time management, budgets), the creative process (e.g., idea generation, the role of chance in discovery), and communication (both writing and talking). Development of individual research proposals during the course increases interest in the subject matter, adds motivation, and provides continuity and integration of all course elements.

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TEACHING SCIENTIFIC REASONING

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Abstract. Scientific research is a problem solving activity. Fortunately, ability in problem solving is a skill, like swimming and piano playing, that can be developed by practice and imitation. The development of problem solving ability ought to be a major educational objective of postgraduate instruction in research methods. To achieve this objective, students must be challenged with problems that provide an opportunity to practice inductive reasoning, the formulation of conjectures, and the critical evaluation of empirical evidence. The books of George Polya contain many such problems, and much more. I will discuss a selection of his problems, and my experience with using them to teach problem solving to graduate students in a Canadian university.

I teach research methods to beginning graduate students in the School of Forestry at Lakehead University. My objective in presenting this paper is to discuss one facet of that course: teaching scientific reasoning. I emphasize this topic for two reasons. First, as a prerequisite to a successful scientific career, students must understand, as thoroughly as possible, the scientific method and the nature, origin and limits of scientific knowledge. Understanding scientific reasoning is, in my opinion, the key to these insights. Second, teaching thinking skills, of which scientific reasoning is an example, is more difficult than teaching either declarative knowledge or technical skills. Hence, there is a greater need, I think, for teachers to share their ideas in this area than in any other.

In spite of the narrow emphasis of this paper, my course covers a balanced blend of information about research. In the hope that this balance will show through, I will begin with an overview of the course's content and my philosophy of teaching research methods.

THE QUESTION OF WHAT TO TEACH

In deciding what to teach, I have asked myself "What does a scientist have to know?" Of course, s/he has to know a lot, but most of the material can be classified into two broad categories. One category covers heuristic knowledge. The heuristic knowledge of a scientist includes problems solving and reasoning skills. But, it covers a host of other, less tangible, and perhaps intuitive elements as well. For example, I think that many scientists eventually develop a hierarchy of aesthetic values about research that influences their own work, for better or worse, in a variety of subtle ways. Scientists rely on heuristic knowledge when they decide what to do and how to do it, and when they decide what their results mean.

The other subject matter category covers a diverse body of technical skills and knowledge. Some of these are general purpose items, e.g. the skills and knowledge associated with the design of experiments; others are specialized items, e.g. the skills and knowledge associated with tissue culture or electron microscopy. Scientists use these tools whenever they actually do some research (as opposed to just thinking about it).

In my research methods course, I cover both the heuristic and general technical aspects of the subject. The technical topics covered include the basic principles of research problem analysis, library research, project planning, experimental design, proposal writing, the execution of experiments, data analysis, and the communication of research results. The heuristic topics covered include problem solving and reasoning skills. I do not, however, teach the less tangible elements of a scientist's heuristic knowledge, at least not explicitly. Probably this knowledge cannot be taught in any large

measure in a formal course, but instead grows out of one's personal, lifelong experience with science.

THE QUESTION OF HOW TO TEACH

Teaching Technical Skills and Knowledge

The set of rules for deriving the expected mean square terms in an ANOVA table is an example of a technical information topic. Information topics are relatively easy to communicate. Conventional methods include lectures, assigned readings, and if the material lends itself, hands-on practice. Practice is especially necessary when the educational goal is to develop a skill as opposed to the communication of declarative knowledge.

To provide practice with some important research related skills, I ask my students to undertake a research problem analysis, to write a research grant proposal, and to deliver a seminar on a technical subject. I also assign a series of homework problems that exercise experimental design skills.

Teaching Problem Solving and Reasoning

My personal experience, and I believe that of others, has been that it is more difficult to teach thinking skills (e.g. problem solving and reasoning) than it is to teach either technical skills or declarative knowledge. In the remainder of this paper, I discuss why this is so, and I outline a few ideas for overcoming these difficulties.

What are problem solving skills?

The inputs to the research process are problems. It is not surprising, therefore, that successful scientists are good problem solvers, at least within the domain of their own research. Expert problem solvers have many characteristics in common regardless of the nature or context of the problems themselves. For example, expert problem solvers are aware of, and use a problem solving (PS) strategy. Experts also understand the need to alternate between direct PS activities (e.g. gathering information or perfoming calculations) and PS management activities. Occasions when experts jump in and out of their role as PS manager include: 1) when they want to assess where they are, and where they are going, in the PS process, and 2) when they are stuck. In the latter case, experts know a variety of ways to get unstuck. For example, they may "work backwards", consider simplified cases, or seek and use analogies.

What are reasoning skills?

In my experience, many students begin graduate study with several misconceptions about the process by which new scientific knowledge is acquired. Of course, there is much variation between students. I suspect at least two reasons for this situation. First, the (North American?) educational system, up to and including much of the undergraduate university level, is concerned with the communication of what is "known". Often little is said, however, about how the current state of knowledge came to be. Second, science and mathematics textbooks, especially at the introductory level, often give the impression that science proceeds largely through the use of deductive reasoning. Perhaps this orientation is a natural consequence of the desire to compactly and systematically communicate "the facts".

Experienced scientists, on the other hand, have quite a different view of how new knowledge is acquired. They know that in the course of a single research effort they will use a whole spectrum of reasoning skills. These range from strictly formal deductive reasoning to more or less informal inductive reasoning. Experienced scientists understand further that the conclusions drawn from empirical evidence always must be based on inductive reasoning. They appreciate the limitations of such conclusions, and all that this implies about the nature and limits of scientific knowledge itself.

Clearly, the transition from beginning graduate student to mature scientist involves, among other things, the acquistion of a collection of reasoning skills and attitudes about evidence, truth and knowledge that is both broad and deep. Polya (1954, 1968) refers collectively to these skills and attiiudes as "the art of

plausible reasoning". It would be good if these things could be taught.

The trouble with teaching thinking

There are two sources of problems that must be overcome in order to effectively teach thinking skills such as those outline above. First, both student and teacher must understand that thinking is a skill. The fact that it is is good news because it means that accurate, efficient thinking can be learned as other skills are learned through practice and imitation.

The second problem stems from the fact that thinking is an invisible skill. Thus, while a student of, say, swimming can observe and analyze the techniques of an expert, a student of thinking cannot — at least not without some extraordinary cooperation on the part of the expert.

The rest of this paper is about solving these two problems.

Sources of ideas about teaching thinking

There is a rapidly growing body of literature on problem solving. This literature deals both with general problem solving methods and with methods of teaching problem solving. One valuable source of this information is PS news, a newsletter by Dr. Don Woods (see literature Cited). PS news is also a helpful guide to the wider body of PS literature.

Through PS news, I discovered the books of George Polya. "How to solve it" (1957) is well known and worthwhile. However, Polya's other books on problem solving (1962, 1965) and the art of plausible reasoning (1954, 1968) are even better sources of general PS methods and teaching ideas. Also through PS News, I learned of the work of Drs. Arthur Whimbey and Jack Lochhead. Their book (1982) and an earler article (Whimbey 1977) discuss the problem of how to make thinking a visible skill.

Teaching problem solving and reasoning as skills

Until recently, I tried to teach "the logic of scientific discovery" as factual information. I assigned readings (e.g. Russell 1931, Popper 1962, 1968, Bunge 1967a,b) and gave lectures on the philosophy and methods of science. While these efforts were not completely wasted, I now believe that a more effective teaching technique is one that allows students to actually practice the problem solving and reasoning skills associated with doing research. The question is where to find a suitable setting in which practice research can take place.

Analogy is a powerful aid to understanding complex ideas. Therefore, I believe that the object of study of practice research ought to be a system, since a system is the object of study of much real research. Furthermore, the goal of practice research ought to be to discover the rules that govern the system's behaviour, since the discovery of governing rules is the goal of much real research. In short, it seems reasonable to make the analogy between real and practice research as strong as possible. I am about to suggest, however, that there is a limit.

At this point, the search for the ideal practice system becomes complicated by two factors. First, the ideal practice system must be at the right level of difficulty. It should be possible, for example, for graduate students of average ability to make significant progress towards understanding the behaviour of the practice system in, say, a few days. Second, I feel that the ideal practice system ought to be noise-free. I will explain this last statement since it may seem contradictory. After all, the educational goal is presumably to train natural scientists, and natural systems are anything but noise-free.

The case for noise-free practice research systems

Natural scientists face two problems in the course of their research. First, they must extract meaningful information about system behaviour from a set of noisy observations on the systems. Second, they have to use this information as a ground for discovering the rules that govern the system's behaviour. The body of knowledge that treats the first of these problems is statistics. The body of knowledge that treats the second might be called "scientific reasoning".

Of course, statistics and scientific reasoning are separate topics, and in my opinion, the best policy is to teach these two topics separately. That way the fundamentals of scientific reasoning can be seen clearly. This separation can be accomplished if the principles of scientific reasoning are taught by means of practice research in a noise-free setting. Once students understand these principles, statistics can be presented as a tool for coping with the special problems of noisy systems.

Two noise-free practice research systems

Mathematics is a rich source of noise-free systems that span a wide range of complexity. The two problems that follow are from Polya (1954).

THE TRIANGLES PROBLEM: The 3 sides of a triangle are of lengths l, m, and n respectively. The numbers l, m, and n are positive integers such that l is less than or equal to m, and m is less than or equal to n. Find the number of different triangles of the described kind for n = 1, 2, 3, 4, 5,.... Find a general law governing the dependence of the number of triangles on n.

THE LAST INTEGER PROBLEM: Try to discover the rule that governs the following table.

a	b	Last integer that **cannot** be expressed in the form aX + bY
2	3	1
2	5	3
2	7	5
2	9	7
3	4	5
3	5	7
3	7	11
3	8	13
4	5	11
4	6	19

The numbers X and Y must be non-negative integers.

On the first reading, it may not be apparent that these two problems exercise the patterns of reasoning used by natural scientists. To partially appreciate this fact that they do, notice that both problems preserve three important elements of real research in the natural sciences. First, in both problems the object of study is a system. Second, both systems transform (map) input values onto output values in a way that is at first puzzling (so there is a phenomenon to be investigated). And, third, the research problem in both cases is to discover the rules that govern the system's mysterious behaviour.

To fully experience that these problems exercise scientific reasoning skills, you should solve them yourself. Of course, in doing so you must limit yourself to the experimental methods of a natural scientist, and avoid the special methods, e.g. mathematical induction, of the mathematician. As an alternative to solving both problems yourself, I have outlined in the Appendix the steps taken by several graduate students in the course of solving the Triangles Problem. If you wish, read the Appendix before continuing.

Making thinking a visible skill

I mentioned earlier that thinking is an invisible skill, and, therefore, inherently more difficult to teach than a visible skill like swimming. In the case of teaching scientific reasoning, the problem for both the teacher and the student is to discover how the student thinks while s/he is attempting to solve a research-type problem. Only then can errors in reasoning be discovered and corrected and good habits reinforced. Three educational methods for making thinking visible are: group discussion, Whimbey-pairs, and student PS chronicles. Let's consider each of these in turn.

Group discussion

One of several educational applications of discussion groups is to teach analytical thinking and reasoning (McKeachie 1978). Therefore, one might be lead to believe that this method would be ideal for teaching scientific reasoning. My own efforts in this direction, however,

have often been disappointing. Hence, while the method is of value, and ought to be used, it is not foolproof.

Some of the things that get in the way of productive class discussions are these:

1. Students do not know how to participate in a discussion group.

Beginning graduate students are not necessarily skilled at participating in group discussions. When this seems to be the case, it may be wise to invest a little class time teaching the fundamentals.

2. Students lack sufficient knowledge about the topic under discussion.

Individual students may feel that they have not progressed far enough in their thinking about science to enter a discussion. Often, beginning graduate students have only limited personal experience with independent research. Practice research problems provide one source of common experience upon which class discussions can build.

3. Students are reluctant to reveal their thoughts in public.

This may be the biggest obstacle to fruitful class discussion. When it is a problem, it may be due to the next item.

4. The teacher is unskilled as a discussion leader.

Students know that my motive for group discussions is to get their thinking out in the open. This strains our relationship, especially when they believe that their thoughts are contrary to my own on the issue under discussion. Fortunately, there are several helpful source books with advice on how to resolve such problems. Two good ones are Auer and Ewbank (1954) and Maier (1963).

Whimbey-pairs

Whimbey (1977) discussed the use of a method for teaching cognitive skills in which students work in pairs or small groups. Briefly, Whimbey asks one student in each pair to solve a problem aloud while the second student listens. Whimbey choses problems that exercise problem solving and reasoning skills. The problem solver is instructed to vocalize everything that s/he thinks about in the course of attempting to solve the problem. The listener is instructed to check for errors in the problem solvers reasoning, monitor his/her PS strategies, encourage the problem solver to vocalize thoughts when s/he is silent, and so on. The members of the pair switch roles with each new problem.

The Whimbey-pair method focuses attention on the fact that problem solving is a learnable skill, and that there are general methods that work on a wide variety of problem situations. Because problem solvers work with their thinking revealed, it is possible for others to see (hear) how different individuals think when they are in a problem solving situation. Thus, students can learn good habits from each other.

Fortunately, most beginning graduate students are good at solving the short, reasoning-type problems that Whimbey uses. Unfortunately, the Whimbey-pair method does not lend itself directly to practice research problems like the two given above because these problems take too long to solve.

Student PS chronicles

My solution to the problem of making thinking visible in a practice research setting is to ask students to keep a chronicle of their problem solving efforts. I assign a problem like either of the two given above, and give the class several days to work on it. Periodically, we discuss their progress to date, but without giving too much away to those still working. Throughout, I emphasize that it is the reasoning process, and not the "right answer", that should be the focus of their attention.

When all of the students have experienced at least some measure of success, we spend a class period reviewing the research methods that various members of the class used in the course of their work. In these "debriefing sessions", I point out general types of problem solving activities and reasoning patterns that occur in their chronicles. These include: the collection of initial

observations, searching for patterns, guessing what might be true, figuring out critical tests, performing these tests, weighing evidence, and various examples of plausible reasoning that were involved in drawing conclusions.

CONCLUSIONS

Scientific reasoning is one of several topics covered in a graduate level course on research methods. The educational objective of this segment is to teach students how to think like scientists. Since scientific reasoning is a skill, it is taught by having students undertake practice research. The practice research preserves two attributes of real research in the natural sciences. First, the object of study is a system, and, second, the goal of the research is to discover the rules that govern the system's behaviour.

Two problems must be overcome before these ideas can be fully implemented. First, when novices consider real research, the thread of scientific reasoning may be lost in a maze of statistical procedures. It is desirable, therefore, to teach scientific reasoning and statisitics as separate topics, at least initially. The principles of scientific reasoning can be isolated by basing practice research on noise-free systems. Second, thinking skills, like those involved in scientific reasoning, are invisible. Therefore, special teaching methods are needed to focus attention on the process of correct reasoning, and on the good and bad reasoning habits of individual students. Some of these special methods were discussed.

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APPENDIX: A CLOSER LOOK AT THE TRIANGLES PROBLEM

Many students begin by trying to clarify the problem. They do this by rereading the problem statement, and by examining some particular cases of n to see how n affects the size of the family of triangles associated with n. Everyone begins by looking at the simplest cases, n = 1, 2, 3, ... (Notice that this is an experimental approach, even though the experimental results ar noise-free, and the conclusions drawn are necessarily inductive.)

Eventually, the student researcher discovers two things:

a) there is a systematic way to write down the triangles that belong to any particular family, and

b) some of the allowed combinations of l, m, and n are not triangles.

The allowed combinations of l and m for n equal 4 are presented in Table 1.

By this point the PS'er , if s/he is so inclined, can see that the problem situation in terms of an input-output system. The sytem accepts whole number, n = 1, 2, 3, ..., as input and transforms these into another set of whole numbers that represent "the number of different triangles of the described kind". Let's call these second values S(n).

Table 1. The allowed values of l and m for n equal 4 presented systematically and without regard to whether the result is a triangle.

n	m	l	Is it a triangle?
4	4	4	Yes
4	4	3	Yes
4	4	2	Yes
4	4	1	Yes
4	3	3	Yes
4	3	2	Yes
4	3	1	No
4	2	2	No
4	2	1	No
4	1	1	No

A natural scientist at this stage of understanding might develop a table showing the relationship between n and S(n) in particular cases. Student researchers do this as well (Table 2).

By now the PS'ers has some experience with how the system works. It remains, however, to discover the rule that governs the relationship between n and S(n). As experience accumulates, most PS'ers discover that, somehow, it makes a difference whether n is odd or even, and they sort their results accordingly (Table 3).

Table 2. The first 10 values of n and S(n)

n	S(n)
1	1
2	2
3	4
4	6
5	9
6	12
7	16
8	20
9	25
10	20

Table 3. Values of n and S(n) sorted according to whether n is odd or even

n	Odd values of n S(n)
1	1
3	1+3=4
5	1+3+5=9
7	1+3+5+7=16
9	1+3+5+7+9=25

n	Even values of n S(n)
2	2
4	2+4=6
6	2+4+6=12
8	2+4+6+8=20
10	2+4+6+8+10=30

The pattern in Table 3, once it has been discovered, leads to the following hypothesis.

Hypothesis 1: The number of triangles depends upon whether n is odd or even according to the following rule

$$s(n) = \begin{pmatrix} 1+3+.....+n & \text{if n is odd} \\ 2+4+.....+n & \text{if n is even} \end{pmatrix}$$

Some students believe that Hypothesis 1 is the "general law governing the dependence of the number of triangles on n". Others, however, guess that the rule can be expressed more concisely. Following this intuition, for odd values of n, leads to another tabulation of the observations as shown in Table 4.

Table 4. The relationship between n, the rank of n as an odd integer, and S(n)

n	Rank of n in the odd integers	S(n)
1	1	1
3	2	4
5	3	9
7	4	16
9	5	25

Recognition of the pattern in Table 4 leads to the following hypothesis.

Hypothesis 2. When n is odd, S(n) is the sum of the first (n + 1)/2 odd integers which is given by

$$s(n) = \left(\frac{n+1}{2}\right)^2$$

Flushed with this success, it is easy to discover the general law for the case of even n.

THE CASE METHOD IN TEACHING RESEARCH METHODS

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Abstract. I teach a research methods course in the USU College of Natural Resources, mainly to graduate students in forestry, recreation, wildlife, range, and hydrology. It also attracts students from biology and ecology. The course covers the scientific methods of induction, retroduction, hypothetico-deduction, and analogy. It also covers several methods used in planning research.

I use the case method to simulate the research process. I have patterned it after the way it is done in business schools. I take a piece of published research from some source like Science magazine, and write the case up by presenting the student with the objective of the research and all the "givens." Then I ask them to complete the research design and present and defend it in class. The students do not know where the problem comes from. For example, a case typically posses a hypothesis and presents all the kinds of facts that can be experimentally obtained. The student has to deduce test predictions from the hypothesis and design the experiment.

I will pass out copies of several cases for you to take home, think about, and judge their effectiveness as a teaching tool.

This paper explains the case method of teaching, describes its use in a research methods course given to graduate students in forestry, recreation, wildlife, range, hydrology, ecology, and biology at Utah State University, and illustrates a case that is used in the class.

CASE METHOD OF LEARNING

Most learning in college is passive: through lectures the instructor loads the minds of students with facts, laws, and theories. The case method of learning is designed to allow students to derive principles rather than have principles announced to them. The first credited use of the case method was in 1871 at Harvard Law School. Around 1910 it was subsequently adopted at Harvard Business School.

There are two kinds of cases. In the **issue** case, the students are given a real unstructured problem, complete with background and context, and they must synthesize a solution. In the **descriptive** case, the students read someone else's solution of an unstructured problem and they criticize it. Unlike mathematical problems, real problems seldom have proposed solutions that others cannot mount challenges against. For this reason, the case method leads to argument and debate, and it promotes creative thinking.

Some of the features of the case method are (paraphrased from The Case Method in Library Education by Thomas J. Galvin):

--Aims primarily to develop an attitude and a way of thinking rather than a technical vocabulary and a long list of established facts.

--By presenting a case to the class and leading a class discussion in which all participate, the student presenting the case and the class share their perceptions with each other; and by trying to find order in the cases, the class can see more and understand better.

--Allows the simulation of real-life conditions — i.e., partially unordered, reasonable unstructured problems in which students discover their modes of response.

--Places the burden of thinking on each class member, forcing the student to formulate, present, and defend sound solutions to a body of problems.

--Helps provide judgment that is often missed when learning is restricted to

memorization of facts and views which others have codified.
--The goal is not the production of "right" answers, but the development of modes of thought and methods of analysis that will result in sound judgment in action.
--Aims to produce "a professional personality." By forcing the student to explore for the very heart of a problem and to come up with a practical decision, the student derives a maturity and independence of mind that is not readily available in a typical lecture course.
--The instructor must give just enough guidance so that the student is forced to think, but if too little is given the average student comes away with a fuzzy sense of no accomplishment and a feeling of frustration. In the absence of objectives that are clear to the student, little or no growth takes place.
--One major aim is to develop the qualities of self-reliance and self-confidence in the student.

CASE METHOD USED TO TEACH RESEARCH METHODS

Both issues cases and descriptive cases can be used to teach the following:
1) Posing good questions. For an issue case, the students are given the bounds of a body of knowledge. They search out what is known within the bounds, thereby identifying the gaps. The goal is to pose good questions, i.e., good problems for research. This forces them to choose criteria which define the goodness of a problem in science. For a descriptive case, the students read a published article in which problems were identified, and they critique the scientists' conclusions.

As a device for learning, the issues case is much more effective on this task than is the descriptive case. In general, the main advantage of the descriptive case is in making scientists who have published seem less "godlike": when a student can see how he could have done something better than a mature scientist did, this raises his confidence.

The main difficulty in using the case method to pose questions is that it takes three to four weeks to research what has been done in the area. At most, one or two exercises of this type can be carried out in a term.

2) Posing good hypotheses. For an issue case, the students are given background knowledge and the question is stated. For example, the question could be, "How do salmon navigate across an ocean or lake to find the mouth of their home stream?" The students are expected to pose good alternative hypotheses. This forces them to set down criteria for the goodness of a scientific hypothesis.

For a descriptive case, the students are given scientific articles in which the investigator tested a hypothesis and found it to be false. The students are to propose alternative hypotheses to test.

3) Testing hypotheses. For an issue case, the students are given background knowledge, one or more hypotheses to test, and background about the kinds of experimental manipulations that are possible. They are to predict consequences of the hypothesis and design an experiment which will give facts to compare with the predicted consequences.

For a descriptive case, the students read an article in which the scientist used the hypothetico-deductive method to test a hypothesis, and they are asked to devise a more efficient test, i.e., one that (1) requires a cheaper experiment, (2) will be subject to less experimental error, or (3) will be otherwise more conclusive.

An Example of an Issue Case Used to Test a Hypothesis

The following case is based on actual research: "Vibrations: Their signal function for a spider kleptoparasite"[1]. Students are unaware of this until after they analyze the case and discuss it in class. Then they are given the article to read and it is discussed and compared to their analyses in the next class.

This is the statement for the issue case:

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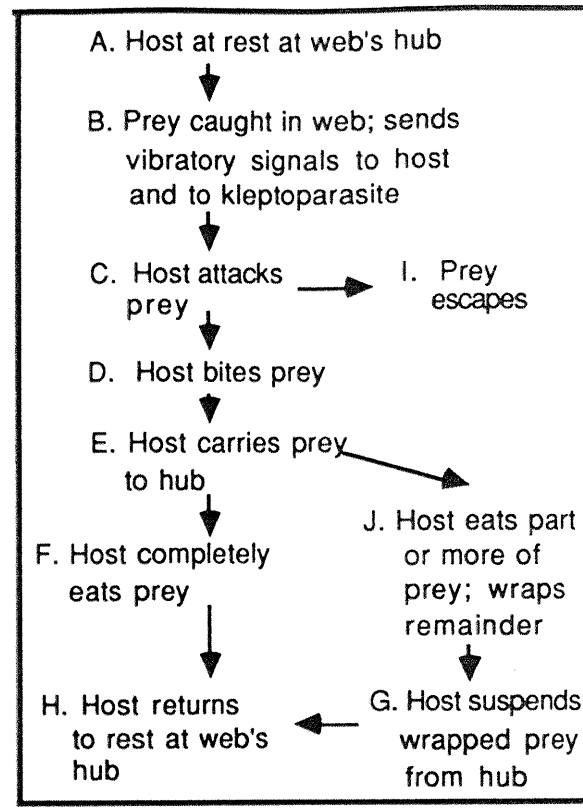
[1]Vollrath, F. 1979. Science 205(1149-1151).

Tropical spiders of the theridiid genus *Argyrodes* Simon inhabit the webs of other spiders. The kleptoparasitic *Argyrodes elevatus* Taczanowski constructs no web of its own but primarily uses the snares of the orb-weaving spiders *Nephila clavipes* and *Argiope argentata* to secure its food. Fine threads connect its resting place, 20 to 30 cm. outside the host's capture area, with the hub and several radii of the host's web. The kleptoparasites move along these lines either in search of small insects entangled in the sticky spiral, but neglected by the host, or to steal large prey items caught and stored by the host. Raids for stored prey packets are triggered by the host's prey-catching habits, and a distinct stealing behavior is displayed by the kleptoparasites, indicating a high degree of specialization toward either host species.

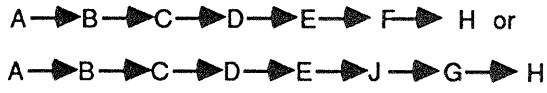
Since the vision of most web-building spiders is poor and the use of acute olfaction has not been demonstrated, it is generally assumed that vibrations are of major importance to the kleptoparasites, i.e., the vibratory pattern of the host's prey-catching sequences is read and this information is processed and the kleptoparasites adjust their stealing behavior accordingly. The host's prey-catching sequence follows this script:



Each activity in this sequence generates a distinctive vibratory pattern which can be recorded.

When the sequence is filmed, it is usually the case that after step H is completed and the host becomes active again, i.e., moves off the hub, that the kleptoparasite is lured to the hub in search of prey. The host has certain "antiparasitic" behaviors—such as searching for stolen prey or abandoning its present web site for another—which are detrimental to the kleptoparasites. By monitoring the host's movements, a kleptoparasite can reduce the likelihood of being perceived; for example, it avoids moving on the orb when the host is inactive at the hub and most sensitive to vibrations in its web. In addition, an ability to evaluate the vibratory pattern enables the kleptoparasite to adjust its pillaging to the availability of wrapped prey packets and consequently allows it to conserve energy.

We would like to know which event(s) in the prey-catching sequence (below)



are crucial for informing a kleptoparasite that a trip to the web's hub will probably be profitable. That is, the question is: which vibratory signal(s) constitute information for the kleptoparasite's decision-making model of whether or not food is available?

Remember, each event gives its own characteristic vibratory pattern which is transmitted by the structure of the web to the kleptoparasite. However, it is not clear whether the kleptoparasite can perceive all of these patterns. That is, not all signals are transmitted with equal "strength" and the kleptoparasite's vibratory receptor may not be tuned to all frequencies.

Design one (or more) tests of hypotheses which will isolate which event(s), A through H , is necessary for informing the kleptoparasite that food is available. Write out the hypothesis(es), the test conclusion(s), and the background conditions.

Finally, a note on what is possible in terms of spider experiments. It is **not** possible to artificially induce the web to vibrate in the manner each of the real events A through H would make it vibrate. While it is possible to record the vibratory patterns, attempts at playing them back to the web fail because the inertia of the vibrating mechanism of the playback device distorts the real pattern. It is, however, possible to simulate the signals of a trapped prey, thus inducing the host into attacking the source of the simulated vibrations, even though the prey may or may not be at the source point. It is also possible to feed a prey, or parts of a prey, directly to the mouthparts of the host spider at rest on the hub, in which case the host will either consume all of the food which is proffered, or it will wrap and hang the food. It is also possible to hang wrapped prey collected from other webs without disturbing the host. After the host has carried the prey to the hub (event E) it is **not** practical to remove the prey from the host. It is also possible to remove the host spider from the web at any point in the sequence A through H without the kleptoparasite, it is assumed, being aware. (END OF CASE)

Student's solutions differ in the costs of the proposed experiments, the amount of experimental noise, and in the amount of experimental control that can be exercised. Students also generally feel that the case is a puzzle that is fun to solve. On some cases I allow them to work in teams of two or three people because they seem to profit from sharing each other's ideas.

While this is a classroom exercise and the students can never be as motivated to solve it as the original researcher was, I believe the approach is more effective than having them read the article, observe how someone else was creative, and see how much rubs off on them. If students actively participate and create their own solutions to a variety of problems using the case method, then the premise is reasonable that this prepares them as well as any classroom method can.

The case method of teaching is demanding of the teacher. Good cases are hard to construct. The instructor cannot use a key to grade cases and presentations. Finally, the effectiveness depends on the "chemistry" that develops among members of the class.

USING SIMULATIONS TO EXPLORE SCIENTIFIC REASONING [1]

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Abstract. An important part of preparing science students is to make sure that they understand, in a practical way, the implications of developments in philosophy of science. This paper focuses on Popper's idea that science advances not by proving theories right, but by proving them wrong. Psychologists have developed a number of tasks that simulate this aspect of scientific reasoning. This paper describes three experiments using two of these tasks. The first two experiments demonstrate the value of giving students instruction to falsify, or disconfirm, their hypotheses. The third experiment shows that falsification must be accompanied by replication in situations where there may be error in the data. Classroom simulations that illustrate the value of falsification are derived from each experiment and their actual use in forestry and other science classes is discussed, including suggestions for improved use in the future.

Abimbola (1983) argues that philosophy of science could be used to improve science education. Abimbola particularly emphasizes Popper's (1962) idea of falsification (see Chalmers, 1979, for an introduction to Popper). This article will show how psychologists have designed tasks that simulate falsification, what experimental results they've obtained with these tasks and how these tasks can be adapted for classroom use in forestry and other scientific disciplines.

According to Popper, to be scientific, a theory must make specific predictions which can be falsified, or proven wrong. An example is Einstein's theory of General Relativity, which predicted precisely how much rays of light should be bent in a gravitational field. If results had not accorded with predictions, Einstein's theory would have been falsified.

One implication of Popper's views is that science advances not by proving theories right, but by proving them wrong. Therefore, scientists—and by extension, science students—should search for evidence that has the potential to falsify hypotheses. But as we shall see, psychological experiments suggest that most of us seek confirmatory evidence.

[1] An earlier, longer version of this paper appeared in School Science and Mathematics, 1986, 86(4):306-321 under the title, "Falsification in Experimental and Classroom Simulations".

EXPERIMENTAL STUDIES OF FALSIFICATION

Wason (1960) developed a task designed to model falsification. He told college students that the number triple '2,4,6' was an example of a rule. He asked them to guess the rule by proposing as many additional number triples as they liked; every time a student proposed a number triple, Wason indicated whether it corresponded to the rule or not. When students thought they had discovered the rule, they announced their guesses and Wason told them whether they were right or wrong. Note some of the ways in which reasoning on this task is analogous to science. Each triple can be regarded as an experiment and the rule the students are trying to discover is like a scientific law. Faust (1984) discusses why research using simulations like the 2-4-6 tasks is relevant to science.

Wason noticed that most of the students would propose strings that were positive instances of their hypotheses, e.g., if a student thought the rule was 'numbers must go up by twos' he or she might propose '8,10,12' '14,16,18' and '21,23,25'. All these triples are instances of the rule and so the student would confidently announce that his or her hypothesis was the rule. Wason took this as evidence of a 'confirmation bias' on the part of the students: very few of them played triples like '1,2,3' that should be **wrong** if the rule were 'number ascending by twos.' In other words, students failed to seek evi-

dence that had the potential to falsify their hypotheses. In fact, the triple '1,2,3' is a correct instance of the rule; this information decisively disconfirms the 'numbers ascend by two's' hypothesis. Because of their confirmation bias, many students had trouble guessing the correct rule, which was simply that the three numbers must ascend in order of magnitude. Mahoney (1976) found that scientist's displayed a similar confirmation bias on this task. (For an alternative interpretation that replaces the term 'confirmation bias' with 'positive test heuristic', see Klayman and Ha 1987).

Tweney et al. (1980) wanted to see if they could combat this confirmation bias by giving students additional training. So they instructed students to employ either a confirmatory or a disconfirmatory strategy on this task. Students given the disconfirmatory instructions did try to falsify their hypotheses more often than students instructed to confirm, but the former did not solve the rule significantly more often than the latter.

One of the problems with Tweney et al.'s study is that, like Wason, these researchers told students not only whether each of their triples was right or wrong, in terms of the rule, but also whether each of their announced hypotheses was right or wrong. For example, a subject who announced that the rule was 'numbers ascending by twos' was told that his/her hypothesis was wrong and was allowed to continue the task. Note that the experimenter has, in effect, falsified this subject's hypothesis.

It occurred to me that the true value of instructing students to falsify would only become apparent if the student's had to decide whether their hypotheses were right or wrong without being able to ask the experimenter. As Tweney, Doherty and Mynatt (1981) point out, falsification is most useful late in the inference process, when the scientist already has a clearly-formulated hypothesis. Similarly, falsification would be most effective on Wason's task after students had formulated a hypothesis, which is precisely when most of them turn to the experimenter and ask if their hypotheses are correct. If students cannot appeal to the experimenter, then those students who are given instructions to falsify their hypotheses should do better than students instructed to confirm, or follow a control strategy.

EXPERIMENT ONE

College students were randomly assigned to one of three instruction conditions: confirmatory, disconfirmatory and a control condition. Confirmatory subjects were urged to test their hypotheses by proposing strings that they thought would be correct, disconfirmatory were urged to test their hypotheses by proposing strings they thought would be incorrect, and control subjects were not told about strategies. All subjects worked individually on Wason's 2-4-6 task and were given no feedback concerning whether their hypotheses were right or wrong until the end of the experiment. (For more details concerning the specific instructions and procedures used on this and two related tasks, see Gorman and Gorman, 1984).

Results.

Results were quite dramatic: 38 out of 40 subjects instructed to disconfirm solved Wason's rule, as opposed to only 19 out of 40 subjects instructed to confirm and 21 out of 40 subjects in a control condition (χ^2=10.76, df=2, p<.005). On the average, 41% of the strings proposed by disconfirmatory subjects were incorrect, as opposed to 22% of the strings obtained by confirmatory subjects and 21% by subjects in the control condition (F(2,117)=22.37, p<.001). Disconfirmatory subjects also proposed a string they thought would be incorrect after they had announced a guess significantly more often than subjects in other conditions (χ^2=11.48, df=2,p<.005).

Discussion

Once experimenter feedback concerning the correctness of hypotheses was eliminated, disconfirmatory instructions greatly improved subject's performance on the 2-4-6 task. Subjects instructed to disconfirm obtained a higher proportion of incorrect strings and were more likely to test a hypothesis with an incorrect string, indicating that their instructions

had helped them avoid the 'confirmation bias' first noted by Wason (1960).

USING THE 2-4-6 TASK IN THE CLASSROOM

The 2-4-6 task can be used to illustrate falsification in the classroom. An instructor can put the number string '2,4,6' up on the board and ask members of the class to propose other number strings in an effort to guess the rule. As students propose strings, the instructor should write each string on the board and put a Y or an N next to it to indicate whether it conforms to the rule or not. Students should be asked to say why they chose each string. This encourages everyone in the class to think about problem-solving strategies. Tell students to raise their hands any time they think they know what the rule is. When someone proposes a solution, don't stop — urge the class to test it until the majority are satisfied the correct rule has been found. Then ask the class to write down what strings were most helpful in guessing the rule. The role of incorrect strings should become apparent to students as they discuss what they wrote.

The author has done this kind of exercise successfully in classes with from twelve to eighty college students. Some of his educational psychology students have used it successfully in junior high school and high school classrooms and it may be simple enough to be used in elementary school classrooms.

EXPERIMENT TWO

Would the results obtained on the 2-4-6 task generalize to other tasks and situations? To find out, the present author conducted an experiment using another task explicitly designed to model scientific reasoning. Romesburg (1979) discussed how to use the card game "Eleusis" to teach students about scientific inquiry. As he put it, "For beginning students who have not yet had any science, Eleusis is a training ground for bringing science concepts relegated to the blackboard into action" (Romesburg 1979, p. 607). Briefly, players try to guess a 'rule' the dealer has in mind by playing cards; the dealer tells players whether each card is right or wrong (Gardner 1977). The cards are laid out in a sequence so players can see which cards were right and which were wrong. An example of a simple rule (where H=hearts, C=clubs, D=diamonds and S=spades):

10H	**5C**	**9H**	**KS**	**2H**	**1S**
	JC	**3D**			
		4H			

The top line of cards is correct. The lines of cards going off at right angles were incorrect when played **after** the correct card above them, i.e., the three of diamonds and the four of hearts were incorrect when played **after** the nine of hearts. The rule in this example is alternating colors.

The present author designed an experiment to compare the performance of college students using confirmatory and disconfirmatory strategies on Eleusis. The students worked in groups to discover four Eleusis rules designed by the experimenter. The rules were:

1. Adjacent cards must be separated by a difference of one.
2. Adjacent cards must be separated by a difference of less than three.
3. Odd and even cards must alternate.
4. Cards must alternate either in terms of parity (odd vs. even) or color (red vs. black) or both.

Students were arranged in groups of four. Each group was given one of three sets of strategy instructions: confirmatory, disconfirmatory or a strategy that combined elements of both. Confirmatory groups were urged to test their hypotheses by trying to play cards that should be correct; disconfirmatory groups were urged to test their hypotheses by trying to play cards that should be incorrect; combined groups were urged to discover hypotheses by trying to play correct cards until they had a hypothesis, then testing that hypothesis by playing cards that should be incorrect.

As expected disconfirmatory groups did significantly better than confirmatory or combined groups. The most marked differences in performance occurred on the later, more difficult rules. Every disconfirmatory group solved rule three,

as opposed to only one confirmatory group, and three quarters of the disconfirmatory groups solved rule four, as opposed to one-quarter of the groups in other conditions.

The success of disconfirmatory groups is explained by the fact that they deliberately played more incorrect cards. On the fourth rule, for example, there are dozens of ways to get a card right but it requires deliberate effort to get one wrong: only an odd card played after an odd card of the same color or an even card played after an even card of the same color will be incorrect. Disconfirmatory groups were more successful on this rule because they falsified their hypotheses more often than groups in other conditions: they played incorrect cards 42% of the time on this rule, as opposed to 29% for combined groups and 21% for confirmatory. In fact, on every rule but the first, disconfirmatory groups played significantly more incorrect cards than groups in other conditions.

Discussion

This experiment demonstrates that falsification can be translated into a simple problem-solving strategy that college students can use to discover Eleusis rules. Those groups that persistently attempted to falsify their hypotheses were the most successful. These results, therefore, replicate those obtained on the 2-4-6 task and suggest that the value of falsification is not limited to a single task or situation. Disconfirmatory subjects on the 2-4-6 task proposed incorrect strings 41% of the time, and disconfirmatory groups proposed the same percentage of incorrect cards on their Eleusis rules; in both cases, persistent attempts to falsify lead to superior performance.

USING ELEUSIS TO TEACH FALSIFICATION

The author regularly asks his Educational Psychology classes to work together in groups to discover Eleusis rules. Instead of giving students instructions that tell them what strategy to use, they are encouraged to think about what they are doing as they tried to solve each rule, exchanging ideas and taking notes.

Typically, rules 3 and 4 in the experiment above are used. After the groups either discover the 'odd-even' rule or give up, all are told the correct solution and asked to discuss how to improve their problem-solving process before attempting an additional, more difficult Eleusis rule. (Most groups, given adequate time, will solve this rule.)

In their group discussions, students began to discover the value of trying to play incorrect cards. As one student wrote at this stage, "We had theories, only to find that one card 'shot' the theory. But just as there is a silver lining behind every cloud, it rid the excess clutter and brought us closer to the rule ... we eliminated what the rule **couldn't** be to the point where there was only one rule that it **could** be." This student realized that it is not discouraging to see a theory 'shot down'; on the contrary, this means that the group is one step closer to the correct rule.

The students then employ the insights they gained from the first rule to solving the second. After enough time has been spent on the second rule, the whole class discusses what strategies were helpful in discovering the rule. Invariably, someone mentions trying to get cards wrong or trying to disprove ideas; at this point, the author presents Popper's falsification, using the students' performance on Eleusis as an example. Other ideas come up as well: students talk about the role of competition in science, about the advantages and disadvantages of group versus individual problem-solving and about whether strategies that work on a simple problem like Eleusis will work in the real-world of science, where the 'rules' if they exist, are not simple.

Students enjoy working on Eleusis and find it a valuable experience. As one student commented recently, "I think that working on Eleusis was an excellent learning experience — especially doing it two separate times. I think after the first time we were able to talk about our approaches and then when we did it again it really made the whole process better. I will definitely incorporate it into one of my own science classes in the future."

FALSIFICATION AND ERROR

Working scientists are acutely aware that a single experimental result may be due to chance, even when the experimenter has tried to be careful about procedures. Therefore, a theory is never falsified by a single experiment. For example, the first experimental test of Einstein's theory of special relativity apparently disconfirmed it, yet Einstein did not abandon his theory even though the experiment was conducted by the eminent physicist Walter Kaufmann and even though "Einstein had to acknowledge that there seemed to be small but significant differences between Kaufmann's results and Einstein's predictions. He agreed that Kaufmann's calculations seemed to be free of error, but 'whether there is an unexpected systematic error or whether the foundations of relativity theory do not correspond with the facts one will be able to decide with certainty only if a great variety of observational material is at hand'" (Holton 1973, p. 235).

Further research supported Einstein's theory, of course, and it was eventually discovered that a procedural error caused Kaufmann's result. But the point is, Einstein knew that a single experiment could not disconfirm his theory; he would have only been concerned if 'a great variety of observational material' raised problems for special relativity.

In Eleusis and the 2-4-6 task, however, a single 'experiment' does disconfirm a theory. For example, if a subject guesses that the rule is 'red-and-black cards must alternate' and a black card is correct after a black card, the subject knows his or her theory has been disconfirmed. But what would happen if the same subject knew that there might be some errors in the feedback he or she was receiving?

EXPERIMENT THREE

To investigate the effects of the possibility of error on disconfirmatory reasoning, the present author modified the design used in Experiment Two and ran a third experiment (Gorman 1986). Each of twenty-four groups of four introductory psychology students was randomly assigned to one of three instruction conditions: disconfirmatory, confirmatory or a control strategy. Strategy instructions were identical to those used by Gorman et al. (1984) except that a control strategy was substituted for the combined strategy used by Gorman et al.. Control groups were urged to test their guesses by proposing any cards they thought would give them information about the rule.

The second rule used by Gorman et al. was eliminated, to allow extra time for reading the error instructions. After subjects had completed the first rule, they were told there was a 0-20% possibility that a string would be classified erroneously, i.e., if it were actually correct, it would be classified as incorrect and vice-versa. Experimenters used random-number generators on calculators, consulting them every time a card was played to determine whether or not it should be erroneously classified.

If fact, there was not error: subjects were given accurate feedback on every trial. The point of the experiment was to assess the effect of the mere possibility of error. Subjects indicated where they thought errors had occurred by flipping cards over.

Results

There were no significant differences across strategy conditions, in terms of correct solutions. Only five groups solved Rule 1, only four solved Rule 3 and none solved Rule 4. (Recall that Rule 2 was eliminated for this experiment.)

While performance in both this experiment and the previous one was similar on Rule 1, success rates for Rules 3 and 4 were much lower under error conditions, particularly for disconfirmatory groups. While all eight disconfirmatory groups solved Rule 3 in Experiment Two, only two solved the same rule in Experiment Three. Disconfirmatory groups also did not obtain significantly more incorrect cards than groups in other conditions.

Discussion

Only one disconfirmatory group had any error cards remaining at the end of the odd-even rule. Five of the seven other

disconfirmatory groups failed to solve this rule, even though they realized that there was no error. Four of these groups proposed rules involving a difference of one between adjacent cards and did not make sufficient attempts to disconfirm their hypotheses, in part because they were so obsessed with making sure there were no errors in the data. The two successful disconfirmatory groups had to combine a strategy of disconfirmation with systematic replication of situations in which apparent disconfirmation occurred. When error is possible, falsification has to be combined with replication (see Lakatos 1978, p. 24, for a similar argument).

INCORPORATING ERROR INTO CLASSROOM SIUMLATIONS

I have combined Eleusis with the error instructions from Experiment Three to create a classroom simulation of the effect of error on disconfirmation. The format is very similar to the Eleusis demonstration described above: students work together in small groups on the odd-even rule while an instructor walks around, checking a calculator for errors. I have used this simulation successfully with Forestry graduate students, as well as undergraduates in psychology.

A typical undergraduate classroom group performs much like the experimental groups. For example, one such group began by going from Ace to King in sequence, assigning errors to any cards that did not fit the pattern. After they reached the King, one student said, "We've got to figure out a way to go about this. We're pretty haphazard." They decided to systematically eliminate the possibility that the rule involved color by playing four Aces of different colors and suits after the King. When all the Aces were wrong, they correctly concluded that the rule did not involve color, but incorrectly concluded that only a Queen could follow a King. Like many of the experimental groups, they ended-up guessing that the rule involved differences of one between adjacent cards; systematic replication took up so much of their time and energy that they made insufficient attempts to disconfirm their guesses. It is heartening to note that Forestry graduate students showed greater sophistication, systematically developing and testing alternate hypotheses that led them closer to the correct rule.

Overall, students comment that the possibility of error forces them to become more systematic, to replicate in an effort to eliminate alternate hypotheses. They are less likely to discover the value of disconfirmation when simulation includes error. Therefore, as a follow-up exercise the instructor should tell the groups that there was no error on the rule just tried, and ask them to continue to work on it in the absence of error. Students will usually begin to disconfirm more. The class can discuss how it felt to work under error and no-error conditions. Typically, the students complain about how frustrating it is to have to be constantly aware of the possibility of error; it means that they can never be sure a guess is right. But students admit that the exercise demonstrates why falsification must be combined with replication in science, and why it is so tempting to stop falsifying after a clear pattern has emerged from the haze of possible errors.

Even more realistic and sophisticated simulations can be constructed. I have incorporated actual 20% error into classroom simulations, and found that groups correctly identified where most of the errors occurred. Ryan D. Tweney told me how he split a graduate psychology class into groups, each group working on its own Apple computer in an effort to solve an extremely complicated rule (see Mynatt, Doherty & Tweney, 1978, for a description of the task). Eventually, some groups began to cheat, trying to surreptitiously observe others' experiments. One subject broke-off from his group and tried to solve the rule on his own because he felt the group was following a poor approach. In other words, students began to discover things about the emotional as well as the rational aspects of science. This class experienced firsthand how competition affects falsification.

CONCLUSION

As Lunetta and Hofstein (1981) point out, a major advantage of simulations is that they allow the instructor to model only those aspects of reality that are essential

to his or her instructional goals and eliminate irrelevant features. Eleusis and Wason's 2-4-6 task eliminate much of the ambiguity and complexity of real scientific problem-solving; it is much harder to falsify a scientific theory than an Eleusis rule. But that simplicity is also an enormous advantage — it permits students to discover the failure of falsification inductively, in a short time. An instructor can add elements like the possibility of error to the simulation to help students discover the importance of combining falsification with replication.

However, as Bright, Harvey and Wheeler (1983) showed in a study of the effect of Mastermind on students' reasoning skills, games alone are not sufficient to teach students scientific logic. One of the lessons of recent research using the 2-4-6 task is that, in order to falsify effectively, students must have a mental representation of the problem that tells them where to look for disconfirmatory evidence (Gorman, Stafford and Gorman 1987). It is not enough to know that one must falsify; one must know how. In science, that 'know-how' comes from extensive background knowledge in a particular field, be it forestry, chemistry, or physics.

Therefore, to make simulations effective in the science classroom, they must be combined with laboratory or field experiences. Students can work together first on simulations like the 2-4-6 task or Eleusis, then be reminded of what they learned from the simulations as they come up with results in their forestry laboratory. They should brainstorm alternate explanations for the results obtained, keeping the possibility of error in mind, then design further experiments or field studies that might falsify their current hypotheses (see Platt, 1964, for a discussion of how this approach works in physics).

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A SURVEY OF RESEARCH STRATEGIES

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Abstract. Two features of science which distinguish it from non-science are its unique goal and its unique method. The method of science consists of three major activities: Problem identification, Discovery, and Justification. Problem identification is briefly discussed with respect to the Kuhnian concept of paradigm and with respect to Leary's measure of the productivity of scientists.

The endpoint of Discovery is the formulation of one or more hypotheses. A critical attribute of hypotheses is their testability. Fully testable hypotheses may be corroborated or disproved on the basis of evidence. A high degree of statistical power is necessary to assure the full testability of some hypotheses.

Justification has three components: Research Strategy, Empirical Test, and Inference. Research strategies may be classified with respect to two factors: logical intent and number of hypotheses. The syllogistic structure and key features of seven research strategies are discussed in the context of the two factor classification. A principle of modern science is that inferences be made only on the basis of tests consisting of comparisons of hypotheses or their predictions with empirical data. The strength of an inference based on an empirical test depends on the syllogistic structure of the corresponding research strategy.

Mario Bunge (1967) asserts that science has a unique goal and a unique method that distinguish it from non-science. He states that the goal of the factual sciences is "to build conceptual mappings of the patterns of facts - i.e., factual theories." With respect to method, he asserts that "the scientific method is a mark of science, ... no scientific method, no science." In discussing the components of scientific method, Hans Reichenbach (1938) emphasized the distinction between the context of Discovery and the context of Justification. The starting point for Discovery is an identified problem or gap in the current state of knowledge, and the ending point is one or more hypotheses, models, or solutions proposed to solve the problem or fill the gap. The starting point for Justification is the set of hypotheses, models, or solutions, and the ending point is a justifiable inference concerning them. A brief survey of Discovery and Justification strategies follows.

DISCOVERY

One of the important distinctions between Justification and Discovery is that there is a logic of Justification, while Discovery has usually been considered a creative enterprise for which no logic can be constructed. Strategies that have been used to discover hypotheses include the following:

Trial and Error
Systematic Search
Serendipity
Inspiration
Illumination of the well-prepared mind
Analogy
Derivation from Theory
Induction
Retroduction

The latter two, Induction and Retroduction, are frequently considered Justification strategies, but, as will be discussed later, it may be more proper to consider them Discovery strategies. A recent innovation, which may ultimately lead to a logic of Discovery, involves applying

the techniques of artificial intelligence to discovering hypotheses (Langley, et al 1987). The consensus at this point is that there are no right or wrong ways to achieve Discovery; in essence, anything goes!

Although anything goes in Discovery, the resulting hypotheses must satisfy certain criteria. One of these is the testability criterion. A hypothesis is testable if it is sensitive to comparisons with empirical evidence. If the comparison is favorable, the outcome is corroboration; if the comparison is unfavorable, the outcome is contradiction or disproof. Corroboration is used in the sense of supporting but not conclusively proving. The testability criterion has two parts: if the hypothesis is correct, corroborating evidence must be detectable, and if the hypothesis is false, contradicting evidence must be detectable. Note that some hypotheses do not satisfy both parts of the criterion. Universal hypotheses such as 'all swans are white' can only be disproved, while existential hypotheses such as 'there are signals that travel faster than the speed of light' can never be disproved. The degree of testability of most hypotheses is much more subtle than these examples illustrate, and must be determined before experimentation.

For many hypotheses the degree of testability depends on experimental design. In testing a statistical hypothesis, choices are made for the significance level, say .05, a test statistic, and the corresponding critical value. There is confidence that if the hypothesis is correct there is a 95 percent chance of detecting corroborating evidence. Thus, half the testability criterion is satisfied, in probability, simply by choosing a significance level. Satisfying the other half of the criterion is usually not so simple. First, it is necessary to state what would be sufficient evidence to disprove the hypothesis, and then determine the chances of detecting those conditions if they exist. This is the matter of statistical power. If the probability of detecting the conditions is very small, then the hypothesis is not fully testable. The reporting of nonsignificant results from experiments with low statistical power has been called "scientific fakery" (Anonymous 1985a).

JUSTIFICATION

Justification strategies may be classified with respect to two factors, logical intent and number of hypotheses (Figure 1). The logical intent of Justification strategies has traditionally been corroboration. Only since the influential

<table>
<tr><th rowspan="2">Logical intent</th><th colspan="3">Number of hypotheses</th></tr>
<tr><th>0</th><th>1</th><th>2+</th></tr>
<tr><td>Proof</td><td></td><td></td><td></td></tr>
<tr><td>Corroboration</td><td>Induction/ Retroduction</td><td>Hypothetico-Deduction</td><td>Multiple Hypotheses</td></tr>
<tr><td>Contradiction</td><td></td><td colspan="2">Research Programs</td></tr>
<tr><td>Disproof</td><td></td><td>Falsification</td><td>Strong Inference</td></tr>
</table>

Figure 1. Justification strategies.

work of Karl Popper (1968) have negation oriented strategies been seriously considered. Although the categories of logical intent are shown here in their natural order, they are discussed in the chronological order of their development. Strategies that begin without hypotheses provide little justification and probably should be considered Discovery strategies. Single and multiple hypotheses strategies, particularly with a negation oriented logical intent, provide powerful alternatives to traditional strategies. The strategies resulting from combining the categories of logical intent with zero, one, or multiple hypotheses are described in terms of their key features.

PROOF

The Greeks believed that truth in the form of universal structures existed despite the variability they saw in actual observations. Greek scientists sought to discover these truths by way of intellectual or rational insight. Well into the 15th and 16th centuries, disciples of Greek science ridiculed attempts to reconcile knowledge with observation. For them genuine knowledge of the natural world through empirical science was impossible.

However, with the Renaissance this view changed. By the 17th century Francis Bacon's (1960) *Novum Organum* had become the fundamental treatise on the logic of scientific method. In this work Bacon insisted upon a gradual passage from concrete facts to broad generalizations and upon the use of controlled experimentation, not just observation. The phenomenal successes of Newton who used and extended Bacon's methods firmly established empiricism as a fundamental principle of science.

However, in his exaltation of induction and experiment, Bacon held that general laws could be established with complete certainty by using these almost mechanical processes. It was not until David Hume's (1966) Treatise of Human Nature in the 18th century that the myth of scientific proof by inductive methods was completely debunked. With empirical and experimental methods in hand and a clear understanding of the impossibility of proof by inductive methods, modern science emerged.

CORROBORATION

As stated earlier, corroboration is used in the sense of supporting but not conclusively proving. Corroboration strategies are loosely based on a valid, logical argument form called Modus Ponens. The essence of Modus Ponens is that if the antecedent, p, of a conditional proposition is established, then the consequent, q, must logically follow. Scientific corroboration strategies, unfortunately, usually attempt to establish the consequent and then imply the antecedent. They are guilty of what Copi (1967) calls the "Fallacy of Affirming the Consequent". Thus, corroboration strategies do not follow a valid argument form, and their conclusions are not necessarily valid. Closer examination of specific corroboration strategies clarifies the issue.

0 HYPOTHESES - Induction: Induction might be defined as the ever increasing accumulation of hard facts, F, which can be understood by means of tentative generalizations, G. A key feature of induction is that the facts are acquired before the generalizations are formulated. The generalization is usually not tested on data other than that from which the generalization has been formulated, and it is usually uncertain if the second part of the testability criterion has been satisfied. Successfully meeting the second criterion would seem to be mostly a matter of chance with Induction. Carl Hempel (1966) states:

> Scientific knowledge is not arrived at by applying some inductive inference to antecedently collected data, but rather by inventing hypotheses as tentative answers and then subjecting them to empirical test.

Induction would be better considered a Discovery strategy than a Justification strategy.

0 HYPOTHESES - Retroduction: Hanson (1958) describes retroduction as the following sequence:

1. A surprising phenomenon, P, is observed,
2. P would be explainable if hypothesis H were true
3. Therefore, there is reason to think H is true.

Retroduction, like induction, is guilty of affirming the consequent. Also, like induction, the phenomenon is observed before the hypothesis is formulated, and so it remains uncertain if the second part of the testability criterion is satisfied. Retroduction would also be better considered a Discovery strategy.

1 HYPOTHESIS - Hypothetico-Deduction: Hypothetico-deduction is used as a strategy for corroborating single hypotheses. The hypothesis, H, is discovered by any means available to the researcher. From the hypothesis, H, a prediction or deduction, D, is derived, which is then compared to empirical evidence. The evidence is acquired after the hypothesis is stated and therefore should be appropriate in kind and amount to satisfy the second part of the testability criterion. Nevertheless, Hypothetico-Deduction is still a corroboration strategy and, as such, suffers from an invalid argument form.

2+ HYPOTHESES - Multiple Hypotheses: In addition to an invalid argument form, single hypothesis strategies also may suffer from scientists' attachment to their hypotheses. The moment a scientist offers an apparently satisfactory hypothesis it becomes that scientist's personal possession. Hanson pointed out that even when scientists are free from such attachments, observations are generally not free from observer bias. An example (Figure 2) due to Hanson and also discussed by Brown (1977) demonstrates that the response of an observer's senses to external stimuli may depend on the hypothesis in mind. Furthermore, Hanson contends that it is not possible for multiple responses to occur simultaneously. Lack of objectivity may be a serious problem when attachment and observer bias occur.

These difficulties may be partially avoided by using the "method of multiple hypotheses" as proposed by Chamberlain (1897). With this strategy, each problem is surrounded with hypotheses and a series of experiments is performed to distinguish among them. Chamberlain points out that this strategy "distributes the effort and divides the affections." The structure of the argument is essentially a series of Hypothetico-Deductive arguments and therefore still suffers from an invalid argument form. However, when multiple hypotheses are used, scientists may perceive the same observations from multiple perspectives.

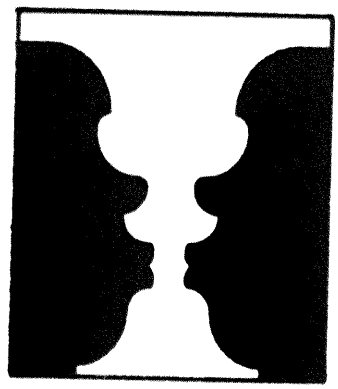

Figure 2. A vase or faces kissing?

DISPROOF

Karl Popper (1968) suggested that the problem of the invalidity of the induction argument could be avoided by shifting from corroboration strategies to disproof strategies. He argued that conclusive disproof is possible because it takes only a single counterexample to disprove a hypothesis. The basis of disproof strategies is the valid argument form, Modus Tollens. The essence of this argument is that if the negation, ~q, of the consequent, q, of a conditional proposition is established, then the negation, ~p, of the antecedent, p, must logically follow. Although corroboration strategies do not follow a valid argument form, disproof strategies do. Popper argues that science advances by disproof because hypotheses are conclusively eliminated from further consideration. For Popper, the only results regarded as corroborating

evidence for hypotheses are new and interesting failures to detect counterexamples where they would be most expected to occur.

1 HYPOTHESIS - Falsification: The falsification strategy requires a clear statement of the hypothesis and a clear statement of the conditions under which the hypothesis would be abandoned. The objective is to disprove the hypothesis by acquiring evidence establishing the falsifying conditions. Just as there is a clear distinction between the objectives of corroboration and falsification strategies, there is also a clear distinction between their experimental designs. Corroboration designs must provide for acquiring both supporting and contradicting evidence to satisfy both parts of the testability criterion. Designs for falsification experiments concentrate resources to provide maximal opportunity to detect counterexamples. If the conditions are detected, conclusive disproof is established. Failure to detect the conditions is construed as corroborating evidence because the hypothesis has withstood an extremely rigorous test.

2+ HYPOTHESES - Strong Inference: The problem with falsification, as Platt (1964) pointed out, "is that disproof is a hard doctrine." It is not easy to continually place hypotheses representing years of labor onto the cutting edge. This difficulty can be partially alleviated by using Chamberlain's method of multiple hypotheses. In fact the full potential of the multiple hypotheses concept is not realized until it is combined with a disproof intent and a logical tree structure. Platt dubbed this strategy "strong inference". The steps of the strategy are as follows:

1. Surround the problem with an exhaustive set of hypotheses whose deductions are mutually exclusive.

2. Arrange the hypotheses into a tree structure on the basis of similar and dissimilar features,

3. Perform falsification experiments at branching points to eliminate one branch or the other.

Consider the problem of determining the cause of the Cretaceous-Tertiary extinction (Why did the dinosaurs die?). The following list of hypotheses, although not complete, illustrate the strategy:

Oceanographic changes
Atmospheric changes
Climatic changes
Geo-magnetic reversal
Solar flares
Meteors or comets
Nemesis: a solar companion
Supernova
Solar movement: moving through galactic arm

This completes the first step, surrounding the problem with hypotheses. If the set of hypotheses is not exhaustive, then the surviving hypothesis may later be falsified itself. If the deductions from the set of hypotheses are not mutually exclusive, then there will be difficulty in falsification at some branching points. The second step is to arrange the hypotheses into a tree structure (Figure 3). This is not necessarily the only tree structure possible. The final step is to distinguish among the hypotheses on the basis of falsification experiments at succeeding branching points beginning at the left.

Walter Alverez, the Nobel laureate physicist, and his son Luis (1980) have reported what they believe constitutes a falsification of the terrestrial branch. They base their assertion on detection of elevated iridium levels at the Cretaceous–Tertiary boundary. They contend that because such levels are unknown on the basis of natural terrestrial processes the cause must be extra-terrestrial. Whether they are right or not, the example illustrates the way the strategy works. Platt attributes much of the recent rapid advances in molecular biology to use of this strategy. When the premises are sufficiently satisfied, the Strong Inference strategy can be a powerful tool.

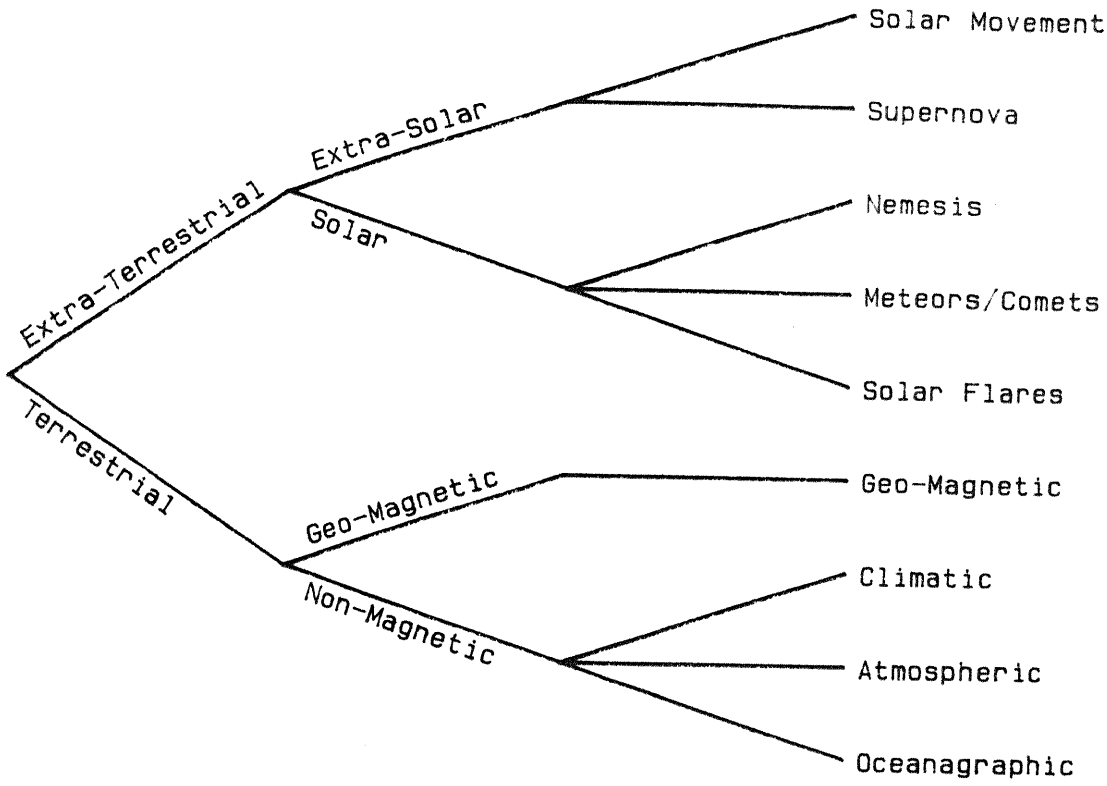

Figure 3. A tree structure for C-T extinction hypotheses.

CONTRADICTION

Disproof strategies suffer, in general, from reluctance on the part of researchers to accept their conclusions. Skeptical non-falsificationists are frequently unwilling to ascribe the power of conclusive disproof to Popper's counterexamples. This is particularly true when a favorite hypothesis or an established theory is the candidate for disproof. Non-falsificationists admit the contradiction between the evidence and the hypothesis but relegate such results to the category of anomalies rather than counterexamples.

There is justification for this attitude. Hypotheses are rarely tested in a vacuum. Virtually every test of a hypothesis is actually a test of the hypothesis in conjunction with paradigms, theories, supporting hypotheses, and additional assumptions concerning methodology, instrumentation, and observation. The falsification is of the conjunction and may be attributed to any of the conjuncts. For many reasons it may be appropriate to attribute the falsification to a conjunct other than the basic hypothesis. The history of science abounds with examples of such actions later being justified.

Research Programs: In an attempt to shore up Falsification, Lakatos (1980) developed a strategy he called Research Programs. The procedure is to first specify the protected hard core, C, of propositions that are currently considered established. The supporting and/or tested hypotheses, H, are then formulated and the falsifying conditions, ~D, are specified. The falsifying conditions are now seen as the negation of a deduction, D, from the conjunction of H and C. As in Falsification, an experiment is designed and performed in an attempt to detect the falsifying conditions. If such a counterexample is detected, the falsification is attributed to the supporting and/or tested hypotheses, H, not to the hard core, C. Research continues with this hard core as long as progress occurs. The

decision to discontinue the research program or alter the hard core occurs by consensus among scientists working in the program.

As an example, consider the numerous attempts that have been made to measure continental drift. One current method, Very Long Baseline Interferometry or VLBI, uses dish antennas on opposite sides of the Atlantic to record radio signals from the same quasar, an extremely distant and therefore stationary reference point. The differences in arrival times permit precise estimates of the distance between the two receivers. The annual rate at which North America and Europe are separating has been estimated by this method at 2.0 centimeters per year (Anonymous 1985b). Unfortunately, the precision of the VLBI method is only about 2 or 3 centimeters per year. Based on the data, it might be logical to conclude that the rate is not significantly different than zero and that there is no evidence for continental drift. However, VLBI scientists contend that atmospheric interference, bending of the signal in the ionosphere, and the unsteadiness of the Earth introduce noise into the signal that prohibits achieving a precision of about 3 millimeters. In this example, it is clear that the continental drift hypothesis is a part of the protected hard core and that it is actually the instrumentation that is being tested.

The logical intent of the research programs strategy has been labeled contradiction. Logically, it is negation oriented like disproof, but it is less conclusive due to the lack of logical validity for protecting the hard core.

SUMMARY

Discovery and Justification are different components of scientific method and require strategies with different features. Discovery strategies must satisfy only one demand: they have to work. Justification strategies must satisfy the demands of an imprecise, but fairly well agreed upon logic. Some Justification strategies satisfy these demands better than others. The classification of Justification strategies in Figure 1 and the ensuing discussion are intended to help scientists better distinguish among such strategies and select the appropriate one.

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THE CONCEPT AND MEASUREMENT OF MULTIRESOURCE SITE QUALITY

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Abstract. Conceptual ambiguities and measurement problems have clouded the evaluation of forest lands for the joint production of goods and services. Reduction of resource potentials to a common metric, whether timber site quality or a utilitarian measure of social value (a monitory unit), relies on reifications of resource concepts and unfounded assumptions of equilibrium. The nature of resources as social constructs is fundamental to any assessment of forest site quality, since social purposes define the qualities of forest lands that we attempt to measure. Social purposes are dynamic and, outside certain limits, unpredictable. As a result, site qualities are inherently unstable, and their measurement tends to be imprecise and outmoded. This paper suggests a dynamic, ecosystems based approach to assessing the functional potentialities of forest lands. Multiple functions of forest lands can be best achieved by managing forest ecosystems in accordance with certain structural and functional requirements. Examples of such requirements are provided.

PROBLEM

Contemporary forest planning necessitates that forest lands be assessed in terms of their capability to produce a wide variety of goods and services. This need is evidenced by resource conflicts in both developed and developing countries. The forester's emphasis on a single good, e.g., timber, has been opposed increasingly by those who favor other goods and services. Social conflicts between advocates for the production of timber, wildlife, fish, scenery, and recreation opportunities in national forests of the Pacific Northwest and Southeast Alaska led to research from which this paper was developed. However, the methodology to address these problems is applicable to any other region of the world where wood production conflicts with other uses. This paper discusses the conceptual and measurement problems involved in assessing the capability of forest land to produce a variety of goods and services. A subsequent paper will compare methods for assessing the productive capability of particular sites (Lee and Clark, in process).

Concepts of "resources", "multiple use", and "site quality" are central to this paper. Yet the meanings of these concepts are poorly understood. Field foresters, forest policy makers, and forest scientists share a universe of discourse in which these concepts are used routinely to simplify experienced reality rather than to analyze difficult questions. Simon (1955) has shown that people employ simple mental models to represent complex aspects of reality. Frustrating experiences arising from complex and intractable aspects of forest land management are made less threatening by imposing simple abstractions. Recent research has gone further in showing that there is a tendency for people to deliberately falsify their experience of "reality" in order to conserve or protect an orderly and "rational" image of the world (Bailey 1983). This is especially true of experience related to inherently complex, dynamic, or threatening circumstances. I assert that much of the ambiguity associated with the forestry concepts of "resources" and "multiples use", and to a lesser extent "site quality", is due to their status as abstractions for falsifying experience — for denying troublesome sources of instability and complexity in forest management.

Ritual utterances of these terms "clears the air" and assuages fears and anxieties that "things are out of control". The purpose of these concepts is clearly not to clarify their meaning as referents for actual objects or events[1]. Most writers

[1]In this paper I will ignore the possible latent sociological functions of such rituals; the sociological literature would suggest that in addition to its manifest

have relied solely on definition of the term "multiple use" as a way to specify its meaning (Leary 1985). Linguistic elaboration, including adding new labels, has prevailed over attempts to identify how the concept can be used to refer to objects and events. Similar reliance on elaborating definitions occupies most of the literature on multiple forest resources. Such definitions conserve common sense meanings and assure orderly interpretations of otherwise problematic experiences. Falsifiying troubling aspects of experience has taken precedence over attempts to use these concepts as instruments for scientific investigation.

The tasks of conceptualization and measurement of multiresource site quality are especially formidable. Forest scientists have made little progress toward adequate scientific formulations because they have not broken away from the common sense discourse in which these problems are generally stated. In formulating this paper I responded to the challenge issued by Rolfe Leary (1981) when he helped initiate the new IUFRO subject group on Philosophy and Methods of Forest Research. Leary suggested that we assess the maturity of forest science and seek to improve the quality of research by 1) becoming "more rigorous in our terminology" (p. 362), and 2) relying on appropriate mathematical formulations. I will address the first criterion by analyzing the meaning of multiresource site quality. Then I will address the second criterion by summarizing an approach we are using to evaluate propositions linking resources to ecosystem structure and processes.

SPECIFYING MULTIRESOURCE SITE QUALITY

The capability of a forest to satisfy a variety of human wants and needs has generally been assessed by defining a number of resource potentials and reducing these potentials to a common metric. In national forest planning, the U. S. Forest Service first defines the potential for a site to produce timber, recreation, water, wildlife, range, and other resources and then assigns a monetary value to each resource as a means for reducing potential resource outputs to monetary units — a utilitarian measure of social value. A complicated linear programming model, FORPLAN, is used to analyze resource values and prescribe an optimum combination of resources as outputs from an aggregation of planning areas.

An earlier tradition of forest planning assumed that "good" silviculture and timber management would automatically produce multiple resource potentials. Much of this tradition was informed by the conventional wisdom we inherited when European forestry practices were first adopted. There was a tendency to associate forest resource potentials with wood production, thereby assuming that good timber management on highly productive sites would promote resource potentials other than timber (See Duerr and Duerr, 1975, for an insightful discussion of this and other forestry doctrines). As with rhetorical statements about multiple use, "good" forestry was seldom specified, and eventually, requirements for modern forest planning were promulgated to force foresters to become more accountable in preparing forest management prescriptions.

Regardless of these planning requirements, methodologies for land assessment still rely on the conventional wisdom of the forestry community to define the concept of multiple use and its key constituent element—multiple resources. The multiresource "producing power" of the site is ignored almost entirely, or is assumed to be derived by summing the separate site ratings for each resource under consideration; interactions between biological production possibilities are thereby assumed to be insignificant.

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function of conserving a sense of orderly life, ritual utterances may also perform the latent function of maintaining existing power relationships. Hence, an apparent paradox could be resolved by hypothesizing that the existing emphasis on timber production is in part maintained by symbolic displays of the importance of multiple use.

The most advanced efforts at conceptualizing site quality and multiple use have been provided by Leary (1985). I will build upon his efforts by joining these two concepts and incorporating a conceptualization of natural resources provided by Zimmerman (1951). Since the concept of a "resource" is fundamental to both site quality and multiple use, discussion will begin with a review of Zimmerman's contributions.

Resources as Social Functions

The conventional use of the term resource is a prime example of what Whitehead referred to as the "fallacy of misplaced concreteness"—the reification of an abstraction. How often have you heard foresters talk about timber, forage, and scenery as if these resources are tangible objects to be found in the forest? Yet when we seek to find these resources, we see trees, grasses and forbs, and configurations of light, sky, mountains, trees and water. Resources are no more tangible than ecosystems; both are social constructs—invented to enable us to gain more from nature.

Zimmerman (1951) rejected the common sense view of resources and substituted the following definition:

> The word "resource" *does not refer to a thing or a substance but to a function which a thing or substance may perform or to an operation in which it may take part*, namely, the function or operation of attaining a given end such as satisfying a want ... the word "resource" is an abstraction reflecting human appraisal and relating to a function or operation (p. 7, original emphasis).

The false impression of resources as things that are static substances, fixed in tangible natural forms, continues to be the greatest obstacle to the advancement of research on forest resources. Large sums of money are spent annually in attempts to describe the biophysical properties of resources for purposes of inventory and management. Research on the beguiling "visual resource" is perhaps the most fascinating illustration of the linguistic capacity resource professionals have for falsifying the complex and dynamic character of experience — in this case the experience is the intangible aesthetic attribution of beauty to nature. Fifty years after Zimmerman first clarified the functional meaning of the resource concept many researchers still cling to the obsolete notions of resources as tangible substances[1]

Greater rigor in the use of the term "resource" demands that it be used as a relational "concept which legitimately belongs to the social scientist" (Zimmerman 1951, p. 10) rather than to the resource specialist. Unlike the tangible objects found in natural systems, resources change unpredictably in response to changes in societal tastes and values or changes in knowledge and technology. New resources emerge and old resources disappear with societal changes such as the invention of new wood processing technologies: scrub hardwoods are becoming a valuable source of raw material for making wafer board, and the market displacement of plywood by wafer board threatens to eliminate the need for large growth peeler logs for plywood stock. Socially defined tastes, values, knowledge and technology are extremely dynamic and, outside certain limits, are largely unpredictable. This makes the task of predicting resource requirements and production capabilities exceedingly difficult for all but short-range (5-10 year) resource planning. Before suggesting how an ecosystem-based approach can partially overcome this

[1]Again, I will resist a sociological analysis of the manifest and latent functions of linguistic conventions that define fixed properties of thing-like resources, but alert the reader to the possible sociological significance of professional forestry rituals involving both research on and management of such "resources". The more sophisticated reader will quickly appreciate the social fact that such naive resource definitions are themselves critical resources for those who manage forests, as well as for those who seek to influence their management.

limitation, I will suggest how the concepts of multiple use and site quality can be joined by incorporating a functional conception of resources.

Multiresource Site Quality

Site Qualities and the Resource Concept

The traditional concept of forest site quality concerned the problem of assessing the capability of a site to produce forest growth. Leary (1985) draws an analogy between a forest as a habitat for animals and a site as an environment for the forest. In each case he linked a "field" of influences (vegetation complex or soil/atmosphere complex) to a "test body" (animal or tree) through the latter as a "testing device" for "sensing" the "field" of influences. Emphasis on the performance of an organism in a given environment is preferred when knowledge of the workings of a soil system or ecosystem are inadequate for purposes of predicting how an organism will respond.

However, use of the organism as the "testing device" requires that responses to sociological and other contextual influences be separated from responses to vegetation or the soil complex. Attention should be focused on the immediate functional requirements for vegetative or the soil environment, while the influences of factors such as population density and reproduction levels need to be eliminated. Consequently, Leary (1985) notes that "the key individual in assessing the 'animal producing poser of the forest' is not so much the animal ecologist as the animal physiologist" (p. 3.25). Habitat quality is indicated by qualitative, physiologically-related attributes such as the size of horns in Dall sheep (Bunnell 1978) instead of the total number of animals in a localized population.

The analogy between habitat and site quality can be productively extended to a human-centered assessment. Biotic and abiotic objects in the natural environment constitute the "field" and the "test body" is the particular social function served by these objects. The resource concept is appropriate for specifying the relationship between social functions or operations and the natural objects that make them possible. At this point I suggest modification and extension of the resource concept to cover function relationships between animals and their habitats, as well as between forests and their environments. Hence, we can appropriately talk about resources for trees, animals and humans.

Just as with trees and animals, an organism-centered approach to assessment requires that attention be focused on identifying how natural objects serve particular functions in human societies. The influence of sociological and other contextual factors on these functions needs to be separated from the influence of the operational environment afforded by forest ecosystems.

Defining Resource Site Quality

The term "resource site quality" will be used generically to refer to the capacity of the biophysical environment to produce objects and their attributes that are required in order for natural organisms or human society to function. "Site" will denote a space of ground to be occupied by vegetation, organisms or human artifacts that perform essential functions for human society or nonhuman organisms. In the case of resources for trees (not timber), a moist and fertile soil is a requirement for rapid biological growth. These site qualities are essential for the physiological processes involved in rapid tree growth.

Similarly, timber resources for humans may be trees of sufficient height, girth, and freedom from defects to provide wood required for large construction timbers. Only those trees with attributes that enable them to yield timbers can perform this function. But, in addition to their attributes, these trees must be accessible and affordable before they can become timbers, and there must exist a body of knowledge, technology, and labor appropriate for extracting them from nature, transforming them into timbers, and assembling them into structures. However, timber site quality would be limited to relevant objects and attributes found in a delimitable space of ground that is capable of supporting tree

growth. In addition to the biological possibilities for growing suitable trees, accessibility, including surface conditions (swamps and rocks) and topography might be considered as relevant objects and attributes. The other conditions might also influence whether a timber-producing industry will prosper, but they are irrelevant for assessing site qualities. Let me now extend the analogy by linking site quality to multiple use.

Site Quality and Multiple Use

The conventional view of resources as tangible things has informed almost all discussion of multiple use. Considerable progress in conceptualizing multiple use was made by Leary (1985) when he considered "resource" to be a relational concept linking populations of natural objects with populations of prospective users. He also considered location and time as critical variables when reformulating multiple use, and included in a multiple use system the managers for timber, wildlife, range, and other resources. Human users were distinguished from non-human biological users, and abiotic and biotic populations of natural objects were treated separately. Multiple use was operationally defined in terms of relations between sets of biotic and abiotic natural objects, human and non-human users, managers, locations, and times. Multiple use statements were said to map the Cartesian products of these sets.

Leary (1985) states this as a propositional function that maps objects into statements—thereby achieving a formulation that conceives of multiple use in terms of its referents:

$MU: P_b \times P_a \times M \times U_b \times U_h \times L \times T \rightarrow S$

where:

MU designates the concept (predicate) "multiple use"

P_b denotes the set of biotic populations, i.e., trees, not timber; animals, not wildlife; shrubs, not brush, etc.

P_a denotes the set of abiotic populations, e.g., water bodies (streams and lakes, etc...), natural "mineral licks", etc.

M denotes the set of managers

U_b denotes the set of populations of biotic users of biotic and abiotic natural object populations

U_h denotes the set of populations of human users of biotic and abiotic natural object populations

L denotes the set of locations

T denotes the set of times

S designates the statements that use the concept

$\times$ denotes the mathematical symbol for the Cartesian product of sets, and

$\rightarrow$ designates mathematical mapping.

Improvements upon Leary's formulation can be made. Leary was not entirely successful in removing "resource" as one of the referents of multiple use. Although the resource concept was appropriately replaced by populations of objects and populations of users, Leary did not consider sufficiently the possibility of functional linkages between these sets. Analysis of multiple use can be simplified substantially by reintroducing the resource concept.

The set of managers most appropriately represents these functions in the present formulation. Resource managers naively act as apologists for particular social functions because, as has been explained above, they conform to linguistic conventions which lead them to focus on certain aspects of experience and to deny other aspects. Managers can be defined as resource agents who manipulate natural systems to facilitate some social functions and to inhibit others. Thus, by including managers, Leary unknowingly reintroduced the resource concept.

The need to consider relational concepts as central to multiresource site quality has led us to suggest a reformulation of Leary's propositional function. It is summarized here so that concepts may be clarified by comparing competing formulations. A different formulation is

suggested for purposes of measurement — to be discussed in the following section.

$$MU: E \times P \times C_n \times C_h \times D \times M_r \times L \times T \rightarrow S$$

where:

E denotes the set of populations constituting the abiotic environment, e.g. climate, soil, streams, etc.

P denotes the set of populations of primary producers, e.g., the photosynthetic "engine".

Cn denotes the set of populations of non-human consumers, e.g., grazers, browsers, fructivores, etc.

Ch denotes the set of populations of human consumers, e.g., wood gatherers, berry gatherers, mushroom gatherers, fishermen, recreational hikers, etc.

D denotes the set of populations of detrital feeders, e.g., fungi, molds, etc.

Mr denotes the set of populations of resource managers who purposefully manipulate all of the other sets to promote certain social functions, and other sets are as above.

The logic of sets and their possible relationship is specified more clearly by using the theory of ecosystem structure and process to reformulate the propositional function. Inclusion of human actors (collectivites, groups, or individuals) as consumers and managers makes it apparent that we can broaden the referents of the multiple use concept to include both "resources" and "resource relationships"[1] - two useful concepts eliminated in Leary's formulation. Most importantly, by extending the concept of resources to represent functional relationships between non-human objects, we are able to talk about a "chain" or "web" of "resource relationships" — even further broadening the referents of multiple use.

Multiple use is linked to site quality through the resource concept. Multiresource site quality assessment involves evaluating the capacity of a forest site to facilitate a variety functions involving both human and non-human organisms. The resource concept is fundamental because the first step in any such assessment is to ask, "For what purposes (functions) am I evaluating a forest site?" Site quality assessment is a resource driven process.

Assessment methods for several individual resources have been developed and continue to be refined. The above discussion of timber site quality and wildlife habitat illustrates such methods. However, assessment involving more than one resource is only now emerging. The function concept of resources, when coupled with the reformulated concept of multiple use, provides a rigorous terminology for developing such methods.

MEASURING MUTLIRESOURCE SITE QUALITY

We are pursuing the further development and refinement of one such approach in our work in the Pacific Northwest. Our purpose is to link specific resources to the changing state of forest ecosystems as they recover from disturbance (especially harvesting).

There is an important reason for using an ecosystems based approach. Societally-defined resources are inherently unstable. As a result, their measurement tends to be imprecise and frequently outmoded. Even such relatively stable technologies as sawmilling change so rapidly that timber inventories based on utilization standards from the 1960's are

[1] "Resource relationships" are central to multiple use concerns because they underly most user (consumer) conflicts. Competition between social functions may occur indirectly in the form of "resource conflicts" as well as directly in market processes, interpersonal conflict, or political and legal conflict. Incompatible uses for a tree, such as timber versus a nesting site for eagles, illustrates a "resource conflict", whereas crowding of the tree by other vegetation would constitute a "conflict" between natural objects.

inadequate for present conditions. Recreation tastes and preferences have changed even more rapidly with the adoption of off-road vehicles and other forms of modern technology.

Methods for simulating the dynamic behavior of forest ecosystems will provide a means for predicting populations of objects and attributes that facilitate a wide variety of functions. Emphasis on site conditions that support resource functions rather than on resources themselves will assure a more durable and precise basis for long-term multiresource assessment. Such ecosystem models can be developed and refined through cumulative research efforts, while resource studies often have a very limited useful life. Less sophisticated methods can be used to link resources to the state of forest ecosystems.

We are presently evaluating the use of a systems dynamic model that simulates ecological processes to provide a projection of resources in biologically possible combinations. The system, developed by Boyce (1977, 1980), is named DYNAST — acronym for "Dynamically Analytic Silviculture Technique". DYNAST consists of a core ecological model that simulates successional recovery following a disturbance (in this case, harvesting). Input to this core model consists of manager-induced interruptions in the normal recovery process: silvicultural controls of rotation length, harvest rate, size of harvest units, and the type and rate of species conversion. Output from the core consists of a simulation of forest structure at various times in the future: proportion of the forest in specific age/species classes, stand area, and vegetation types.

The multiresource capability of DYNAST is contained in modular algorithms for specific objects, attributes, or resources: wildlife, timber, scenery, recreation, water, forage, and so forth. A module can be written for any resource, object or attribute that can be linked directly to forest structure. All resources or objects are treated equally, and are constrained only by what is ecologically possible. In this sense, the system does not consider conflicts between resources or consumers. Hence, DYNAST is ideally suited for assessing the qualities of a forest site for which silvicultural practices are the disturbances.

Most of our research focuses on the development of modules for specific resources. The algorithms contained in each module enable us to "measure" how each resource will be affected by specific silvicultural practices — thus providing an assessment of how relevant qualities of a site are altered by various practices.

Existing literature is the primary source for assembling an inventory of propositions linking the specific resources to forest structure. Where existing propositions are lacking, in conflict with one another, or vague, original research is undertaken to develop and/or test propositions. We have found this to be an especially efficient approach for constructing an information base suitable for multiresource site assessment.

CONCLUSION

This paper has provided a rigorous terminology for analyzing the capability of forest lands to produce a variety of goods and services. It has also suggested an approach for measuring resources production as a function of ecological change induced by forest management. These advances in terminology and measurement are expected to result in greater emphasis on the significance of ecosystem structure and processes. Correspondingly, emphasis on the need for resource inventories is expected to diminish. Assessment of multiresource site quality will very likely lead to the conclusion that there are related site requirements for a large number of resources and that these requirements can be defined in terms of the structural and functional properties of forest ecosystems. Let me suggest some possible forms that these requirements may take.

We hypothesize that the following two general properties of forest ecosystems will provide sets of attributes required by a wide variety of resources (and anticipate that additional properties will be identified):

•Diversity in the structure and composition of forests
•Capability of forests to resist disturbances and to restore themselves following disturbance.

Diversity in both structure and species composition is related to smaller harvest units and a spatial distribution of unit ages that increases ecological edges and interrupts regularized patterns over large areas. Studies have shown how such diversity provides habitat conditions suitable for a wide variety of animal species. It is also related to the requirements of recreational and scenic resources.

The resiliency of forest ecosystems is important for assuring appropriate temporal distribution of resources. Forest ecosystems that require decades to recover from disturbances will be less likely to provide attributes associated with advanced successional stages, and a large share of forests in a region subject to frequent disturbances may produce only a few such benefits for very long periods of time.

Some may be intrigued by the close resemblance of these requirements to the conservative forest management traditions found in Germany (Plochmann 1981) and other European countries. Such convergence should not be taken as evidence that there is a "right" way to manage forests. Rather, it should indicate that the ecological possibilities for multiresource production are limited to a narrower set of conditions than we might have otherwise assumed.

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Concepts of Special Significance in Forest Pest Management

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Abstract. Integrated Pest Management (IPM) is defined as the maintenance of destructive agents, including insects and diseases, at tolerable levels by the planned use of various techniques that are ecologically sound, economically efficient, and socially acceptable. A general conceptual model of a forest pest management system was presented. The basic components include: (1) pest population dynamics, (2) forest stand dynamics, (3) treatment techniques, (4) impact on resource values, (5) benefit-cost analysis, (6) monitoring of pests and forest stands, and (7) transferring of pest management information/knowledge to the user community. Fourteen concepts of special significance in forest pest management were briefly examined. Each concept was defined and examples given when appropriate.

INTRODUCTION

The ecological, economic, and social values of the forests have increased considerably during recent years. We must be able to manage perturbations caused by insects and diseases as it becomes necessary to more efficiently utilize our forest resources. The philosophy, concepts, and techniques used in forestry and agriculture today to handle destructive insects and diseases is referred to as IPM. IPM is defined as the maintenance of destructive agents, including insects and diseases, at tolerable levels by the planned use of various techniques that are ecologically sound, economically efficient, and socially acceptable. Additional information on the development, concepts, and evolution of forest insect pest management is provided by: Stark and Gittin 1973, NAS 1975, Apple and Smith 1976, Waters and Cowling 1976, Stark 1977, Waters and Stark 1980, Coulson 1981, Coulson and Witter 1984, and Berryman 1986.

Foresters and forest entomologists have recognized certain key relationships between pests and forest resource management for many years (Knight and Heikkenen 1980). Waters (1978) mentions three key issues: (1) forest insects are integral components of forest ecosystems, (2) activities of insects and diseases can have major effects on forest stand growth and productivity, and (3) insects and diseases can be disruptive to forest management objectives and schedules. The key issues that IPM addresses are not new, but much of the basic research that has greatly increased our understanding of IPM was conducted from 1972 to 1985. Therefore, the implementation of IPM systems at the forest level is just beginning to occur.

COMPONENTS OF AN IPM PROGRAM

The interdisciplinary and multidisciplinary research and application programs on major forest insect pest problems were primarily responsible for developing the major concepts of IPM in forestry. These key "big bug" projects were in operation during the 1970s and/or early 1980s, and involved the following pests: (1) gypsy moth, *Lymantria dispar* (Linnaeus), (2) bark beetles of the genus *Dendroctonus* and *Ips*, (3) Douglas-fir tussock moth, *Orgyia pseudotsugata* (McDunnough), and (4) spruce budworms, *Choristoneura fumiferana* (Clemens) and *Choristoneura occidentalis* (Freeman). Certain unifying principles and concepts emerged, even though different insects and forest ecosystems were studied.

The general conceptual model of a forest pest management system that evolved is presented in Fig. 1. The basic components of an IPM program include: (1) pest population dynamics, (2) forest stand dynamics, (3) treatment techniques, (4) impact on resource values, (5) benefit-cost analysis, (6) monitoring of pests and stands, and (7) transferring of pest management information/ knowledge to the user.

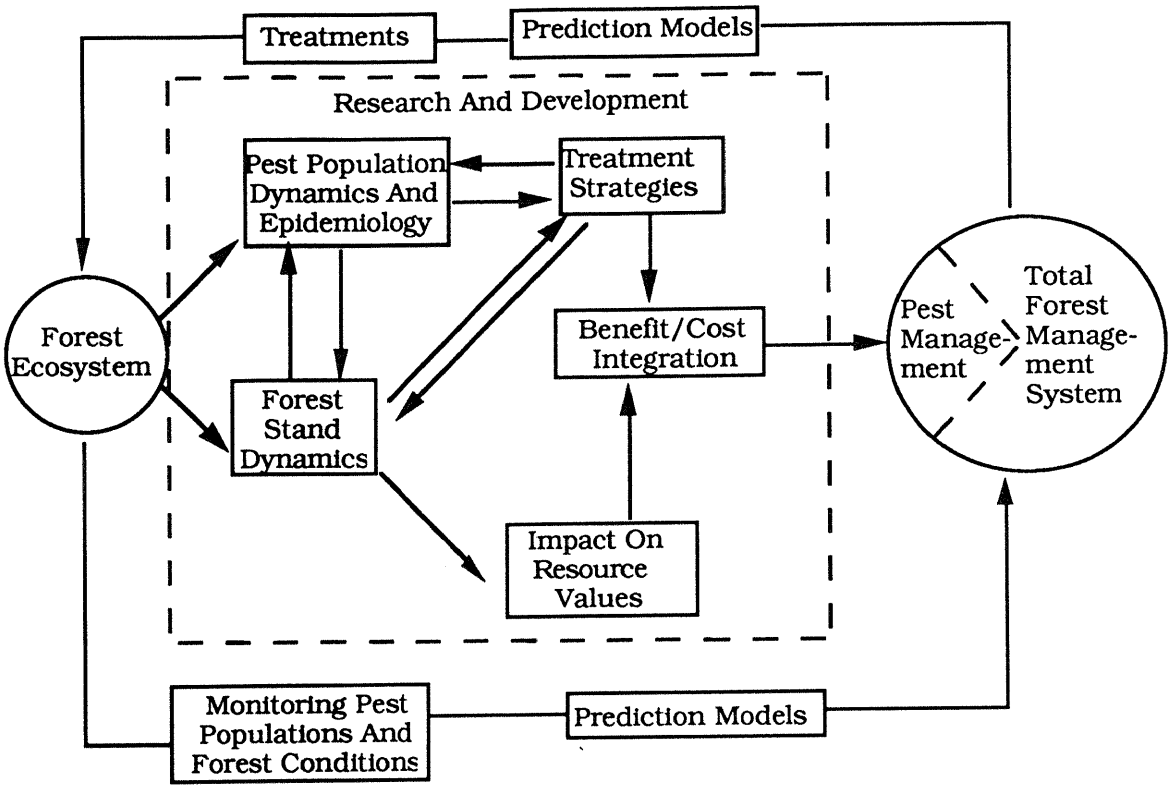

Figure 1. General conceptual model of a forest management system (From Waters and Cowling 1976).

CONCEPTS

The remainder of the paper focuses on fourteen concepts that I feel are of special significance in forest pest management. The specific examples used to back up the concepts come from the four "big bug" projects or from other pest insects of northern ecosystems, since most attendees at this symposium work in northern ecosystems. Additional information on these concepts are presented in Coulson and Witter (1984).

Concept I. Extent of IPM use in forestry depends on the forestry situation involved

Coulson and Witter (1984) described four forestry situations where forest insects are considered pests: (1) forest ecosystems, (2) specialized forest settings, (3) urban forests, and (4) manufactured wood products and structures. The principles of IPM in forestry were developed at the ecosystem level of organization. The IPM programs available today in forestry are used on extensively and intensively managed forest ecosystems. Examples of currently available and/or useful IPM programs are: (1) Douglas-fir tussock moth (Brookes et al. 1978), (2) gypsy moth (Doane and McManus 1981), (3) Saratoga spittlebug (Heyd and Wilson 1981, Coulson and Witter, pp. 437-438, 1984), (4) spruce budworm (Montgomery et al. 1984, Simmons and Montgomery 1985), (5) southern pine beetle (Thatcher et al. 1980), and (6) western spruce budworm (Brookes et al. 1987a,b,c).

Specialized forestry settings include forest nurseries, seed orchards, Christmas tree plantations, wind breaks, arboretums, and research plots. The components of an IPM program apply to specialized forest settings, as well as forest ecosystems. IPM practices on specialized forest settings are more like the practices used in traditional agriculture than those used on forest ecosystems. The major difference between IPM on specialized forest settings and forest

ecosystems is the heavy use of pesticides in specialized forest settings. Cameron (1981) presents an IPM program for southern pine seed orchards.

Pests in the urban forests are normally controlled with insecticides or ignored. Few IPM programs have been developed for urban forests. Montgomery et al. (1988) developed an IPM program for gypsy moth in Michigan's cities and suburbs.

Managing of pests attacking manufactured wood products and structures are usually done by the structural pest control industry. Their approach relies primarily on preventive wood treatment, pesticides, and sanitation; conceptually, it differs significantly from the IPM concept.

Concept II. There is a forest resource that needs protection from pests.

"Pests" is an anthropocentric definition given to forest insects and other organisms when they adversely affect the ecological, economic, and social values that we associate with forest and urban trees. We assume that this forest resource is valuable or there is no justification in protecting the resource from the pest. Examples of forest resources that may need protection from pests are: wood fiber, logs, aesthetic value of forests, state park campgrounds, water quantity and quality, wildlife cover, and stream habitat.

Concept III. Actual importance of a pest species is determined by evaluating the effects on values associated with a resource.

A forest pest can adversely or beneficially affect the following values in a forest: timber; water quantity and quality; fish and wildlife habitat and/or numbers; and recreational activities such as camping, hiking, fishing, boating, hunting, and observing nature. Huff et al. (1984) provides detailed information on the effects of the spruce budworm on spruce-fir forests in the Lakes States using a static economic model which established the nature of the Lake States spruce-fir market and a comparative economic model which examined changes brought about by spruce budworm outbreaks. Brookes et al. (1987a,b,c), Witter et al. (1984), Thatcher et al. (1980), and Brookes et al. (1978) present detailed information on evaluating the effects of western spruce budworm, spruce budworm, southern pine beetle, Douglas-fir tussock moth, respectively, and on various kinds of values associated with the forest resource.

Concept IV. The amount of damage associated with a particular pest is often related to the availability of a preferred plant module(s).

In describing plant-animal interactions, it is useful to employ the concept of a tree as a modular organism (Harper 1977, 1981). A module is defined as a repeated unit of a multicellular structure which is arranged in a branched system. A forest tree consists of different kinds of modules: main stem, branches, foliage, reproductive structures, and roots (Fig. 2).

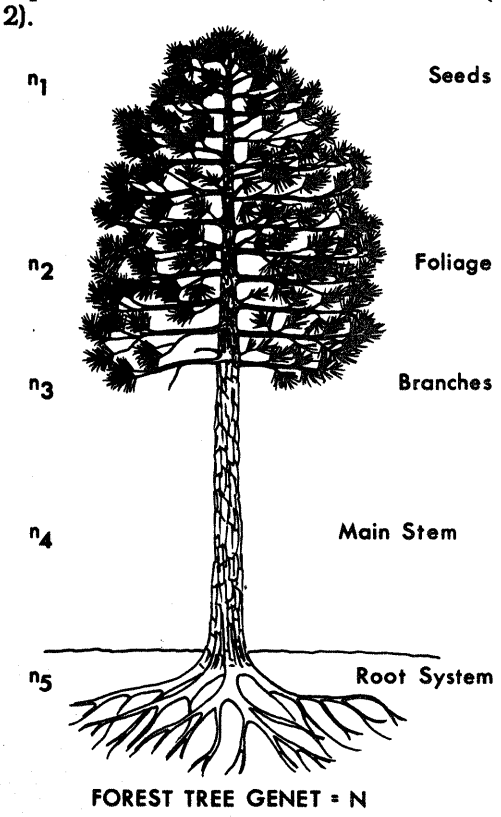

Figure 2. Diagram of a forest tree genet (N) composed of five types of structural modules (n_1-n_5).

The amount of a particular module present within a tree or stand varies considerably through space and time. For example, Witter and Waisanen (1978) showed a very significant relationship between the mean proportion of buds infested with tortricid caterpillars and the mean flushing date of aspen clones. In other words, the earlier flushing clones had a much higher proportion of buds infested than the later flushing clones.

Concept V. Mathematical models of forest trees provide information necessary for evaluation of impact at the stand level.

Stand prognosis models are used to predict the future growth of forest stands, including effects of outbreaks of forest insect populations. The stand-prognosis model represents our knowledge of how trees grow and how that growth is modified by pest management and silvicultural activities (Fig. 3). Brookes et al. (1978) and Wykoff et al. (1982) provide additional information on the use of stand prognosis models.

Concept VI. Most insects in the forests are either directly or indirectly beneficial or neutral in their relationship to humans.

It is erroneous to regard all insects as pests. Estimates of the number of insect species that occasionally cause damage in the United States vary from about 750 to 1500 species depending on the evaluation criteria used. Less than 1% of all the described species in North America are major insect pests. The majority of insects are either directly or indirectly beneficial or neutral in their relationship to human beings. Insects pollinate plants, provide food for other animals and products for human consumption, serve as natural control agents of other pests, enrich the soil, and contribute to the educational and aesthetic experience of human beings.

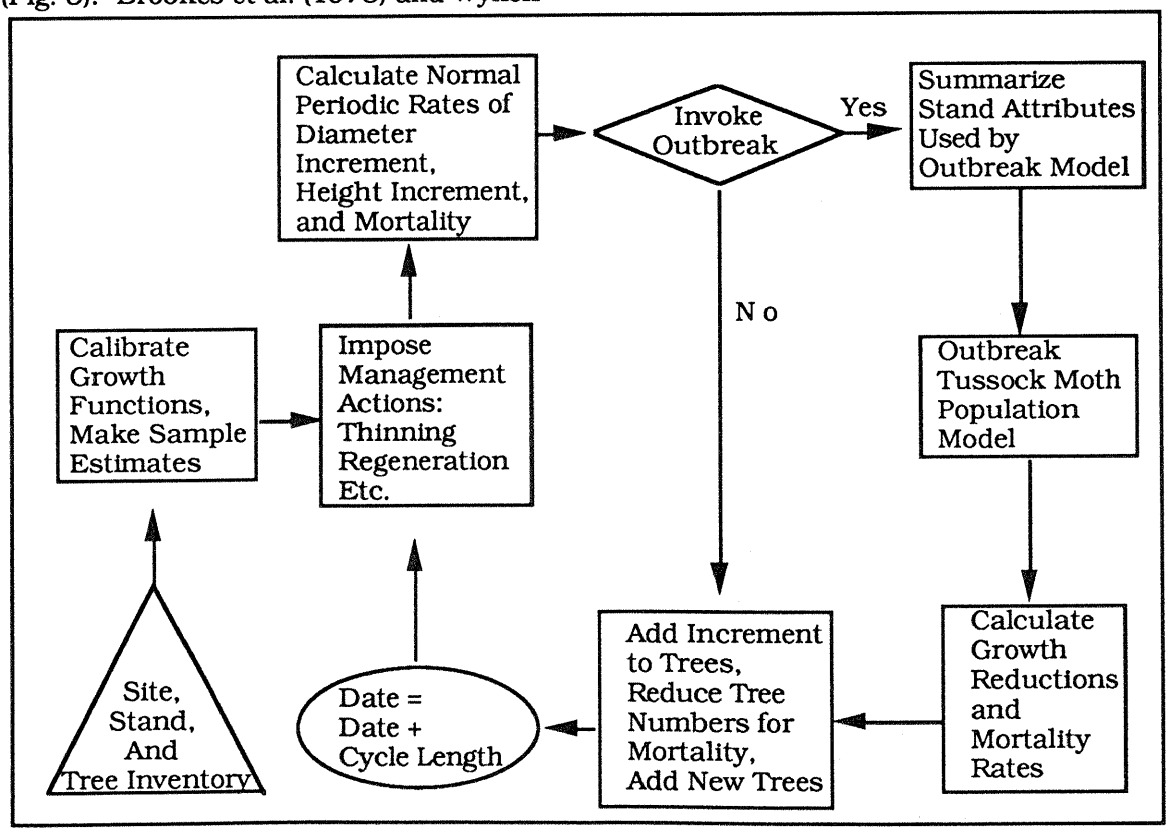

Figure 3. Stand prognosis model for Douglas-fir tussock moth (modified from Brookes et al. 1978).

Concept VII. Understanding the population dynamics of pest insects is essential so that we can describe how a system will operate under different conditions.

It is necessary to consider a population as a system of interacting components to understand how and why insect distribution and abundance change through space and time. The basic components of a population system are: (1) environmental properties, (2) individual properties, (3) population processes, and (4) population state variables (Fig. 4). Coulson and Witter (1984), Berryman (1981,1982), and Clark et al. (1967) provide additional information on the components of the population system and factors responsible for changes in distribution and abundance of populations.

Concept VIII. Forest insect outbreaks can be classified into six types.

Berryman (1986) identified six types of insect outbreaks which were divided into two broad classes. The broad classes are eruptive outbreaks, characterized by their self-perpetuating spreading nature, and gradient outbreaks, characterized by arising and subsiding in place in response to external environmental conditions. The outbreak subclasses reflects the tendency for an insect population to cycle with regular periodicity, to decline rapidly after reaching outbreak numbers, or to persist in place for several insect generations. Berryman (1986) describes the six types of outbreaks as: (1) Sustained eruption – persist at high densities for several to many years at any one location and host plants only die after many years of attack, if at all, (2) Cyclical eruption – occurs at regular intervals (e.g., 8 to 11 years apart), and never causes severe or widespread mortality of host plants, (3) Pulse eruption – occurs at irregular intervals and often causes widespread mortality to host plants, or is quickly terminated by natural enemies, (4) Cyclical gradient – occurs at regular intervals (e.g., 8 to 11 years apart), rarely causes extensive mortality to host plants, often associated with particular site and stand conditions, and is usually terminated by natural enemies

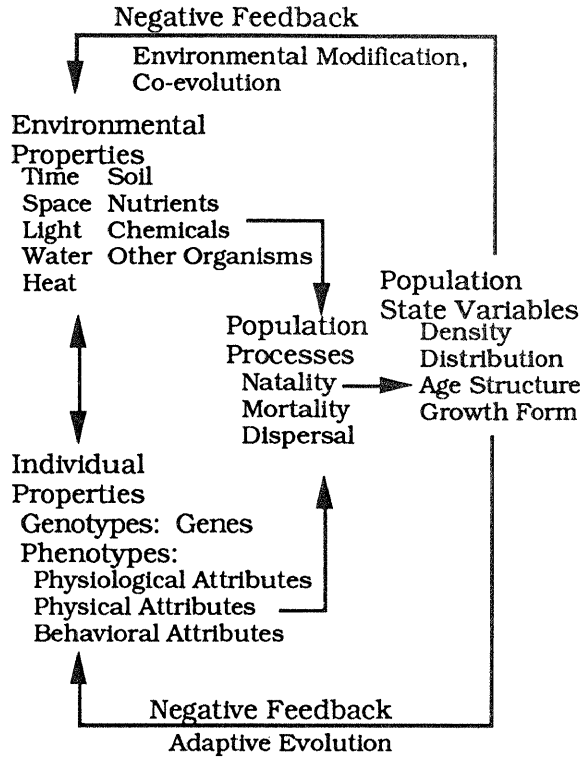

Figure 4. Basic components of a population system (Modified from Berryman 1981).

or by host-defensive responses, (5) Sustained gradient – occurs more or less continuously on particular sites and stands, and (6) Pulse gradient – occurs at irregular intervals, following major environmental disturbances or outbreaks of other organisms, and subsides soon after environmental conditions return to normal. Land managers will be better able to create conditions where insect populations are maintained at tolerable levels when the managers understand the basic insect outbreak patterns and the feedback processes that give rise to the various outbreak patterns.

Concept IX. Monitoring pest populations and forest stands is a necessary component of IPM.

Normally, monitoring of forest stand conditions and pest population numbers are undertaken as separate surveys. They are represented at the periphery of Fig. 1 linking activities taking place in the forest ecosystem to predictive models

and finally to forest resource management. Monitoring the conditions of forest stands are often done using aircraft, remote sensing techniques, video cameras, GIS, and computers. Monitoring insect populations varies considerably depending on the specific forest insect. See Sanders et al. (1985), Coulson and Witter (1984), Doane and McManus (1981), Thatcher et al. (1980), and Brookes et al. (1978) for specific techniques used to monitor various pest populations.

Concept X. The land manager uses cost-benefit analysis as a tool to make judgments on what actions to take.

If the land manager believes that there is a problem from an insect outbreak, judgments must be made on what action to take (treatment or no treatment). The land manager then evaluates all possible treatments and the impact that the treatment will have on resource values. Cost-benefit analysis is the procedure used to make judgments (Sassone and Schaffer 1978). One chooses the action that gives the greatest positive difference between costs and benefits.

Concept XI. A number of techniques often are available to reduce pest populations.

Information on pest population dynamics, stand dynamics, impacts on resources values, and costs of application are used to determine if a treatment(s) is needed and what technique(s) to use. Techniques used to suppress populations are: (1) chemicals, including insecticides, behavioral chemicals, and hormones, (2) biological control using natural enemies such as insect parasites, insect predators, avian predators, and pathogens such as bacteria, viruses, or fungi, and (3) mechanical control such as trapping insects, destroying habitat, modifying habitat, or collecting insects from the host. Techniques used to prevent insect outbreaks include silvicultural methods and regulatory practices such as quarantines or containment and suppression programs designed to prevent spread of pests into new areas. Additional information on techniques used to reduce forest pest populations is provided by Knight and Heikkenen (1980), Coulson and Witter (1984), and Berryman (1986).

Concept XII. IPM programs can be simple or complex.

IPM refers to the philosophy, concepts, and techniques that are used to handle destructive forest pests. Therefore, a successful program may be fairly simple or complex. The general conceptual model (Fig. 1) was developed for large complex insect/stand interactions and implies that each component is a complex subsystem, and ideally consists of detailed information which is abstracted in the form of a predictive mathematical model (Coulson and Witter 1984).

A relatively simple IPM plan is being used for reducing Saratoga spittlebug damage on red pine in Michigan (Fig. 5, Heyd and Wilson 1981). A proposed planting site or an established plantation is risk-rated for Saratoga spittlebug damage. Saratoga spittlebug causes economic damage only when suitable alternate hosts are present. Heavy infestation of the Saratoga spittlebug is normally correlated with density of sweet fern. Risk-rating of proposed planting sites or young plantations under 5 m tall is conducted during the spring or summer when alternate hosts are present. The forester or pest management specialist determines the percentage of ground cover occupied by sweet fern at chosen intervals within the site. A risk-rating triangle is used to determine whether the area is of low, moderate, or high risk. Data for the entire stand are sketched onto a map. With this survey information, the land manager can apply preventive, silvicultural, or chemical techniques. The prevention technique involves restricting red pine to only non-risk or low-risk areas. Silvicultural techniques that reduce the number of alternate host plants are deep plowing, shallow plowing, or chemical herbicides. Registered insecticides may be used effectively against the adult spittlebug if a spittlebug problem develops in an established plantation. The land

manager uses a net present value analysis for each management strategy before making the final decision on technique(s) applied to a particular site or plantation.

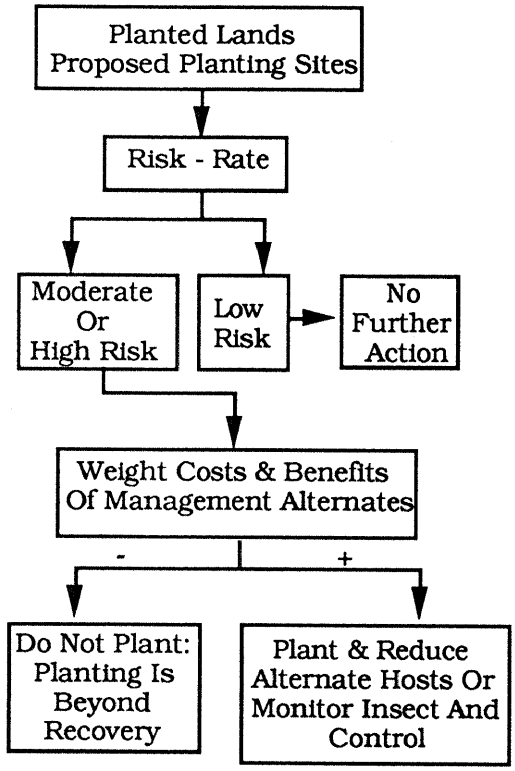

Figure 5. Flowchart of steps in determining management strategies for red pine based on potential susceptibility to damage by Saratoga spittlebug (After Heyd and Wilson 1981).

Concept XIII. Pest management is a component of forest resource management.

This is a very important concept that entomologists and pathologists often forget or downplay. A pest management program for an organization (ie., national forest, state forest) should not be a stand-alone program, but must be incorporated into the long-term plan for the organization. Specifically, insects and diseases need to be considered when the forester enters the compartment every 10 years so that it becomes primarily a preventive philosophy for control instead of a suppressive philosophy for control.

Concept XIV. It is essential that the forest pest management specialist: (a) has a plan for transferring technology to the user community and lay public, and (b) uses the plan to inform and educate the user community and lay public about the pest problem.

Witter et al. (1982) and Montgomery et al. (1984) document a very successful technology transfer program that was developed and used from 1981 to 1985 to provide land managers in the Lake States with pest management information on the spruce budworm. It is important to review this technology transfer program, because, unlike agriculture, forestry has few examples of well-organized or time-tested technology transfer programs. Montgomery et al. (1984) offers the following advice on developing a technology transfer program: (1) Understand the various institutional forces and constraints within which you operate, (2) Focus your attention on the problem and identify your target audience, (3) Involve people relevant to the issue and establish an advisory committee to oversee project activities, (4) Review past and current research activities that relate to the problem, (5) Review other technology transfer efforts in forestry and natural resources; incorporate their strong points into your plan, when appropriate, (6) Develop a short, simple,and flexible plan, (7) Clearly state your objectives, (8) Get to know your audience, and understand their perceptions and beliefs regarding the problem, (9) Learn what factors help and hinder the technology transfer and innovation decision process, (10) Base your choice of what information to disseminate on needs assessment surveys and the expertise of your staff; (11) Base your choice of media on the type and size of audience and the message being disseminated, (12) Consider using "information movers" (e.g., cooperative forestry extension agents) as your primary audience or divide your audience into smaller groups if your target audience is large, (13) evaluate your process, products, and impact of program, (14) Realize that ideas, objects, and practices will be adopted and implemented at different

rates, and (15) Attempt to institutionalize your program.

Methods commonly used to inform and educate the user community and lay public about pest management problems include newspaper articles, magazines, manuals, handbooks, leaflets, workshops, radio and TV spots, computer software packages, talk shows, meetings, films, and personal contacts.

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STRATEGIES FOR SETTING RESEARCH PRIORITIES IN DISCOVERING NEW KNOWLEDGE ABOUT FORESTS

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Abstract. Strategies for setting research priorities in discovering new knowledge about forests must be developed with a systems perspective of forestry research. Research priorities should reflect the multiple value systems of the many actors who play important roles in making decisions within forestry research systems. In developing research priorities it is important to recognize and, if possible, reconcile the different value systems of those who fund research, manage research, do research, disseminate research, and use research results. In modern, technology-based societies, choices about science inevitably involve more than the professional concerns of scientists.

INTRODUCTION

Forty years ago Vannevar Bush (1945) aptly characterized science as "The Endless Frontier". Today scientific research still seems to raise more questions than it answers, and scientific frontiers continue to expand with no end in sight. In any field of science many questions remain unanswered that could be the subject for research. Some of these questions may be critical to solving important scientific or social problems. We don't have enough resources, people, funds, facilities, and time, to answer all scientific questions that arise in the course of research. In forestry research, as in other fields of science, we are forced to make choices of what research projects or programs to work on. We must set priorities on what, how, when, and where to study. In setting research priorities the tough job is to select important problems that are do-able within a reasonable time and within an allowable budget.

In the limited time we have today I want to explore with you some of the factors that should be considered in developing strategies for setting research priorities in discovering new knowledge about forests. This will be a brief review that can only hint at the complexity of the subject.

FACTORS AFFECTING THE SETTING OF RESEARCH PRIORITIES

In modern technology-based societies, choices about science inevitably involve more than the professional concerns of scientists. People in modern societies are deeply affected by direct and second-order consequences of technological innovations that arise from the growth of scientific knowledge produced by research. Scientific research has become an integral part of the economic and social structure of our society. In a political context it is important to recognize and address the concerns and value systems of the different power groups involved in research decisions. I shall argue that in deciding what research to do in forestry, in developing strategies for setting research priorities, it is important to go beyond the immediate professional concerns of scientists and consider the concerns of others in society who take part in and are affected by forestry research decisions.

Much of the philosophy of science literature concentrates on developing logical-scientific methodologies and criteria that underly choices of topics for scientific research and research methodologies (for example, Platt 1964 and Bunge 1967). This is critically important in improving the scientific effectiveness of research. Yet, I would argue that this addresses only part of the problem of setting research priorities.

We recognize that there is a strong irrational/illogical element in scientific research and discovery (Brown 1977). The history of science recounts many instances of major scientific discoveries being made through blinding flashes of intuition when least expected. Strategies for setting research priorities should allow for nonrational elements in the process of scientific research. We know that scientific discovery is not a deterministic activity. Rather, it contains a large measure of uncertainty. Forestry research is not a tightly structured, closed, deterministic system that lends itself to quantification and prediction. Rather, it is a loosely structured, open, evolutionary, uncertain system that is difficult to quantify and defies precise prediction. Because science is evolutionary it is difficult to anticipate the future development of a given field of science, the acceptance of new scientific discoveries by future scientific communities, and potential impacts of such discoveries on science and society. This greatly complicates the planning and evaluation of future research projects and programs. Strategies for setting research priorities must reflect this basic uncertainty.

We are aware that science is a social activity that takes place within a community of scientific peers (Mulkay 1977). Knowledge does not become a part of science until it is shared publicly with others in the scientific community. The accumulated acknowledged body of scientific knowledge helps to determine research priorities within a scientific field.

Science is strongly dependent on the rest of society for the financial support of research and for the education and training of future scientists, among other things. Science also strongly influences technology and thus affects the lives of people throughout society in many ways, directly and indirectly (Ziman 1976). The voices of interest groups in society may play an important role in shaping research priorities.

All of these factors, and others, should be considered as part of a strategy for setting research priorities in forestry.

A SYSTEMS VIEW OF FORESTRY RESEARCH

Strategies for setting research priorities in discovering new knowledge about forests must be developed with a systems perspective of forestry research. The forestry research system invovlves more than the actual doing of research, more than the interests of scientists and their value systems regarding scientific research. Forestry research is part of society and has a broad influence throughout society. Many different groups of people besides scientists are part of and have a direct or indirect interest in forestry research. These different groups influence what, where, when, and how research is done. They frequently have differing value systems that reflect their own interests, education, training, and background. In developing research priorities it is important to recognize and, if possible, reconcile the different value systems of those who fund research, manage research, do research, disseminate research, and use research results, as well as all those throughout society who ultimately are affected by the choice of new technologies. Criteria for establishing forestry research priorities should reflect these multiple value systems.

In looking at the forestry research system one can identify five major groups of people in society who are involved, directly or indirectly, in forestry research:

- Those who fund research;
- Those who do research;
- Those who disseminate research findings;
- Those who use research results;
- Those who are affected by the use of research results.

Each of these groups has an interest in the development of forestry research priorities.

Funders of Research — Federal and State legislative bodies, government agencies, and other public and private sources who fund research must judge and select from among a host of competing programs, both research and non-research. The decision makers and their staff have

organizational and personal values that affect their choices. These must be considered in developing research priorities.

Doers of Research — Much of forestry research is carried out by government research organizations and by university forestry schools and agricultural experiment stations. Organizational administrators have to justify research programs, funding levels, and facilities to those who fund research and to others. The priorities they place on research projects and programs reflect their concerns. Research managers must organize the people, facilities, and funds available to them to accomplish research goals. The priorities they place on forestry research reflect their responsibilities. Research scientists set research priorities based on their own concerns. They must use the skills and resources at their disposal to conduct research successfully and produce results that will meet with the critical approval of their peers. In establishing research priorities the doers of research must somehow reconcile the differing interests and value systems of research administrators, managers, and scientists.

Disseminators of Research Findings — Research findings must be disseminated to their intended users if they are to be of value to science or to society. Many are involved in this dissemination process: individual researchers, research organizations, extension personnel, teachers, professional journals, professional societies, libraries, book publishers, other media, and perhaps most important, other users of such research results. All of these help shape the priorities placed on research through their role as gatekeepers in the transmission of research results to potential users.

Users of Research Findings — Research scientists, as scientific peers reviewing project proposals, study plans, and other documents relating to proposed research in forestry, greatly influence research priorities. Through their critical comments about completed, current, and proposed research they can affect the selection of research problems and methodologies. Field foresters and others who expect to apply research results can influence priorities placed on proposed research.

The Public Affected By Research — Individuals, groups, and organizations outside of the research organizations may be impacted by a proposed line of research. They may use their political influence or use other means to influence the choice of research topic, the research methodology, or the level of funding provided for a given type of research.

In the research process there are several actions that must be undertaken. Researchers must:

- Decide what research to do. This is the strategic phase of research.
- Decide how to do the research. This is the tactical phase of research.
- Do the research. This is the operations phase of research.
- Communicate research results to scientist peers and other potential users. This is the diffusion phase of research.
- Monitor use of results and take action to correct defficiencies detected in the research process. This is the feedback phase of research.

Each of these research activities requires the involvement of different groups of people who are part of the research system. Each activity requires different information about the system. In setting research priorities each activity should be considered and evaluated for each proposed research project or program.

STRATEGIES FOR SETTING RESEARCH PRIORITIES

In developing strategies for setting research priorities in forestry it is important to recognize and incorporate the value systems of the different groups of people involved in forestry research. The people involved in a proposed research project or program depend in part on the type of research being done. To determine the type of people who might be involved in research, it is helpful to distinguish between basic and applied research, although this is not

always easy to do. These two types of research affect science and society in different ways and involve different groups of people.

It is helpful to think of basic and applied research as two ends of a continuum, rather than as two sharply divided classes of research. At one end of the spectrum lies basic research. For this discussion I will define basic research as research aimed at producing knowledge about or an understanding of some aspect of the natural or human world, to be used by other research scientists as an input to further research. Thus, the primary users of results from basic research are other scientists, and perhaps some scholars and educators. The results would be published in scholarly scientific journals, to be read by peer scientists. In other words, basic research is done to provide input to more research.

In contrast, at the other end of the spectrum is applied research. Here the aim is to produce knowledge, information, or new technologies that can be used to change the way things are done in the world around us. The intended users of results from applied research would be those people outside of the research establishment who do things. In forestry this might include land managers, forest product harvesters, forest products industries, and other groups of people who use forests and their related goods and services.

Much research may fall somewhere between these two extremes, producing knowledge, information, and new technologies that may in part be used as input to future research and in part as input to change the way things are done in the world around us.

If proposed research is primarily basic, then the important values to be considered are the potential contributions that this research might make to science. Such contributions to science are difficult to quantify and involve peer judgement to a large degree.

If the proposed research is mainly applied, and intended to change the way things are done in society, then the important values to be considered are the anticipated kinds and intensities of changes, and the extent of their social, economic, and environmental impacts.

Different strategies are needed to determine research priorities in each of these two extreme cases.

It is important to recognize that there is no one overall system for setting research priorities in forestry. Each organization, each of the different groups of people involved in the forestry research process, has its own strategy for determining priorities. The outcome is likely to be a set of conflicting research priorities that cannot be resolved analytically, but only through some political bargaining process among various interest groups.

CRITERIA FOR SETTING RESEARCH PRIORITIES

If we want to set priorities on several potential research problems or projects, then we must evaluate these research alternatives according to some criteria. Many criteria have been suggested for evaluating research projects. We have time only to mention briefly two sets of criteria that would be useful in setting research priorities.

The National Science Foundation[1] has used four criteria to evaluate research proposals:

1. Research performance competence —
   Are the investigators capable?
   Is the approach technically sound?
   Are there adequate institutional resources available?
2. Intrinsic merit of the research —
   Is it likely that the research will lead to new discoveries or fundamental advances within its field of science or engineering, or have substantial impact on progress in that field or in other scientific and engineering fields?
3. Utility or relevance of the research —
   Is it likely that the research can contribute to the achievement of a

[1] From APPENDIX B (Attached to NSB - 81-384). Approved by NSB, 8/21/81.

goal that is extrinsic to or in addition to the goals of the research field itself?
Is it likely to serve as the basis for new or improved technology, or assist in solving societal problems?

4. Effect of the research on the infrastructure of science and engineering —
Does this research have the potential to improve the understanding, quality, distribution, or effectiveness of the Nation's scientific and engineering research, education, and manpower base?

In addition, I suggest that we should ask of each alternative:
Does this research problem fit with the unit's research mission?
Will its solution help to achieve the unit's goals?
Can the choice be justified to those who will fund the research?
Can it be done?
Are the skills and resources available to do the research within a reasonable time?
Is there a reasonable chance of success?
If not, are the potential gains if successful large enough to offset the low probability of success?
Would the results be used?
Could the results be used by the people for whom they are intended?
By others?
Are they likely to be used?
Is the problem important?
Would the results be widely used?
If the results were widely used, what differences would it make?
What would be the impacts of their use? How much, where and when?
What would be affected, and to what extent?
Who would be affected?

CONCLUSION

I think we all could agree that with limited resources available for research it is important to develop strategies for setting priorities on potential topics for research. I have suggested that such strategies must:

- Insure that basic logical-scientific methodologies and criteria are met.
- Allow for the nonrational elements and uncertainties inherent in research.
- Recognize the social dimensions to scientific research, the interactions among scientific peers and the interrelations of science and society.
- Incorporate a systems perspective of research and address the different value systems of the diverse groups that play a key role in the forestry research system, including those who:
  Fund research;
  Do research;
  Disseminate research findings;
  Use research results; and
  Are affected by the use of research results.
- Recognize that separate strategies are needed for basic and applied research. Priorities for basic research are set primarily by those scientists who do the research and use the results. Priorities for applied research must consider potential impacts on society.

Many criteria have been suggested for determining research priorities. One of the most simple but profound sets of criteria I have encountered was given by Dr. Charles Muscoplat, Molecular Genetics Inc., Minneapolis, Minnesota, at a meeting on research policy in March 1982. He suggested three criteria that any research proposal must meet, at a minimum, to be considered a viable research alternative by his company:

- **Is it doable?** Not just, Is it a problem? But, Is it something that we can do with our skills and resources within a reasonable time and be pretty sure of success?
- **Is it usable?** Could it be and is it likely to be used by the people you intend it for?
- **Is it important?** Will it be widely enough used and make enough difference to have an important impact?

Any strategy for setting research priorities that answers those three questions

— Is it doable? Will it be used? Is it important? — will go a long way towards developing more effective research programs in forestry.

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INTERGRATING SOCIAL SCIENCES INTO FOREST RESOURCE RESEARCH

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Abstract. Forestry and the value of forest resources are inexplicitly intertwined with the human habitat and social value systems. Traditional forestry studies and forest management have generally concentrated on narrow academic disciplines related to the growing and harvesting of trees. This paper discusses the need to broaden the base of scientific disciplines for forestry to include social sciences. The possibility of integrated systems analysis studies for forestry is also examined.

INTRODUCTION

The need for integrating multiple resource inventories and inter-disciplinary research on forests is often stated. For example, the Forest and Rangeland Renewable Resources Planning Act of 1974 and the National Forest Management Act of 1976 explicitly direct the U.S. Forest Service to assess the total renewable resource situation on all forest lands. The National Environmental Policy Act of 1969 requires Federal agencies to "utilize a systematic interdisciplinary approach which will insure the integrated use of natural and social sciences ... ."

Current forest resource research often follows narrow disciplinary lines even though research objectives are guided by broader economic and social needs which are better assessed by methods of social sciences. This paper explores the possibilities of greater use of social sciences as an integral part of forest resource study and management.

THE IMPORTANCE OF SOCIAL SCIENCES

The purpose of science is to understand how things work. The purpose of social science is to help us understand how society works. Societies involve interactions (functions or behaviors) and patterns of relationships (structures) of two or more people. In the broadest sense a society can consist of as few as two people or as many as all on earth, i.e. the socalled modern global village. One person cannot be a society, thus individual thought or study is the purveyance of psychology or philosophy.

Social sciences are not usually viewed as a single field. Just as the natural sciences are divided into such areas (disciplines) as physics, chemistry, biology, geology, and astronomy—to mention only the main headings—so are the social sciences also divided. We typically think of six social sciences: sociology, economics, political sciences, history, anthropology, and human geography. In addition, psychology can also be considered as a social science when it deals with groups, even though much of its study is about the individual.

Economics deals with the production and distribution of marketable goods and services and the allocation of scarce resources. Economics is used extensively in forestry management in allocation of resources and investment decisions. Political science studies government and the effects of government institutions have on how things work. Sociology deals with all of the human interactions between individuals not dealt with by economics or political sciences. These three main social sciences are independent of time and place.

Anthropology studies particular societies and how the patterns of living differ and on differences in cultural values. History gives explicit times places, and persons or groups usually with emphasis on time sequences of events. Geography gives special attention to particular places and to relation of things to space. Psychology to the extent that it relates to understanding the relationships of individuals or societal groups can also be considered a social science. In addition, combination of discipline such as eco-

nomic history, political economy, social psychology, or economic geography are recognized as separate specialty study areas (Kuhn 1975).

The need for substantive interdisciplinary analysis is outlined by Sherif and Sherif (1969) who write:

> The core problem of interdisciplinary relationship for a particular science is to determine what findings and what concepts it has to borrow and in what matters it has to be in transaction with other disciplines in order to stand firmly on its own feet, with all of supporting evidence that it needs to insure the validity of its formulations. It will not gain this support by standing like an ostrich with its head ground into a fixed hole through the sheer force of orthodoxy. On the contrary, assessment of what it needs from others and with whom it needs to transact will provide ingredients for weaving its own fabric. It will provide a balanced view of its bearings relative to other disciplines.
>
> After all, no scientific discipline is an island unto itself. It cannot develop firmly in isolation from others. Deliberate assessment by one discipline of what is needed from other disciplines and who it needs to transact with will provide a center of gravity for its own development that is conducive to probing its problems to any degree of intensiveness desired. In fact, intensive study of its own domain will gain both in depth and scope. Insulated from related disciplines and lacking firm bearings relative to them, intensive study within a discipline sooner or later starts to produce floundering expeditions into territories already explored by other disciplines, with the resulting exhibitions of ignorance that have been displayed in the past by psychologists who sociologized their own brands of human motivation or perceptions. It should be clear that what is being advocated as the core problem of interdisciplinary relationship for a particular science is a cry from hastily conceived notions of interdisciplinary study as a flashy veneer of the finishing school which aimed to turn out young ladies who could converse gracefully on diverse topics in a drawing room.

It has been remarked that many of the difficulties that beset the world today can be explained by the fact that progress in the social sciences has lagged far behind that of the physical and biological sciences. Moreover, the continued explosion of knowledge in biotechnology, computers, material science, and other natural science and engineering areas will have profound implications for forest research and human society as a whole. For social and technological factors interact and technological development is often determined by social logic rather then technological logic (Bijker et al. 1987). On the one hand the potential for improved biological and physical production is great, while on the other lack of social and economic control threatens the the very environment of the planet. Problems of maintaining the environment, providing sustainable economic development, managing disruptive social change, and the equitable distribution of resources are of major concern in many parts of the world today. These concerns are expressed by Arno Rosemarin, editor of AMBIO[1]

> The term 'forest' stems from the Latin *foris* meaning outside or outdoors. That forests often determine the habitat limits to man's terrestrial existence is, unfortunately, not implied from the term. Today, our knowledge about forests and terrestrial ecology is being seriously challenged by observations of long-range transportation of pollutants, forest decline, climate change, global ozone shifts, etc. The message is also clear that the more "conventional" resource management-type problems relating to forest exploitation, silviculture and agroforestry, require complicated solutions which challenge even the best forest managers.

[1]From an editorial in AMBIO A Journal of the Human Environment, Vol XVI No. 2-3 published by the Royal Swedish Academy of Sciences.

FORESTRY AND SOCIAL SCIENCES

Forestry needs to broaden its base of scientific disciplines to maintain its position of leadership in forest resource policymaking (Clausen 1977). Some forest economists led by Professor William Duerr, have attempted to expand the use of social sciences in forestry. However, most research is limited to studies of economic efficiency and resource allocation. For example, see the index of social sciences in forestry which was compiled for many years under his influence and is now compiled at the University of Minnesota (Albrecht 1987). Traditionally American forestry has based its policymaking foundations on biological models from European traditions combined with eclectic models of timber harvest and utilization. As Peter Glück (1987), an Austrian forestry professor observes:

> The History of forest management is maybe characterized as a history of reckless handling of forests, resultant catastrophes, and response in the form of the establishment of doctrines for achieving harmony. These doctrines are 'timber primacy,' 'sustained yield,' 'long term,' and 'absolute standard'. Originally from Europe, they invaded North America and influenced forest activities all over the world. The four doctrines form the basic framework of academic forest science curricula and have legal status in many countries.

This has led to a concentration on timber management and harvesting as its major areas of concern (Clary 1986). A recent survey by Hendee and Roggenbuck (1984) indicated that of 542 wilderness related courses, only 13.2% were offered by Forestry Departments. Overall, 27.2% were in Resource Management Schools, 17.0% in Biology related schools, and 56% in Educations or Social Science Schools. Implications of involvement of non-resource schools in wilderness appreciation and is additional evidence that the wilderness concept is central to the larger conservation movement. For example, see, "Wilderness and the American Mind" by Nash (1982), a historian. If foresters are not involved in the further articulation and evolution of the wilderness concept, they may similarly be bystanders to the evolution of conservation thought.

The growing concern for conservation is further evidenced by the principles of the World Wilderness Congress which is dedicated to balanced stewardship of the world's natural resources as being essential to human survival and well-being; and conviction that commercial and industrial growth must go hand-in-hand with protection of a spectrum of wild and natural places. Those principles call for balance, dialogue, tolerance, respect, and conservation for survival and human development. They relate wilderness and resource management to conservation and human concerns. The 4th Wilderness Congress planned of Fort Collins Colorado in 1987 emphasizes strengthening world conservation efforts through science, management, and citizen's efforts. According to Hughes and Hendee (1984): "The 4th World Wilderness Congress will be an opportunity for foresters to demonstrate their interest and efforts in broad aspects of conservation and balanced stewardship of natural resources."

Social and cultural attitude toward forestry, forest policy and forest practices is particularly important in determining economically and environmentally successful programs. Current concerns about loss of tropical rainforest because of unsustainable and environmentally damaging economic development projects point out the need to consider cultural and socio-economic factors beyond strict financial investment analysis. Understanding the cultural, social, and economic structure of indigenous peoples is also important for forest management. As Clawson (1975) argues:

> It seems to me there is a great need for sociological research about cultural attitudes towards forest, forestry, forest practices. We need to know much more accurately than we now know, what public attitudes are, to what extent they vary among groups according to the usual socioeconomic factors of age, education,

> income, social class, and the like. More importantly, we need to know why expressed attitudes are held, and what are the trade-offs may between cultural attitude and economic gain. Perhaps the day will come when the forest sociology will be as well recognized as silviculture in the training and expertise of the forester. Until now, foresters and all the rest of us have relied too heavily upon 'common knowledge' and intuition; cultural attitudes toward forestry are as capable of enlightening research as are any biological aspect of the forest.

Most foresters prefer to deal with research in natural sciences. Here the tradition scientific method works well—problem definition, discovery, hypothesis testing, and hypothesis conformation/rejection. Most terms can easily be described in arithmetic terms, codified and modeled mathematically. Many traditional foresters would believe social sciences are un-natural sciences, at best irrelevant, at worst inimical to the study of classical forestry. Their feelings might best be summed up by W.H. Auden's little verse:

"Thou shalt not sit
with statisticians nor commit
a social science".

Management of the forest resource system requires a varying degree of knowledge from a number of different disciplines. Particularly when viewed from the prospective of international forestry, where cultural differences and political and economic expediency often dictate resource use decisions, which may not be environmental sound or desirable. Clawson (1975) has developed a conceptual framework for analysis for forest policy analysis. He categorizes five general criteria for forest resource evaluation. They are list as follows: (1) physical and biological feasibility and consequences, (2) economic efficiency, (3) economic welfare or equity, (4) social or cultural acceptability, (5) operational or administrative practicality.

It is possible to define the capacity of the biophysical environment of various area's of forestland with respect to their abilities to produce a bundle of attributes which we call resources. It is difficult enough to define the physical and biological variables and measure them, let alone define the consequences of various actions in terms of biological potential and resource output. The facts of nearly all situations may be subject to dispute—at least interpretation of the facts. As much agreement on the physical and biological facts must be obtained as possible and relevant resource use questions asked. The social scientist or policymaker must nearly always make a decision under uncertainty, and risk must be reduced wherever possible.

Economic efficiency with respect to the costs of forest investment benefits from the forest and return of investment capital are of major concern in forest management. Evaluation of non-market output from forest such as wildlife, genetic diversity, recreation, wilderness, watershed values, aesthetic value, and any other things not sold directly in markets is particularly difficult. Economic analysis can show the efficiency of producing one forest output as compared with other mixtures of outputs. Sophisticated methods of economic analysis of costs and benefits from natural resource development have been developed. They, however, are no better than the data upon which they are based. They also do not address questions of sustainable development and the impact of forestry programs on the distribution of benefits or the external environmental cost which may be incurred.

The economic benefits of forest management and forest research programs accrue to certain individuals or groups and the cost are borne by other individuals or groups. Seldom are those who benefit the same as those who pay. Often there are also some people who have additional costs or disadvantages when a particular project or management activity occurs. For example, a small landowner may be displaced when a large forest plantation project is developed. The benefits may go to the corporation or the development agency who sponsors the project. A new wilderness area or park may result in fewer

jobs for forest industry. The problem is one of economic equity and economic welfare.

Economic efficiency is not enough to develop acceptable forestry programs. Men do not seek to maximize their material well-being to the exclusion of all else. Everyone is a Economic Man up to a point, but as the economist Kenneth Boulding states – he certainly would not want his daughter to marry an Economic Man. Social, cultural, and political reality must accompany forest policy and its management programs. Burch (1977) outlined some of the factors that should be considered in studying the social aspects of forestry and formulation of national attitudes toward forest resources.

There must be the resources, the means, and the will to carry out a forest policy and its management program. This includes the technical competence, the competent administrative structure for decision-making and implementation, and finally, sufficient men, money, and machines to do the job. Political and social acceptance and economic compensation may be needed for a program that requires the cooperation of many small landowners or users.

Forest policy research needs an approach that integrates all of these criteria into a unified framework. What's more, there are additional areas such as community or societal stability and maintenance of the integrity of the environment which need to be considered. In the end of his comprehensive analysis of the social aspects of forest policy, Burch (1977) states that forest policy research could invoke all the disciplines from anthropology to zoology. Certainly, the social sciences should play a prominent role in the analysis of forestry research and decision-making.

SOCIAL SCIENCES AND SYSTEMS ANALYSIS

In the social sciences, the attempt to emulate the ideal pattern of the natural sciences has led to unnecessary frustration (Brodbeck 1968). By being able to point to the comparative vagueness of their concepts, to the comparative inexactness of their derivations, and to the comparative unreliability of their forecasts, many social scientists rationalized their procrastination and accepted a stagnation of progress in their field that is quite incompatible with the tremendous strides toward an enriched understanding of human interactions that have actually occurred, not only in economics and psychology but to an almost equal degree in sociology and political science (Helmer, 1966).

Social sciences differ somewhat from the classical scientific model of natural sciences in that terms, logical constructs and "laws" may vary through time or between cultures. The definition of concepts may also be different. The economist, Nicholas Georgescu-Roegen (1966), makes the distinction between Aritmomorphic Concepts which have a precise arithmetic meaning and Dialectic Concepts whose meaning is subjective to various contextual meanings. There is however one area of common ground which can be used to unify the difference between various disciplines of natural and social sciences, that is, the concept of systems analysis.

Systems Analysis concepts have been used in forestry since the 1960's when they first became in vogue. Generally, however, system analysis techniques have been applied to familiar problems such as modeling forestry activities like land management or timber harvesting. For example, see the symposium of systems research conducted by the Society of American Foresters (Meadows, et. al. 1975). The broader concept of system analysis techniques has been used by other social scientist and engineers. Orcutt et al. (1961) first tried a large scale multi-disciplinary study of the micro-analysis of socioeconomic systems. More recently, Krone (1980) details scientific knowledge as developed, structured, and applied to systems analysis and policy science. He diagrams the scientific method of systems analysis with an explanation of why systems analysis is art as well as science. The engineering oriented systems approach of heuristic problem-solving are

outlined by Wilson (1984). This approach provides a wide range of problem solving techniques and systems management concepts. Systems analysis provides a common ground in which foresters can relate concepts from many scientific disciplines in an integrated form for forestry research. As Gerald Weinburg (1975) writes:

> The general systems approach, then, can engender a parsimony of thought for the study of subjects. A similar economy is introduced in the study of situations, or special systems. In our experience, the general systems approach has provided a starting point for the study of a myriad of information systems, complex machines, social systems, individuals and working groups, and systems of education. Others have found the general systems approach useful in meteorology, political science, biology, sociology, psychiatry, ecology, engineering, and in fact just about any discipline you can name.

FOCUS ON INTEGRATED SYSTEM SCIENCE FOR FORESTRY

Forestry is the the study and practice of managing forest land and associated resources and the proper role for foresters is "The creation and use of all forestry values from scenery to wood" according to Duerr et al. (1979). Forestry is further viewed as a system: a set of interrelated persons, objects, ideas, and events and a "a set of interacting variables which is part of a larger system that is the sum of human experience". This can be viewed as a social-biological-engineering system. Management is the process of making effectuations to meet people's goals. All attributes of forest land which have value to human beings to meet a given need are termed forest resources. Resources change, by definition, as human needs and culture change. Thus, the very nature of forest resource definition includes value choices based upon social science concepts.

The approach suggested in this paper is to overcome the current overspecialization of academic forestry and the traditional biases of the forestry profession toward timber production, by transposing it to the realm of general systems analysis. Thomas Kuhn (1962) has studied the way in which new "paradigms" are created and old ones are destroyed; how paradigms are transmitted from one generation to the next; and how paradigms both help and hinder the progress of science. In particular, he distinguishes between "normal science"—working within the current paradigms—and "scientific revolutions"—in which the paradigm themselves come under assault. The ethnocentrism of belief systems mitigates against internal scientific revolutions within a discipline. Thus, "leading scientists" should be the least likely people to lead scientific revolutions. According to Weinburg (1975), Kuhn concurs in this conclusion as did Max Planck who wrote in his scientific autobiography:

> A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.

In forestry, the establishment of the paradigm of the next generation is now being influenced by current academic policy of the forestry and natural resource schools. The approach used by Duerr, Teeguarden, Christiansen and Guttenburg in their book on forest resource management is a step in the direction needed to develop the kind of generalist thinking for contemporary forestry and forest management. This book was the result of over 10 years effort and the combined work of 35 authors drawn from the forestry schools, agencies, and business concerns and from outside the forestry circle. Their contributions represented the array of specialized fields that constitute contemporary forest resource management. The authors state:

> A major a aim [of the book] is to focus the work upon integrated forestry: the creation and use of all the forest values from scenery to wood. We have tried to see forestry as a system of interacting variables and also a part of a larger social system

> that in the final analysis is the sum of human experience. Another aim is to view forestry, not as a set of rules, but as a set of resource alternatives. Still another is to demonstrate how modern quantitative methods of generating information can fortify judgement in choosing among resource alternatives.

This book is far from perfect. If nothing else the large number of contributors leads to a variety of writing styles and an uneven flow of the large number of subject areas presented. It also probably makes a large number of adherents to the traditional forestry paradigm uncomfortable. It is difficult to use as a handbook or textbook for traditional forest management concerns—cutting, planting, growing, and protecting trees. It could be said that the forestry profession cannot see the forest for the trees. It is the contention of this paper that the foresters should be the generalists, i.e., ones who know a variety of subjects and can integrate them into general laws of reasoning and systematic analysis.

Combined with the generalist approach to academic study and thinking should be the judicious use of quantitative techniques for management of information and decisionmaking about resource management and research. The development of sophisticated quantitative techniques combined with advances in computer technology provide a specialized means to generalize research and management methods. For example, Hafkamp (1986) has extended multiple layer programming methods to spatial, environmental and economic multiobjective decisionmaking. Kallio et al. (1986) presents a variety of articles revealing the state of the art of application of systems analysis techniques to problems of the forest sector. Care should be taken, however, not to excluded variables which are difficult to quantify or measure. Qualitative analysis can also be very useful for many problem and should not be rejected out of hand.

The advent of the computer and of prepared software packages has made modeling popular among scientists. However, the ease of use of many of these systems has allowed their indiscriminate use. The role and nature of modeling should be carefully examined. As Kac (1969) states about mathematical models:

> The main role of models is not so much to explain and to predict—though ultimately these are main functions of science—as to polarize thinking and to pose sharp questions. Above all, they are fun to invent and play with, and they have a peculiar life of their own. The 'survival of the fittest' applies to models even more than it does to living creatures. They should not, however, be allowed to multiply indiscriminately without real necessity or purpose.

This discussion can be applied to the construction of general system models (Weinberg 1975). There are essentially three sorts of activities related to models: (1) Improving the thought process—"to polarize thinking and to pose sharp questions", (2) studying special systems—real necessity or real purpose, and (3) creating new laws and refining old—"to invent and play with". The most important of these three is that of improving the thought process. The use of general systems principles provides an approach to learning which has certain categories of thought that because of their general nature supercede the particular vocabulary of a discipline, thus encouraging inductive reasoning and helping to remove disciplinary bias. Quantitative methods can also easily be incorporated into general systems analysis studies without regard for academic province.

CONCLUSION

It is the contention of this paper that forestry is now in the process of redefining its paradigm i.e., belief system, to contend with what Wickstrom (1987) calls the post-modern world. He quotes Professor Dusan Mlinsek, past president of the International Union of Forestry Research Organizations (IUFRO) as stressing that forestry has reached a turning which requires it to escape from perceptions of nature as a closed system

that can be atomized and move into the post-modern perception of nature as a dynamic and totally integrated synergistic system. Mlinsek stressed the need for a holistic approach in forestry but recognized that the social aspects of forestry need particular attention if a holistic approach is to be achieved. He also acknowledged that institutional inertia has to be overcome in the process.

A modest proposal is presented here for an integrated holistic approach to the study of forestry. This includes the integration of various disciplines of study including social sciences into comprehensive framework of study. General systems analysis principles could be used to guide thought and action. Quantitative techniques could easily undergird special systems study without any particular disciplinary bias. The basic groundwork for this approach has already been laid by Duerr et al. (1979), albeit imperfectly. All that the forestry profession needs to do is to shed the old skin of its dying paradigm, step forward to become the generalists of conservation thought and natural resource study, and once again assume the leadership of the conservation movement in the brave new post-modern world.

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OUTDOOR RECREATION RESEARCH AND PUBLIC POLICY: THE ROLE OF DISCIPLINARY BIASES

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Abstract. This discussion examines how disciplinary biases within outdoor recreation research can affect public policy. It suggests that important management tools adopted by outdoor recreation agencies reflect the perspectives of researchers with strong disciplinary roots in forestry. This perhaps has created a skewed perspective on what people are seeking from outdoor recreation, and on what it takes to produce a quality outdoor recreation experience.

INTRODUCTION

Living organisms are, in part, products of their environments. This is hardly a revelation to those of us who conduct outdoor recreation research. It seems to be a principle learned long ago. For some of us, the revelation emerged during elementary lectures in forest ecology. For others, it was news conveyed through our earliest readings in social psychology. For most of us, such an observation is, quite simply, old news. In our frantic pursuit to develop new knowledge about outdoor recreationists and their quest, we are quick to dismiss such a proclamation as familiar and unenlightening.

Yet, it also seems that the full implications of this old truism have not been appreciated as we go about the task of producing outdoor recreation research. We have not recognized that we as researchers—just like the living organisms we study—are products of our environments. The concepts we create, the inferences we draw, the perspectives we yield, and indeed the truths we hold are, in part, products of our own particular experiential history.

And while the individual experience each of us has amassed empowers us with enriched perspective for solving problems confronted by recreation managers, it also acts to restrict our vision. We can become bounded by the perspectives of those who taught us; we can become shackled by the tenets of our own particular disciplinary heritage. We tend to be socialized to reflect the prevailing paradigms of the greater research community—rather than trained to break away from them. We tend to fail to see an alternate point of view.

THE STRUCTURE OF RESEARCH

This discussion reflects upon some of our recent products of outdoor recreation research, and examines the role of disciplinary influences in affecting the character of those products. It suggests that disciplinary biases working within the research community are affecting how outdoor recreation management problems are being defined, how data are being interpreted, and even how scientific findings are being translated into management practice.

The fuel for such concern was precipitated, in part, by the review of outdoor recreation research presented by Knopf (1983). Through that review, a model of the forces impinging on the outdoor recreationist was constructed. In that model, the outdoor recreationist was posed as an actor subject to four discrete systems of influence—a social system, a personality system, a home and work system, and a cognitive system.

While the model seemed both theoretically sound and intuitively appealing, the question was raised whether the model accurately summarized categories of forces affecting the recreationist—or whether it did little more than reflect the organization of outdoor recreation research, which was broken into four lines of inquiry adhering to traditional disciplinary lines (Knopf 1983, p. 226). It

is possible, for example, that the concept of a social system emerged simply because there was a line of research being conducted by sociologists on the role of social forces in affecting outdoor recreation behavior. The concept of a personality system may have emerged simply because researchers in physical education and recreation health had concentrated on defining personality typologies of outdoor recreationists. The concept of a home and work system possibly emerged simply because researchers with a forestry perspective had developed a line of research on the motivating effects of conditions outside the recreation setting. And, the concept of a cognitive system may have emerged because yet another line of research was developed by geographers and cognitive psychologists who investigated the role of the mind in creating recreation experience. In Knopf's (1983) review, it seemed clear that different researchers with different disciplinary backgrounds were studying the same phenomenon, but were generating different messages about what forces are important in affecting outdoor recreation meaning.

The review left us with perplexing questions. Is our current understanding of outdoor recreationists and their preferences distorted by the discipline-bounded character of outdoor recreation research? Would our understanding of what is wanted by outdoor recreationists be different if research were to be organized to study meaning from a more holistic perspective? Four systems of influence were proposed, but are there missing systems? Are the boundaries between these four systems real? Or, do they do nothing more than reflect the broken character of recreation research—fractured by disciplinary lines across which there is little collaboration?

IMPLICATIONS FOR POLICY

We now wish to carry such questions raised by the Knopf (1983) review to yet another level of analysis. If our current understanding of outdoor recreationists and their preferences is subject to distortion, might not also the public policy that is based upon that understanding be subject to distortion? Public land managers and planners have frequently searched for clues from researchers on how to produce quality recreation experiences. Based upon revelations from the Knopf (1983) review, it seems likely that the information researchers are forwarding to policy-makers is contingent upon the discipline they happen to be operating within. That is, opinions on what is important in terms of producing a quality experience depends upon what scientist a policy-maker might ask.

Consider, for example, the problem of recreation land classification. The currently popular Recreation Opportunity Spectrum (ROS) land inventory system—now adopted by two principal land management agencies in the United States—clearly has emerged from the work of researchers strongly influenced by the forestry disciplinary perspective (Buist and Hoots 1982). The ROS system suggests that variation in response to outdoor recreation settings is strongly linked to the character of physical setting. In particular, it suggests that variation in response is strongly linked to variation in the physical setting of a recreation locale along a primitive–urban continuum. It presumes there are six fundamental classes of outdoor recreation locales, organized along a spectrum: primitive, semiprimitive nonmotorized, semiprimitive motorized, roaded natural, rural and urban. These six classes are seen as the fundamental organizational framework for the distinction of recreation resources, are seen as delivering distinguishable forms of human experience.

While a nationally adopted planning methodology such as ROS has served well in fueling integrated recreation land planning, the presumptions underlying any such methodology must be examined for possible disciplinary influences. What would happen if a nationally adopted planning methodology were to spring forth from a discipline other than forestry? Would the presumptions remain the same that the fundamental source of variation in recreation response is associated with variation along a primitive-urban continuum?

Would the fundamental classes of outdoor recreation opportunity be the same? The answer to such questions may well be no.

For example, consider the character of a planning methodology that might emerge from the discipline of sociology. Rather that focusing on a primitive–urban continuum, it would probably distinguish among resources that facilitate different forms of social transactions. In fact, Cheek and Burch (1976) have proposed an outdoor recreation resource classification based upon stratifying resources by the character of transactions they evoke. They call for the creation of seven classes of outdoor locales based upon transactional function. The functions are: integration, bonding, solidarity, exchange, fantasy, transition, and custodian (p. 155). In their system, the social transaction variable emerges as the primary determinant of variation in recreation response. Yet, such a variable does not appear even as a secondary concept in the ROS planning methodology.

As another example, consider a planning methodology emerging from the physical education and recreation health discipline. It would more likely incorporate a classification of outdoor recreation locales based upon styles of personality that are serviced. Important discriminating variables identified in the past include: Active–passive, participant–spectator, control–noncontrol, and association–disassociation (de Grazia 1962). From such a discipline, one might doubt if primitive–urban variation would ever emerge as a primary discriminating dimension.

So the question emerges then—what is the primary discriminating variable that distinguishes among outdoor locales delivering different forms of recreation response? Is it truly the primitive–urban variable, which is the keystone variable upon which our national outdoor recreation planning methodologies are built? Or, do the presumptions of these methodologies merely reflect the disposition of foresters, who as a group personally and professionally tend to dwell on the city–nature distinction? What truly is the primary variable that outdoor recreationists pay attention to?

The possible tainting effects of disciplinary bias can extend into virtually every area where managers turn to researchers for assistance. Consider, for example, the concept of defining what is meant by a quality recreation experience. Several questions emerge. What does one mean by recreation quality? What are the important determinations of quality? What does a resource manager operate upon to enhance it?

Those with a forestry perspective have tended to focus heavily on elements of the physical setting as primary determinants of how recreationists feel about the quality of their experience. Quality is defined in terms of appropriate collections of rocks, trees, footpaths, facilities, insects, campfires, number of people encountered and other features that define the physical array (e.g., Rossman and Ulehla 1977). However, those with backgrounds in sociology have tended to define outdoor recreation quality as the degree to which a setting promotes a sense that people in that setting share similar values (e.g., Lee 1977). Those with backgrounds in physical education and recreation health have tended to define quality in terms of the ability of a setting to facilitate the expression of the personality of a recreationist (e.g., Moss and Lamphear 1970). And, those with backgrounds in cognitive psychology might avoid any kind of emphasis on the external environment as a determinant of quality. Rather, they would define quality in terms of the symbolic or emotional constructs that are transpiring in the mind (Blomberg 1982).

So, what are the important determinants of a quality recreation experience? What does a resource manager operate upon to enhance it? The answers, again, seem to depend upon the researcher that is asked.

CONCLUSION

This discussion points to the possibility that the world of science carries the potential for misdirecting public policy,

or at least for causing recreation resource managers to focus on issues that are different from those that are of priority to the recreating public. It may be that policy-makers are preoccupied with managing the physical setting, when what people really care about is the character of the social experience. It may be that policy-makers are preoccupied with managing the external environment, when what people really care about are the symbolic representations of those environments in their head—which may have little to do with the external array. As long as questions policy-makers ask of researchers yield different answers depending upon whom is asked, it is difficult to know if policy-makers are focusing on the right issues.

What is needed is a scientific community that engages in debate and critical analysis, and a resource management community that insists upon it. At present, researchers have been slow to cross disciplinary lines; they rarely acknowledge the work conducted by researchers external to their own discipline. Instances of rich, intense debate among outdoor recreation researchers are rare. In fact, there are sanctions against it. There is fear that the management community might then judge researchers as fickle, indecisive and therefore unreliable.

Yet, debate is precisely what the management community needs. There needs to be a willingness and commitments of scientists and policy-makers alike to open management concepts and procedures to scrutiny. Few efforts would advance the field of outdoor recreation more than to subject such central concepts in "land classification" and "recreation quality" to the rigors of critical analysis. There needs to be a classification of the fundamental differences of opinion; there needs to be a process for generating alternatives to the present point of view. Until we engage in a process that encourages us to imagine the alternatives, we will always carry the risk of misconstruing the true requirements of the recreating public.

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INTERDISCIPLINARITY: BY CHANCE OR BY DESIGN

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Abstract. Interdisciplinarity may involve the active cooperation of scientists with different research interests and training, or the more passive transfer of ideas and concepts from one discipline to another. Transfer of ideas is usually an individual event, a chance event conceived in a well-prepared mind. A new idea is acquired fortuitously either while persuing relevant literature or while communicating with colleagues. Idea transfer precedes active cooperation. The idea is first conceived by an individual who may then seek either the advice or participation of others in reformulating the idea as a researchable problem. Innovative interdisciplinary research rarely arises by design. It must be initiated by an inspired individual.

EFFECTIVE USE OF THE DELPHI PROCESS

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Abstract. The Delphi process systematically combines expert knowledge and opinions to derive a group consensus. It involves a selected panel of experts that share ideas and opinions through a series of questionnaires with feedback provided between rounds that summarizes the panel responses. It is applicable to problems that cannot be solved by precise analytical techniques, but a decisionmaker can benefit from the collective opinions of experts; more individuals are needed than can effectively interact in a face-to-face exchange; time and cost prevent frequent group meetings; or disagreements may be severe and communication must be refereed to avoid domination by strong personalities or by majority opinion.

The Delphi process has been used in forestry in several ways including allocating a forest fire prevention budget among various media; defining road construction standards for high mountain areas that would be acceptable to a wide array of interest groups; forecasting events in natural resource management and wildland recreation; estimating the loss in forest production from acid rain; formulating and analyzing county forest land policy alternatives for funding, timber sale procedures, and land ownership and forecasting ecological and institutional change in the Lake States forest environment.

Makeup of a Delphi panel varies with the problem to be addressed but may include experts, stakeholders and facilitators. Problems may arise in identifying experts and getting their commitment to participate. Other problems with the Delphi are the long time periods often needed for the study, panel drop-out, editing panel responses and feedback by the study director, tendency to treat future events as independent of one another, the large amount of time needed by the director to conduct the study, and difficulty in determining reliability of results.

INTRODUCTION

The delphi process enables a group of individuals, usually experts, to communicate with one another while retaining anonymity. This group process is usually aimed at exploring complex problems and often involves generating alternatives, evaluating impacts or forecasting future events. This panel of experts usually interacts through a series of questionnaires. Feedback is given between rounds in the form of a summary of panel responses to the former questionnaire. This feedback enlarges each participant's view of the situation. Panel members usually have an opportunity to revote on items after seeing the group response.

The delphi concept originated in the early 1950's as a spinoff from defense research by the Rand Corporation (Dalkey and Helmer, 1963). It was introduced to a broader research community in 1964 with publication of Gordon and Helmer's Rand paper, "Report on a Long-Range Forecasting Study".

It has been used thousands of times for a wide variety of problems. Its most common use has been for technological forecasting. How does "forecasting" fit into the array of means for projecting the future? Bunge (1967, pp. 66)) defines five means of forecasting the future:

1. Expectation: an automatic attitude of anticipation found in all higher animals.
2. Guessing: a conscious but nonrational attempt to figure out what is, was, or will be the case without any ground whatever.
3. Prophecy: large scale guessing on the alleged ground of revelation or other esoteric source.

4.Prognosis: an informed guess or common-sense prediction with the help of more or less tacit empirical generalizations.
5.Scientific Prediction: forecast with the help of scientific (or technological) theories and data.

The delphi process is designed as a means of "scientific prediction". Panel members apply known theories and data to complex problems. It is applicable to problems which exhibit one or more of these properties: (Linstone and Turoff, 1975, p. 4).

A. A problem does not lend itself to precise analytical techniques, but a decision maker can benefit from subjective judgments on a collective basis.
B. More individuals are needed than can effectively interact in a face-to-face exchange.
C. Time and cost make frequent group meetings infeasible.
D. The efficiency of face-to-face meetings can be increased by a supplemental group communication process.
E. Disagreements among individuals are so severe or issues so politically sensitive that the communication process must be refereed and anonymity assured.
F. To assure validity of results there is a need to avoid domination by the majority or by strength of personality.

The delphi process has been used a number of times for forestry related problems, including the following:

A. How should a fire prevention budget be allocated among various media to have the greatest effect on reducing the number of forest fires in Alberta (Dunn, Harnden, Newton, 1974)?
B. What criteria should be used to design and construct roads in alpine areas of Germany to satisfy diverse interest groups (Gundermann, 1978)?
C. What policy alternatives and policy impacts should be considered by Minnesota county governments with respect to funding forest management, determining what land to retain in public ownership, and selling timber -- especially long term leases (Baughman, 1982)?
D. What major events are likely to occur in the next 25 years that will influence the natural environment in the U.S.? Events were grouped into 5 categories: natural resource management, wildland- recreation management, environmental pollution, population-workforce-leisure, and urban environments (Shafer, Moeller, Getty, 1974).
E. What is the most likely percentage change in forest productivity in Canada due to long range transport of air pollutants (Fraser, 1985)?
F. What will be the demand for recreation resources in the area around Bemidji, Minnesota, in twenty years (Beliveau, 1981)?
G. What combination of land management strategies in a particular U.S. Forest Service planning unit will produce the least conflict among special interest groups (Freeman, Tremaine, Madson, 1977)?
H. In the Great Lakes forest region, what key variables will affect ecological and social changes over the next 20 years and what policies could be used to guide future change in the region (Flader, Bonnickson, Jordahl, 1980)?
I. What combination of land uses are most appropriate for Minnesota's State Forest lands (Knopp and Caldbeck, 1985)?

DELPHI COMPARED TO POLL AND COMMITTEE

The delphi is a hybrid between a public opinion poll and a committee meeting. It is similar to a poll in that it involves having a group of individuals independently and confidentially respond to a questionnaire that seeks personal opinions about a problem area. It differs from a poll by the feedback of information provided to respondents and the opportunity for individuals to modify their judgments based on their reaction to the collective views of the group. The delphi often involves a smaller respondent group than a poll, but requires a longer time period to secure responses. Since delphi panels are usually carefully chosen and the

members interact through the medium of a questionnaire with feedback, the panel responses are not a random sample from a population, and many statistical tests that would be applied to data from a poll are not appropriate for a delphi.

A delphi is similar to a committee because it often involves a relatively small group of carefully selected people that exchange ideas in the process of dealing with a problem. It differs from a committee by not permitting face-to-face meetings of the participants. Delphi avoids some of the psychological characteristics of a committee process including:

-The domineering individual that takes over the committee process.
-The unwillingness of individuals to take a position before all the facts are known.
-The unwillingness to abandon a position announced publicly.
-The difficulty of publicly contradicting individuals in higher positions.
-The fear of bringing up an idea that might be considered idiotic by others.

DUTIES OF SPONSOR, MONITOR, PANEL

A delphi process often involves three sets of actors—a sponsor, monitor and panel. The sponsor may be an individual, an organization, or several organizations that require analysis of a complex problem. The sponsor should be involved in the study design to ensure that relevant problems will be analyzed. The monitor is responsible for coordination with a sponsor; choosing a panel; questionnaire design, analysis and feedback; budgeting and a final report. Ideally, the monitor should be a team of at least two individuals—one knowledgeable about the problem and one with study design and editorial talents (Turoff, 1970). Depending on the nature of the problem, the panel may consist of three types of participants—stakeholders, experts, and facilitators. Stakeholders are people directly affected by the problem and those who will use the study results. Experts have special knowledge or experience relevant to the problem. Facilitators have skills in clarifying, organizing, synthesizing and stimulating. They may be chosen to represent diverse views (Scheele, 1975).

Panel members are seldom selected at random, but rather are carefully screened and selected because of special knowledge or skills they can contribute to problem analysis. It may be appropriate to identify three or four subgroups in the panel and keep track of how different subgroups vote on specific items. If a logical group of panel members is not readily identifiable, the monitor should solicit nominations from the sponsor, from panel members selected early in the planning, and from key persons knowledgeable about the issue. Invitations to participants are often sent in letter form, but personal meetings or telephone calls will probably yield a higher rate of acceptance.

Panel size can vary from a few individuals to several hundred members. Small groups are suitable for deriving a consensus on a few, very specific, technical questions. For example, a delphi panel of three people estimated the number of elm trees that would be killed by Dutch elm disease in Minneapolis, Minnesota in the following year. Very large groups are appropriate when public involvement and education are prime motives, but deriving a consensus is not important. An example was the study aimed at determining the types and amounts of different land uses that should be permitted on Minnesota State Forest lands (Knopp and Caldbeck, 1985). The initial mailing list had 1,159 names, but the dropout rate was high. Depending on the types of questions asked in the delphi exercise, the second round can generate five to ten times as much information as the first round (Turoff, 1970, p. 92). This snowball effect can quickly overwhelm a monitor as well as the panel. The time required for a monitor to summarize responses and for a panel member to react to the feedback may place limits on panel size. However, in a delphi study with 118 topic statements, there was no change in variance in responses between the first and last pages of this long questionnaire. The author interpreted this to mean there was no panel fatigue (Huckfeldt and Judd, 1974).

Motivation of panel members to continue through the delphi process requires careful attention (Scheele, 1975). Panelists—particularly those with technical backgrounds—must be convinced that judgments often have to be made about issues before all the facts of the problem have been researched and analyzed to the extent they would like. They must be persuaded that their subjective judgments may be a decision makers most valuable source of information. If panelists may be disinterested, find a worthy or prestigious sponsor or make participation of significant publicity value. Panelists must be convinced that time will not be wasted on obvious aspects, that subtleties in responses are understood and appreciated. Attractive and stimulating peers are probably the most powerful incentives for participation. Delphi is very demanding. Respondents should be recognized as consultants and properly compensated for their time with an honorarium, gift or "in-kind" reward if the delphi is not an integral part of their job function.

QUESTIONNAIRE DESIGN

Questions posed to delphi panelists may be presented in one of several formats. In most cases panelists work on written questionnaires sent through the mail. It is also possible to have respondents reply via computer terminals to speed up transmission of information and summarization of responses. Tape recorded responses or personal interviews may also be used.

Questionnaires should be carefully designed to achieve reliable and valid information. They should be pre-tested by persons knowledgeable about questionnaire design and by persons similar to the panel members in background. A month or more may be needed to develop the first-round questionnaire (Turoff, 1970, p. 43). Questionnaires for succeeding rounds should be designed before the first questionnaire is sent to the panel in order to be assured that the information received in one round can be adequately summarized and integrated into succeeding rounds.

How many rounds of responses are needed? Brockhoff (1975) found that on almanac and short range forecasting questions, where panel members were faced with the same set of decisions in each round, there was variance reduction in every round up to the fifth round, but the best results were already known in the third round. There may be a need for more than three rounds if the panel deals with a different set of tasks in later rounds.

The quality of materials sent out reflects the significance of the inquiry (Scheele, 1975). Give materials style, color and quality printing. Use emotive language and vernacular expressions to engage panelists. It may be appropriate to provide respondents two copies of the questionnaire so they may retain one for later reference or to do rough work (Turoff, 1970).

In the first round a factual summary of background material may be provided to panelists so they all start with the same basic level of knowledge about key facts (Turoff, 1970, p. 93). In consideration of the time required to assimilate this information, the background material should not be overwhelming.

Delphi exercises often start with open-ended questions to capture creative ideas or insights. Panelists new to delphi may respond with compound and sometimes lengthy comments. It is a good idea to show them examples of the form that comments should take in terms of being short, specific and singular in nature (Turoff, 1970, p. 93). Statements describing future events should be twenty to twenty-five words in length to achieve the highest consensus in forecast dates. Lower and higher numbers of words yield low consensus (Selancik, Wenger, and Helfer, 1971). The monitor must not impose his views and preconceptions of a problem upon respondents by overspecifying the structure of the delphi and not allowing for contributions from other perspectives related to the problem.

Standardized scaled measures should be available to respondents so they can qualify their responses to specific questions. Rating scales, such as a Likert

scale, are often used to measure relative importance of issues, desirability and feasibility of alternatives, probability of occurrence of events, confidence of respondents and the respondent's selfmeasure of expertise. Such rating scales permit rating one item at a time. They are quick, easy to comprehend, and psychologically comforting. By contrast, the pair comparison method is time consuming, requiring forty-five judgments for a comparison of ten items. The ranking method is fairly easy for a small list but increasingly difficult for a longer list. Participants may get frustrated by not being able to give equal rank to some items.

When editing respondents comments for clarity, try to preserve the intent of the originator. When editing from round to round, avoid changing a statement so that it has one meaning in round one and another in round two (Goldstein, 1975). Personal comments and arguments by respondents should be part of the information feedback.

On some issues selected panel members may contribute more expertise than other panel members. It is fairly common to ask panel members to self-rate their expertise on each issue. It was verified in one study on almanac-type questions that self-rating is a meaningful basis for identification of expertise and that selection of expert subgroups improves accuracy to a greater degree than feedback or iteration (Dalkey, Brown and Cochran, 1970). In another study (Brockhoff, 1975) self-ratings of expertise improved accuracy in only two of four delphi panels dealing with almanac and short-term forecasting questions. Expertise on specific issues did correlate with the number of years spent in a profession and with experience in a particular field related to the issue. There was no adequate measure of expertise for very specific questions.

Statistical analysis of the responses provides useful insights. When rating scales are used, the median and interquartile range (IQR) are commonly fed back to participants. Panelists are often shown their vote from the previous round and are given a chance to revote. Panelists outside the IQR are often asked to justify their answers. The median is used rather than the mean to reduce influence by outliers.

We cannot forecast behavior of the whole by forecasting behavior of its parts. Interactions and secondary events must be considered. Cross-impact analysis is a potentially valuable method for revising estimated probabilities of future events in terms of estimated interactions among those events. A delphi panel can determine initial probabilities and estimate an interaction matrix (Dalkey, 1972, p. 327).

By the third round, frequency distributions of responses should point out which items exhibit a polarized distribution, a flat distribution, or which items respondents are unable to make judgments about. The monitor should solicit additional comments on the latter items. Since delphis strongly tend to induce convergence and agreement, the monitor should purposely introduce ambiguities, even disruptions to act as a catalyst to explore the limits of alternatives. Agreements about a recommendation should be explored to discover whether individuals agree for the same reasons or not. Likewise a monitor team which ignores disagreements may cause discouraged dissenters to drop out thus creating an artificial consensus (Turoff, 1970, p. 88).

Convergence of responses is more common than divergence over several rounds. There is also a strong tendency for increased opinion change by individuals far from the median. In one delphi the feedback was purposely altered to move the mean response. Panelists were influenced by the new mean and shifted toward it. This seems to indicate that most respondents are interested in the opinions of other members of the group and desirous of moving closer to a perceived consensus (Scheibe, Skutsch and Schofer, 1975). In this same vein a highly dogmatic group is less likely to change an answer to a question on which they consider themselves expert than one on which they consider themselves less expert. But, in the presence of some perceived authority, such as a group

median, high dogmatism groups exhibit more change than low dogmatism groups (Mulgrave and Ducanis, 1975).

It is common to have the panel predict future events in a given time frame and estimate the date of occurrence or a probability estimate of the date of occurrence of events. Previous research indicates that short-range forecasts tend to be optimistic, but long-range forecasts are frequently pessimistic (in the long term no solution is apparent) (Martino, 1970a). Panelist uncertainty increases as the median date of an event moves further into the future (Martino, 1970b). We also tend to discount or not relate well to events in the distant future or in distant places (Linstone, 1973). The delphi designer should try to develop scenarios within the planning horizon of the panel. Perhaps the monitor can find a similar event already occurring somewhere else which can be described to compress the time dimension. The monitor could try to bring respondents into the future by having them role play in the future (Scheele, 1975).

We have a natural tendency to seek predictions about which there is strong agreement. We have more confidence in predictions which panelists strongly agree have a 50-50 chance, than predictions with a 70-30 chance but a high degree of divergent opinion. The latter we often consider unreliable (Linstone, 1975, p. 578). A strength of delphi is its ability to expose uncertainty and divergent opinion.

RELIABILITY OF DELPHI

An appropriate question about the delphi process is whether or not two panels addressing the same problem will produce similar results. There is evidence to support the reliability of delphi. Dalkey took first round answers to almanac-type questions and drew different sized subgroups of the entire panel to determine if the group median answer differed among the subgroups. He concluded that with an expert panel no larger than fifteen, it is highly unlikely that another equally expert panel will produce a radically different mean (Martino, 1972, p. 49). Gundermann (1978), at the conclusion of a delphi study, randomly divided an expert panel into two subgroups and compared responses with two statistical tests. He concluded the tests showed that the delphi results were reliable. Other studies comparing the forecast dates for future events obtained from different delphi panels show similar and therefore reliable results (Ament, 1970; Bender, et al., 1969; Martino, 1972, p. 50). Reliability probably depends more on questionnaire design and wording as well as selection of comparable delphi panels for comparison than upon the nature of the delphi process itself.

VALIDITY OF DELPHI

Persons considering use of the delphi method are interested in its validity or the accuracy of its predictions, especially when compared to other methods of prediction. Controlled experiments have been done in which delphi panelists were asked to estimate "almanac-type" data, where the experimenter knew the correct answer. Panel members did not know the answer, but had enough background information that they could make an informed estimate. Furthermore, delphi panels were compared to face-to-face panels. More often than not, the group median was improved by the anonymous feedback in the delphi process, while it became less accurate by face-to-face interaction (Martino, 1972, p. 32). In a forecasting experiment, college students were asked to estimate the point spread for two upcoming football games. Participants had access to information on previous performance of the teams and their players. A delphi and a committee process were conducted simultaneously with different groups. Results of the delphi process were more accurate than the committee process in forecasting point spreads (Riggs, 1983). Several studies have concluded that the degree of uncertainty felt by the panel increases as the time length of the forecast increases. In other words, panel forecasts are more dispersed as the time length of the forecast increases (Martino, 1970b,1972, p. 44).

POTENTIAL PROBLEMS

There are potential problems in using the delphi process that users should be aware of. An individual asked to list his preferences on paper may give a significantly different response from that which he would give in a real-life/real-time setting. A reasonable sounding (surprise free) scenario is usually judged "more likely" to occur than an unfamiliar one even if there is no evidence to support such a differential evaluation (Linstone, 1975, p. 575). A panelist may be unable to communicate in a concise sentence or diagram. Panelists may reply hastily without adequate thought or without reference to available materials. A specialist may not be the best forecaster if he is accustomed to focusing on a narrow field or subsystem and not taking into account the larger system and its interactions. Repeated delphis on the same subject can use up experts, or respondents that are familiar with prior studies may regurgitate old ideas. A panel of experts may not exist in some areas. All respondents and designers have biases both conscious and unconscious.

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ORGANIZATION THEORY AND FOREST MANAGEMENT: PROSPECTS FOR COLLABORATION

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Abstract. Much of the world's forest management is carried on through organizations. To a large degree, the effectiveness of forestry activities depends upon both the arrangements of people and tasks in an organizational setting; and on relations that develop among organizations in pursuing their specific objectives. Understanding these social phenomena may enhance the potential for effective forest management in organizations and society alike. This paper considers the relevance of organization theory to forest management. Following a brief description of three organizational studies of a public forestry agency; different theoretical perspectives of organizations are outlined within a metatheoretical framework designed to differentiate scope of analysis and behavioral assumptions. The above studies are then considered within this classificatory scheme; and areas likely to be important in future research on forestry-related organizations are also suggested.

"Policy is enunciated in rhetoric; it is realized in action." So began Herbert Kaufman's 1960 work **The Forest Ranger,** a classic study of administrative behavior within the U.S. Forest Service. Prior to 1970 it was one of the few studies of its kind with respect to formal organizations in the public sector.

The Forest Service is but one of a number of different kinds of organizations concerned with forestry in the United States. This diversity in turn reflects an entire social landscape that is made up of organizations. The organization is a major vehicle for getting things done in society, whether it be labor or leisure. The best technology, the finest growing stock, the sharpest technical experts, even unlimited funding lie relatively dormant without an effective organization through which they may be activated.

One would expect that a public agency, an interest group, a forest products firm, a research unit, and so on would be structured differently; made up of members with different skills and interests; and would engage in forestry-related activities in different ways. Knowledge of these organizational forms and actions would likely aid managers and others in better understanding the complex social relationships in which they are involved.

This paper describes the developing field of **organization theory** as one tool for fostering such knowledge. The paper begins with short sketches of three organizational studies of the U.S. Forest Service, spanning a period of three decades of agency activity. Attention then turns to a thumbnail sketch of the structure of organization theory, utilizing a recent metatheoretical framework within which differing perspectvies may be classified. The final section of the paper considers the above forestry-related studies within this framework; and offers some thoughts concerning the relevance of organization theory to understanding the nature and management of forestry-related organzations of all types.

ORGANIZATIONAL STUDIES OF A PUBLIC FORESTRY AGENCY

Herbert Kaufman introduced his 1960 work by observing that it is often assumed that the goals established by the top eschelons of organizations will be translated more or less automatically into specific actions at the local level. He noted that within the U.S. Forest Service, the degree of compliance of field administrators with directives from above, while not complete, was so significant as to beg for an explanation. His study was thus to be concerned specifically with "the way the field men are

induced to carry out tangibly the terms of headquarters agreements" (p. ix)[1]

The author focused upon the pivotal role of the district Ranger as administrator of the agency's operational or field-level unit. He selected five districts in different parts of the country for intensive analysis. He visited each, gathering information through interviews with Rangers, examination of documents, and so on.

Kaufman observed that despite the efforts of the Forest Service to promote consistency in management on all districts; it is unavoidable that the substantive content of the agency program is shaped by the daily activities of the district Rangers. Carrying out their various functions necessarily involves interpreting the nature of their jobs; and as a result "Rangers in effect modify and even make policy — sometimes without knowing it" (p.65).

The author then identified a number of "challanges to unity" resulting from the significance of personal and local factors on district management. These were seen to act as 'centrifugal forces' tending to disrupt the intended coherence between field units and upper management levels. These *impulses toward fragmentation* included: a) Problems of internal communication, reflecting social and geographical distance of Rangers from superiors, as well as the former's need for reconciling general instructions with specific situations; b) The potential 'capture' of field officers by local populations, either through Rangers' identification with local interests or their reactions to local political pressures; c) A Ranger's personal predilections or predjudices not being in harmony with objectives of organizational leaders; and d) Influence of the Forest Service's ideology of decentralization, which places high value on assertion of independence, autonomous decision-making, and defense of local and personal points of view. Unchecked, these influences would seem to have the potential to result in such variation in the substance of local programs as to "destroy (the Forest Service) as an integrated, functioning organization" (p.87).

Kaufman then examined why this did not happen. He identified a number of *techniques of integration* established by central Forest Service officials to preserve and enhance organizational unity. In brief, the agency attempted to: a) Institute a variety of procedural devices for preforming decisions, including authorizations, directives, prohibitions, clearances, etc.; as well as financial and workload planning procedures; b) Discourage deviations from these decision procedures via direct control mechanisms such as field reports, daily diaries, and inspections; hearings for public appeals; movement of personnel (to reduce potential for capture by local interests); and formal sanctions; and c) Internalize organizational goals and values by selecting, training and socializing personnel in ways conducive to maximizing organizational identification. Such an internalization of values may then in effect render direct controls unnecessary by fostering voluntary compliance of district Rangers with agency objectives.

Kaufman concluded from his study that for the Forest Service of 1960 the techniques of integration had been effective (p.206); and that the impulses toward fragmentation had for the most part been overcome. Overall performance did not differ significantly from goals established by agency leadership; and other symptoms of distress in relations between Rangers and higher management levels were for the most part absent. The author found the most striking evidence of effective integration to be reflected in the similarity of the Rangers' perspectives on a wide array of aspects that were part of their professional, and to a lesser degree, personal lives.

At the same time, Kaufman warned that excessive 'homogenization' of Rangers could inhibit creative input from the field level. In addition, he cited numerous forces for change which, from the agency's perspective, suggested that integrative techniques cannot remain static.

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[1] Page references to Kaufman's work refer to the 1965 (paperback) edition of the **The Forest Ranger**.

In short, "these ever-changing conditions mean the methods of one decade may not be sufficient in another. The challanges to unity never disappear; they can only be held in abeyance" (p.209).

The Forest Ranger Revisited. Twenty years later Christopher Leman returned to the five districts Kaufman had studied and interviewed the Rangers then in charge. He also interviewed 106 additional agency personnel at all levels of management; as well as the five original Rangers in Kaufman's study, all of whom had left the agency. Given the enormous increase in demands for forest outputs, far-reaching new laws, and technological advances in land management and information processing; it is not surprising that the author found a different world on the Forest Service district entering the 1980's.

In a 1981 paper entitled **The Forest Ranger Revisited**, Leman observes that the Ranger is now in effect the manager of a "miniature bureaucracy" — with many more subordinates, a large number of whom are specialists. The district must now supply a substantial amount of information as inputs to Forest-level planning activities at the National Forest Supervisor's Office; while having to complete ever more detailed operational and work plans for the district. Leman finds that there has been a shift in the focus of controls within the agency from a number of the direct techniques described by Kaufman to those exercised through these planning and budgeting processes. The agency also relies more on professional standards as controlling mechanisms for foresters and specialists alike.

The author finds that, on the whole, the Rangers believe they still maintain a fair degree of autonomy regarding intra-district allocation of goals assisgned to the district from above, and some input into the determination of the latter. In a sense the sheer complexity of their job would seem to make it even more difficult for excessive oversight from higher management levels. Leman concludes that the techniques for integration, though altered from past decades, remain operative; and that the Forest Service remains "one of the most successfully decentralized bureaucracies in the world". At the same time, he warns that the massive informational and analytical requirements of the planning process may reflect an attempt to accomplish the impossible; and that input from the field level remains critical to preventing these requirements from fragmenting the congruence of Rangers' attitudes with central agency policy that has characterized the agency for the last three decades.

Interest Group Influence. One implication of Kaufman's study was that the relatively high degree of compliance by district Rangers with central agency policy — as obtained via the formal and informal agency practices he described — would lead Rangers to *conform* to that policy even in the face of efforts by various interest groups (particularly at the local level) to convince the Ranger to act otherwise. In traditional theories of public administration, it is generally viewed that such conformist behavior is appropriate to lower-level agency managers. This still involves interacting with interest groups, but in a manner in which agency policy is preserved.

An alternative perspective, articulated by McConnell (1966), argues that interest groups are narrow and localized in their perspectives; and that in order to maintain support, government agencies institutionalize group access to land management decision processes. The ultimate effect of these relations is that the agency becomes *captured* by the clients it serves — the very result Kaufman describes the Forest Service as having successfully avoided.

Paul J. Culhane examined this "capture-conformity" debate in his 1981 work **Public Lands Politics**. He selected three geographical areas in the United States and interviewed Rangers on all 28 districts of the four National Forests located in those areas. (Officials of local field units of the Bureau of Land Management were also interviewed). He collected information on district resources and outputs; and personal characteristics of Rangers, including their memebership in other organizations. He

also identified about 40 different types of organizations involved in local federal lands politics in the three areas; and operationalized measures of group values, power, and access to land management decisions on Forest Service districts.

Culhane then identified the interest group contacts of Rangers, in effect defining an **organization-set** (Evan, 1966) for each administrator. He constructed a typology of sets based on the relative prevalence of four key types of groups — forest products firms; livestock operations; mining firms; and conservationists and recreationists. Culhane sought to test the "capture thesis" via the premise that if it were true then "the policies of Rangers ... with one type of organization-set should be significantly different from those of their colleagues with a different type" (p.195). He did not find this to be the case. In addition, most Rangers displayed sufficient variation in the composition of their sets "to maintain reasonably balanced constituencies" (p. 196). The author posited that this *variation within organization sets* was a more important factor in precluding capture of Rangers by interest groups than was their *conformity* to agency policy, the latter being that factor to which Kaufman had attributed causal priority (p.226).

OVERVIEW OF ORGANIZATION THEORY

Both Culhane and Kaufman examined the same organization, but each employed slightly different conceptual lenses in so doing[2] . Kaufman stated at the outset of his work that he did *not* intend to focus on substantive policy problems, techniques of organizational survival, or bureaucratic politics; and that, with respect to what he wished to examine "it makes no difference whether what is done by the Forest Service is ... or is not ... what it should be doing" (p.x). Culhane observed early on that "interest group theories of politics provide an ideal framework for examining federal public lands management" (p.22).

How does Kaufman's viewpoint differ from Culhane's; and, just as importantly, how are they related? How many ways are there of looking at an organization such as the U.S. Forest Service? The answer to the second question is deceptively simple: there are as many ways to look at the agency as there are people willing to take the time to do so. At the same time, there are certain ways which qualify for inclusion under the heading of 'theoretical perspectives'; and the 'field' within social science which deals with the nature and construction of theories of organizations is referred to, appropriately, as **organization theory**. This section briefly discusses a metatheoretic perspective within which the structure of this emerging field may be articulated. The following pages then consider the perspectives of the above authors with the aid of this conceptual framework.

The scheme to be described here is that developed by Van de Ven and Astley (1981) and Astley and Van de Ven (1983). It is presented in Figure 1. Two key factors provide the underlying rationale for the classification of theoretical perspectives: a) the level of organizational analysis; and b) the relative emphasis placed by the theories on deterministic versus voluntaristic assumptions about human nature.

The level of organizational analysis may be viewed in terms of what the authors call a *micro-macro* distinction between the organizational characteristics under study. This makes explicit the fact that any type of collective structure or behavior is characterized by relations between the parts and the whole. At the same time, the 'whole' at one level is a 'part' of the next higher level. Thus within organizational analysis there exists a series of part-whole relations spanning levels from individual, group, unit, division, organization, population, and so on.

In Figure 1 the authors identify the micro level as including individual organizations and the people and/or positions within them. The macro level focus is upon populations or networks of organizations. As indicated above, the selection of these two particular levels (e.g.,

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[2] Leman's theoretical perspective mirrors, understandably, that of Kaufman.

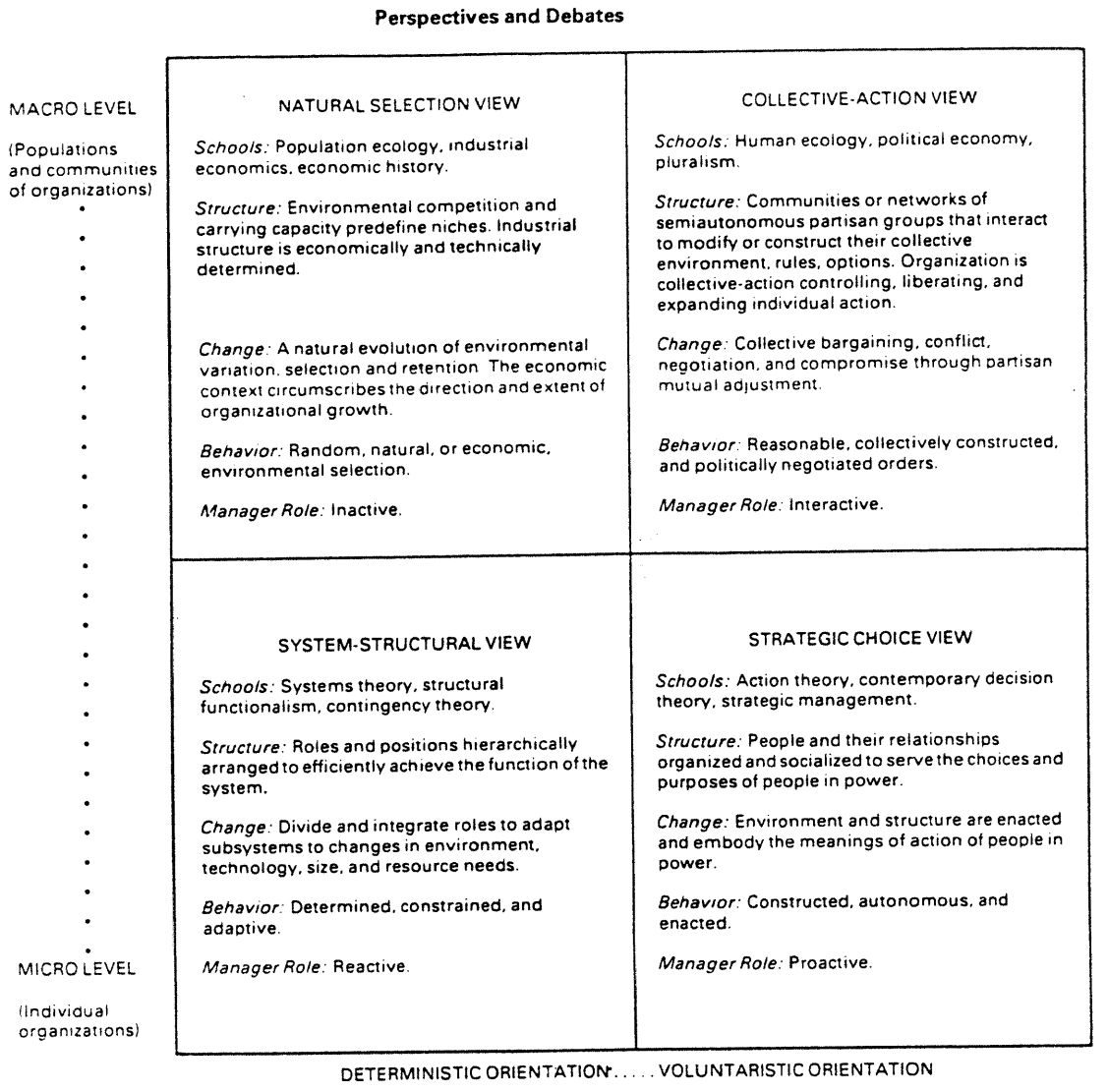

Figure 1. Four Views of Organization and Management. (Source: Astley and Van de Ven, 1983)

organization — population) is not the only one they could have made; they did so because for their purposes this distinction works best for the classification of theoretical perspectives on organizations.

The second analytical axis in Figure 1 derives from the age-old philisophical problem of free will, which is essentially concerned with the basic nature of human action. Do human beings and the institutions they create act autonomously, shaping their environment according to proactive choice; or are their actions determined exogenously by social forces too powerful for individuals or their institutions to influence or change?

A voluntaristic theoretical orientation, accepting the former view, depicts individuals as directing their own pursuits, and therefore as the basic source of change in organizations. In contrast, a deterministic perspetcive views the human individuals and institutions as embedded within a social structure which constrains autonomous action to a significant degree. While this does

contribute to stability in institutional and organizational arrangements; individuals and institutions find themselves having to react more to exogenous societal conditions than capable of autonomously creating their own social reality. The authors note that social determinism and individual freedon represent polar extremes for classifying theoretical approaches to organizations; and that most theories are more moderate in their positions than can be depicted in this scheme. Nonetheless, the distinction touches at the heart of nature of organizations — how human actions come to be arranged for the attainment of particular ends.

A brief overview of the four theoretical orientations depicted in Figure 1 is now in order. A **systems-structural** perspective is perhaps the most traditional of those represented therein. It reflects the synthesis of structural-functionalism (Parsons, 1975), the dominant paradigm in American sociology through the 1960's; and open systems theory (Katz and Kahn, 1978). Both view organizations as systems in which certain critical activities or functions must be fulfilled to insure survival. In Katz and Kahn's framework, for example, key organizational functions include production; production-support (input acquisition and output disposal); maintenance (motivation of members); adaptation; institutional relations; and management of the system.

Functions, therefore, become goals of the system which are attained via the creation of specific structures. The structure of the system is manifest in the arrangement of its functional subsystems, of which the basic structural component is the *role*. A role defines a set of behavioral expectations and responsibilities for the individual who occupies it; and positions in the organization are comprised of a number of roles. The content of the role is derived from its location in the subsystem, which is articulated as an input-output transformation process. Thus, for example, a forestry technician would have a number of roles within the agency's production subsystem.

The basic managerial tasks in this perspective involve *differentiating* the structure of the system to adapt to variations in elements of the environment on which the system depends for survival, i.e., is contingent (Lawrence and Lorsch, 1967); and *integrating* the more complex structure in a manner most conducive to the attainment of organizational goals. Organizational behavior is thus seen to be constrained and manipulated by top management, whose behavioral options are themselves largely dictated by changes in the organization's environment to which they must react appropriately.

A **strategic choice** perspective at the micro-level of organizational analysis is an outgrowth of social action theory (Dawe, 1978). This area of sociology has focused on the meanings which individuals assign to conditions around them and the interpretations derived therefrom as the driving forces of social action.

Within organization theory this approach has been developed from the initial works of Silverman (1970) and Child (1972). The latter has placed particular emphasis on the fact that the interpretations of reality noted above often have a distinctly political dimension. Reacting to the deterministic implications of systems theories, this perspective argues that decisions about organizational structure are not simply responses to contingencies imposed by the environment or by some set of internal system needs; but are "strategic events that include reference to the value positions of the actors involved and to the political processes in which they engage" (Van de Ven and Astley, 1981:436).

In this light roles do provide a framework for action; but within them individuals interact to construct their own definitions of situations; and act upon these 'social constructions of reality' (Berger and Luckman, 1966) to transform organizational structure for specific purposes. Managers are not reactive but *proactive*, both in designing organizational structure and in interacting with the environment. With respect to these external relations, resource dependency

theory (Pfeffer and Salancik, 1978; Pfeffer, 1981) posits that while environments do impose constraints, they can be manipulated through political negotiation to fit the objectives of those in power within the organization.

Shifting the level of analysis to that of populations of organziations, a **natural selection** orientation adopts this dominant paradigm in the biological sciences to the study of structural characteristics of populations of organizations — i.e., communities, industries and even society. Within a population ecology perspective (Hannan and Freeman, 1977; Aldrich, 1979) the basic evolutionary processes of variation, selection and retention are seen to operate on organizational forms. In effect, the forces of environmental competition and carrying capacity drive the evolutionary process that shapes the structure of the population. This viewpoint has a clear counterpart in theories of industrial economics, in which firms (or entire industries) that cannot compete successfully or gain access to scarce resources are 'selected out'; while those that fill the niches the environment provides will survive. Given such powerful forces at work, these theories posit a relatively inactive role for managers — who, while they may exert some influence through symbolic leadership, are essentially powerless to stem the tide of environmentally- driven change.

Within the natural selection perspective, particular organizations or 'elements' of the population are viewed as relatively similar and mutually vulnerable to environmental influences. A population is thus a simple aggregation of its individual members. In the **collective action** perspective, however, the identity of the population derives from its 'organization' as a network of semi-autonomous organizations who purposively join together as a social action system. Such a network manifests emergent properties not found in any of its members; and must therefore be examined at a macro or population level of analysis.

In this perspective, organizations are not seen to be pitted individually against a natural environment in fighting for survival; but as "emphasizing collective survival, which is achieved by collaboration between organizations through a construction of a regulated and controlled social environment that mediates the effects of the natural environment" (Astley and Van de Ven, 1983). Collective action occurs when organizations form networks capable of acting as a unit to pursue collective goals conducive to the interests of network members.

At the same time, the ambiguity arising from semi-autonomous organziations with frequently incongruent preference orderings; when combined with the relatively fluid participation of network members relative to a variety of issues; suggests that power should be an important phenomenon within a collective action framework. Accordingly, attention focuses on bargaining, negotiation and compromise as tools of partisan mutual adjustment (Lindbloom, 1965) in what are frequently decentralized and incremental decision processes.

Another key focus which naturally arises within this perspective is the question of how order is ultimately preserved or regulated under such collective arrangements. What insures that bargaining and adjustment processes will actually be able to proceed? The theoretical focus here is upon the institutionalization of norms reflecting the shared values of network participants which develop through repeated interactions. As Van de Ven and Astley (1981:448) observe, "this process of norm formation is not usually a rational or random endeavor, but rather emerges as an evolving set of working rules based on reasonable solutions to everyday transactions among conflicting parties". Network organizations comply with such norms voluntarily in adopting a collective orientation to social action.

ORGANIZATION THEORY AND FOREST MANAGEMENT

It is worthwhile to consider the works of Kaufman, Leman and Culhane in light of

the preceding brief sketch of the major strands of organization theory. This in turn may suggest some potential avenues of research for social scientists with an interest in forestry-related organizations of all types.

Strategic Choice: Decisions and Power. Consider first Kaufman's Forest Ranger. The Ranger is the target of a number of administrative cues from higher level management — cues that reflect the goals of those in power. The purpose of the cues is to increase the Ranger's psychological congruence with these goals, a condition which should be reflected in *action* (recall Kaufman's opening line) contributing to goal attainment. Many of these cues have the potential for functioning as *structural* constraints on the Ranger's actions by defining regular patterns of activities and/or social relations to which the Ranger is expected to attend.

All of this occurs *within* the organization. Kaufman comments frequently on the fact that top management "manipulates" Rangers through the use of controls and incentives. In addition, although he is not concerned with the motives of higher level management, he observes that " Where the environment of the Forest Service works against unity, it is neutralized. Where the environment may be actively employed to promote unity, it is exploited. Where the environment helps accidently, it is enjoyed" (p.227). The *structure* of the organization he describes (see Figure 1) is quite consistent with that described within the strategic choice perspective. Kaufman offers ample evidence that Rangers are 'organized and socialized'; he leaves open to question whether such influences are exerted to 'serve the choices and purposes of people in power'. To examine this latter question, a strategic choice perspective would investigate the variety of motives and choice opportunities available to higher level management in 'organizing and socializing' district Rangers; and the ways in which the intra-organiztional environment is constructed to achieve this end. As Pfeffer (1981:160) observes, "there are clearly a range of structural responses available to cope with coordination and control issues faced by organizational managers". A similar approach would be taken regarding management's relations to the external environment. The focus here would be upon managers' perceptual processes of attention, including various biasing effects; and their choices in selecting those environmental elements with whom it would be beneficial for the agency to establish and foster social relations.

Given that the focus of this perspective is on choice (of those in power) , it is not surprising that decision making processes receive much attention within this theoretical orientation. Kaufman's work was strongly influenced by that of March and Simon (1958), who emphasized the significance of organizations' establishing performance programs — i.e., extended decision making procedures — as a means of economizing on information. Van de Ven and Poole (1988) observe that, while assumptions of rationality underlie most strategic choice models, current focus is shifting to more indeterminate processes and their influence on managerial choice. This trend recognizes that decision situations are frequently ambiguous and preference orderings problematic. Indeed, one well-known model pictures certain types of organizations as 'garbage cans' into which pour problems, solutions, participants and choice opportunities (Cohen et al. 1972). What emerges depends on what happens to combine. The focus on probabilistic processes, in contrast to the relative certainty which forms the backdrop of rational decision making perspectives, captures an important dimension of organizational life. Research along these lines will likely remain important, serving as a source of insight for theorists and practitioners alike.

The strategic choice perspective focuses on people in power within the organization — i.e., upper management. Those further down in the hierarchy are in effect considered as part of the internal organizational environment that is enacted and manipulated to fit the purposes of organization leaders. This, of course, would appear to take much of the variety out of life for the district Ranger. But

Kaufman also clearly demonstrated that, based on Rangers' testimony, they were actively choosing to accept most of these constraints and incentives. The Rangers "wish to do as a matter of personal preference the things that happen to be required" ... (thus) ... "they are not consciously 'conforming'; they are merely doing what is right" (Kaufman:198). Leman's warning bears repeating, however, that no matter how strong the Rangers' voluntary committment; it may well begin to break down under the strain of the massive amounts of information and analysis required from the district as part of the current planning and budgeting processes.

If voluntary action of the district Ranger in complying with superiors' controls is matched by a capability of influencing the environment; then it is conceivable that a strategic choice perspective of the organization may be appropriate *at the district level as well.* This is precisely what Culhane examined in his study by testing a viewpoint that was not consistent with strategic choice — i.e., "the core principle of open systems theory .. that an organization's environment determines its behavior" (Culhane:195). As noted earlier, this latter perspective would be supported empirically if policies of Rangers with one type of organization-set were significantly different from those with another. This did not happen. Culhane found that the Ranger apparently *can* influence the environment by maintaining a balanced organization-set, in which those with competing interests 'keep an eye on one other'. The result is that the Ranger's autonomy is enhanced, or at the very least preserved. These results are consistent with resource dependence theory — a strategic choice perspective with somewhat greater emphasis on external constraining forces than 'pure' versions of the theory. In this case, the controls and incentives from above are some of the constraints the Ranger must 'put up with' while still maintaining a fair degree of administrative autonomy. The Ranger also depends on the environment in the sense that constituency interaction and support is politically important to maintaining both bureaucratic power and resource management effectiveness.

Thus a strategic choice perspective at the district level would picture the Ranger as: a) proacting with the environment, despite the presence of certain external constraints; b) acting voluntarily in accepting the intraorganizational constraints and incentives from above (this is not actually a *strategic* choice, but the fact that it is voluntary implies that it does not interfere with the Ranger's exercising such choice in other areas); and c) 'organizing' and 'socializing' subordinates on the district to serve the choices of the person in power — i.e., the Ranger.

An apparent anomoly now arises. If strategic choice focuses on those in power, then it would seem that higher level management would exercise such choice. However, in light of the above, it would appear that strategic choice characterizes both top management and district levels; when such choices should emanate primarily from power centers (i.e., upper management).

The critical concept that may aid in clarifying this situation is that of **meaning.** Top management 'sends down' a communication; their interpretation of that message is that it is to serve as a constraint. The Ranger does not interpret it in this manner and responds willingly. Voluntary action cannot decrease the Ranger's overall capacity for exercising strategic choice. Thus it is through the creation of meaning, derived via the interpersonal meduim of language, that a communication of constraint is transformed into a message consistent with voluntarism. The transformation of the social relation is facilitated through the interpretive act of the district Ranger. This preserves the coherence of a strategic choice perspective across levels of management within the organization. In effect, both Ranger and those at higher managerial levels are subjects who describe, explain and attach meanings to communications and other social practices. The significance of how these descriptions and categorizations serve to 'encode' experience with meaning is receiving increasing emphasis within social science (Rainbow and Sullivan 1987). Though numerous conceptual problems remain, perspectives such as strategic choice which adopt this

approach to social action will undoubtedly remain an important part of organizational analysis.

Systems and Structures. Strategic choice thus emphasizes power — the ability to choose and have that choice make a difference — and decision making processes as critical foci for the study of organizations. Nonetheless, even with power and choice opportunities, little will be accomplished without some structure to the activities undertaken. The structuring of such activities within organizations has been the main theoretical emphasis of open systems theory. From the perspective of top management, the district may be envisioned as performing a number of functions within the organization (indeed this is frequently how programs and budgets are organized). District personnel occupy roles within functional subsystems, and both subsystems and roles therein need to be arranged efficiently if the district is to produce outputs consistent with managerial and/or organizational objectives. From this perspective the behavior of the Ranger is viewed as determined from above; with the Ranger reacting to whatever is sent down the managerial pipe. Top management may or may not be choosing strategically. Even if they were, such actions would be examined in terms of their contribution to integration of the system. Moreover, whether or not management's specific purposes are known, the interrelationships among activities being conducted on the district may be studied from the view of how they fit together in producing the kinds and levels of outputs that are produced. With respect to external organizational relations, both Rangers and upper management would be viewed as reacting to impulses from the environment; although the sources and intensities of such stimuli would undoubtedly vary.

One difficulty with this perspective in *social* science arises from the fact that open systems theory deals with the organization as a system of *roles*; when it is people, not roles, who interact. Roles are important in addressing the nature of and relationships among tasks that must be accomplished; but they are essentially abstracted from the actions performed by those who occupy them (Miller, 1978:20). As an element of social structure, a role is a kind of rule that is "recursively implicated" in action (Giddens, 1979:64); A comprehensive social systemic perspective must relate the interrelationships of roles to a more basic question: how do member actions combine to bring about recurrent patterns of social and technical relations (i.e., structure) that lead to the definition and attainment of organizational objectives; and how do these structures in turn influence subsequent actions? In essence this reflects a more general aspect of social reality — the dialectical relationship between action and structure; and understanding this fundamental aspect of human life is one of the major tasks of contemporary social theory (Giddens, 1979). The action-structure dialectic is in fact a key logical basis underlying the voluntarism–determinism axis within the metatheoretical framework for organization theory developed by Van de Ven and Astley (Figure 1). The different orientations therein capture parts of the picture — but the nature and limits of the theoretical linkages have only begun to emerge.

Natural Selection. The natural selection perspective would not, at first glance, seem to be a particularly rewarding one for the study of *public* forestry organizations such as the U.S. Forest Service. The problem here is that the forces for 'selecting out' organizations in the public sector are much weaker than those facing their counterparts in the private sector (Kaufman, 1976). Nonetheless, were the time frame expanded to the 'very long run', an evolutionary perspective of the variation, retention and survival of particular organizational forms in the public sector might be envisioned.

On the other hand, a natural selection orientation has obvious relevance to private sector organizations, particularly economic actors. Of particular interest here is the "market failures" framework (Williamson, 1975) as a theory of how the forms of modern corporate organizations and industries emerge and evolve. The theory postulates that when the transactions costs of

acquring inputs from markets become too great, the firm shifts to hierarchies within the organization to provide the inputs in question. The evolution of the multilevel firm is one result. Such a perspective may do much to enrich the understanding of industrial structure and performance within the forestry sector. At the same time, it is important to recognize that the key dynamic operating here — regardless of the specific theoretical focus within this perspective—is the *structural* dominance of an all-encompassing natural and social environment.

Collective Action: Strategic Interaction. As noted earlier, Kaufman's study was couched at the micro level of organizational analysis. Culhane looked at certain characteristics of selected populations (i.e., organization-sets) and their effects on a particular focal organization. In doing so, he developed a quantitative model of group influence that was essentially econometric in nature. It may be recalled that economic perspectives are premised on a basic assumption that only individuals can act purposively; and that a population is defined as the aggregate of individuals (or individual organizations) therein.

A collective action perspective rejects this assumption, holding that organizations purposely join to form networks that manifest emergent properties. The focus here centers upon the synthesis of voluntary, normative and political means for use in mobilizing people and resources in a purposive collective effort. Culhane asked Rangers to list all key contacts (i.e., group representatives) who were not a part of their agency; while interest group individuals were asked only to list contacts within the Forest Service. Were this framework extended to include interest group contacts with each other, the entire social network could be modeled for a variety of relations (e.g., exchange of resources, money, information, etc.). Attention could then be directed at how individual organizations, perhaps with opposing interests, come to arrive at social arrangements through which policy matters may be effectively addressed; and the norms through which such arrangements are maintained.

At the same time, the structural effects of a collective action network may be articulated as well. This involves examining the ways in which patterns of network resource and information flows exert a structural influence on the outcome of collective negotiations and bargaining. Moreover, from the perspective of an individual organization, an actor's network position (e.g., central or peripheral)—identified via procedures for analyzing its relations with all other organizations simultaneously (Burt, 1980) — could be viewed in terms of its structural impacts on the likelihood of policy outcomes conducive to furthering its interests. The study of the formation and maintenance of interorganizational networks (Lauman and Knoke, 1987) — within or spanning both public and private sectors — represents an area of research with great potential relevance to the management of forestry-related organizations. It is evident here, moreover, that the dialectical relationship of structure and action operates at the macro level as well. Studies of collective action will be an important tool in bringing this relationship more sharply into focus.

Organization theory can contribute much to an understanding of the social processes through which forest management is initiated and accomplished. This brief sketch, though hardly a thorough description of the field, has outlined some conceptual lenses through which forestry-related organizations may be envisioned. Theory not only serves to stimulate the generation of hypotheses; it also enables one to carry on a conversation. Those participating may not agree on the adequacy of particular frameworks, or even interpret them in the same way. Nonetheless, in the process ideas are fleshed out, terms are clarified, and knowledge given a chance to grow. If this paper has aided in any of the above, it will have served a useful purpose.

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USING HOMEOWNERS' TREES IN URBAN RESEARCH

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Abstract. Interest in studying the urban forest has increased over the past several decades. However, performing research studies in this environment presents some new problems. In forest research, a scientist can generally locate the necessary number of study trees within the ownership of a single individual or organization. This simplifies the process of soliciting permission to utilize the trees. It also reduces experimental design problems since all the trees are under similar managment. This is not the case with urban forest research. Wth the exception of city-owned trees, each potential study tree is owned by a separate individual, a homeowner. This greatly complicates locating trees with similar sites or past management histories. It also increases the difficulty of receiving permission to utilize the trees.

Selecting the study trees requires consideration of many factors; uniformity or age class, past management history and distance from other potential study trees are all possible elements. Receiving permission to use homeowners' trees demands a high degree of empathy. The researcher must understand what motivates people to volunteer their trees and the various tactics used to gain and maintain their cooperation.

Our urban studies of the bronze birch borer will be used to demonstrate methods of designing urban forest studies and soliciting homeowners' cooperation.

WHAT BIOTECHNOLOGISTS DO, HOW THEY DO IT, AND WHY

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Abstract. Biotechnology, in its simplest context, is the technological use of biological agents. However, when one asks the question as to what biotechnologists do, one sees how complex, diverse, and revolutionary this research area is. Just considering plant biotechnology we find studies dealing with cell, tissue, and organ culture; in vitro screening, somaclonal variation, secondary product production; protoplast isolation, culture, and fusion;genetic engineering; gene isolation, gene splicing, and gene engineering; and modification of microorganisms so that they serve valuable roles in agricultural production or waste treatment. With topic areas as broad and diverse as these, it is impossible to generalize as to how all biotechnologists conduct their research. However, since this newly developing field offers forestry researchers unparalleled opportunities for rapid advances, it is appropriate to examine how biotechnology experiments are generally developed and why they are so designed. This presentation will briefly describe the types of research conducted to date and then will speculate as to ways to most efficiently address remaining research needs in forest biotechnology.

SOME CHARACTERISTICS OF HIGH QUALITY SCIENTIFIC RESEARCH

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Abstract. High quality scientific research seems a common goal for scientists. But, based on recent analyses of National Science Foundation grant proposals there is little agreement on what constitutes quality in research . The following characteristics that may help to identify quality research are discussed briefly: scholarship, citation analysis, society needs, answers or questions, degree of quantification, creativity or originality, scientific method, productivity, closure, maturity of science, and paradigm. Characteristics are summarized according to the source of the criterion (internal to the scientific questions or external to the specific scientific questions) and the time perspective in which the research is viewed (short term , e.g., the study is an event; or long term, e.g., the study changes something, the paradigm or the maturity of the science). Each combination of the summaries has an arbiter of quality. Thus, there is no single criterion. However, scientists can not afford to ignore the paradigm.

INTRODUCTION

Quality in research is a common goal for most scientists. However, there seems little agreement on what research quality is. For example, Cole et al. (1981) found that the fate of particular National Science Foundation grant applications was partly determined by characteristics of the proposal and investigator, and partly by a random element they called "luck of the reviewer draw." One of the goals of this paper is to organize and discuss some of the different characteristics of research that may label it "high quality."

Is there a need to identify and discuss characteristics of research quality? I think so, because quality in scientific research should not be a secret formula that only a few persons are aware of, or that some scientific laboratories have while others do not. The characteristics of research that warrant it being called "high quality" should be public information.

DESIRABLE QUALITIES OF THE CHARACTERISTICS

Science is a social process. Scientists are human and need reward and gratification just as much as others do, perhaps even more so. In identifying characteristics, we must recognize that merely stating them will cause scientists to react. One reaction, the desired one, of scientists may be to genuinely change methods, approaches, and designs to make their research have the stated characteristics. On the other hand, and again simply stated, scientists may change their research somewhat to make it appear improved, but in fact make no fundamental changes.

Consider, for example, recent efforts to judge research productivity. The criteria have caused scientists to react in an undersirable way. As a scientist's research productivity was publicly suggested to be measured by number of publications in a fixed period of time, scientists responded with a large number of short manuscripts, so-called least publishable units, each just large enough to warrant separate publication (Broad 1981). Productivity appeared to increase, but actually did not. Similarly, as frequency of citation in the literature became a criterion, the frequency of multiple authorships and multiple self-citation became more common. A recent paper in Science had 17 coauthors. A subtle variation used by some scientists who review many manuscripts is to suggest to the journal editor, in anonymous review comments, that the author cite one or more of the reviewer's papers. Combined publication of small units and citation manipulation greatly influence the appearance of productivity, but make no fundamental changes in it.

Each of these characteristics was probably useful in judging productivity until scientists began to change their mode of operation to make themselves appear highly productive.

Any characteristics selected for identifying research quality should reflect the best possible understanding of what constitutes quality and be immune to the reactions of scientists wishing to make their work take on the unwarranted appearance of quality.

NARROWING THE PROBLEM

Scientific research is the process by which scientific knowledge grows, and is dynamic in its own right. To make my task here more tractable, I will treat only the kinds of scientific research done during what is commonly called "normal science," the paradigm phase (Kuhn 1970), or is less commonly known as "the building and internal criticism phase" (Radnitzky 1973). The characteristics of high quality research in the preparadigm and crisis stages (Kuhn), as well as the pioneering and senility phases (Radnitzky), may be quite different from what I will discuss. Also, I will discuss only research where one wishes to make a statement about reality, the world out there, in contrast to a statement about how to study the world (methodology).

With these limits let me begin by reviewing some characteristics that have been suggested as quality indicators.

CHARACTERISTICS

Quality and scholarship: Perry (1985) tested a hypothesis first suggested by Dillon (1981) that scholarship is reflected in the occurrence of colons in the title of a research paper. Scholarship is defined in terms of "publishability, productivity, complexity of thought, distinction of endeavor, and progress of the enterprise" (Perry 1984). The argument here is that colons are needed to adequately express the complexity of the more scholarly efforts, leading to the term "titular colonicity." Perry analyzed 21,000 papers in ecology from seven "classes" of journals, ranging from the Journal of Theoretical Biology (highest), to dissertations (lowest), and found a definite increase in the frequency of colons in titles from lowest to highest. Of course, the mere mention of the characteristic has made it impossible to test it on future research.

Quality and citation analysis: Citation analysis, here limited to frequency of citation, is an increasingly popular evaluation criterion. The logic is sound, at least on the surface. An often cited paper must be a paper of exceptional quality, the argument goes. My reaction is that an often cited paper may be of exceptional quality, but it need not be. To be cited frequently a scientist must be known, publish in widely read journals, be in a well-funded research area, and, ideally, be a member of the inner circle of the scientific community in question. The established scientist has an inherent advantage in any system based on citation analysis. This is expressed as the Mathew effect: "the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark" (Merton 1968). (See also Serratosa 1984.)

Quality and society: Science may be thought to have an inside and an outside. The outside of science is its place in relation to society as a whole, and the latter's needs and expectations. The inside of science is, for example, its method, subject matter, strategies. One might argue that the degree of coincidence between a scientist's research area and the current needs of society help to indicate quality. I will not. Research topics come into and go out of favor as societal needs and funding opportunities change. In my short career as a scientist, I have seen several large research programs come and go: the biome study groups of the International Biological Program, the so-called big bug research programs, to name two. I do not know that any great advances resulted from these programs. Today biotechnology and acid deposition are two highly favored research areas. But, their future seems no different from other short-term research programs

heavily funded from special legislative appropriations. It may be tempting to think that an acid deposition scientist is doing high quality research because the effort has the trappings of a high quality operation—thousands of dollars of research funding, post-docs in the lab, plenty of travel funds, invitations to speak and contribute chapters to books, and so on. Of course, such things do not ensure quality.

Quality and answers or **Quality and questions:** We naturally think of research as providing answers to questions. However, sometimes the primary output from research is a well stated question. An old saying is "well-stated is half solved." Mathematician David Hilbert (1862-1943) is famous among mathematicians for his work in several areas of advanced mathematics, but also for posing some 23 unsolved problems that have served as a yardstick of the progress of mathematics for the first part of the 20th century (Kramer 1970). We definitely should not discard well-statedness of questions as potential characteristic of research quality. This is especially true if the poorly funded lone investigator is to have a chance to be fairly judged. Deep thought, the source of incisive questions, may not be an expensive item to support. However, the development of answers in empirical science usually requires some contact with nature — which can be very costly.

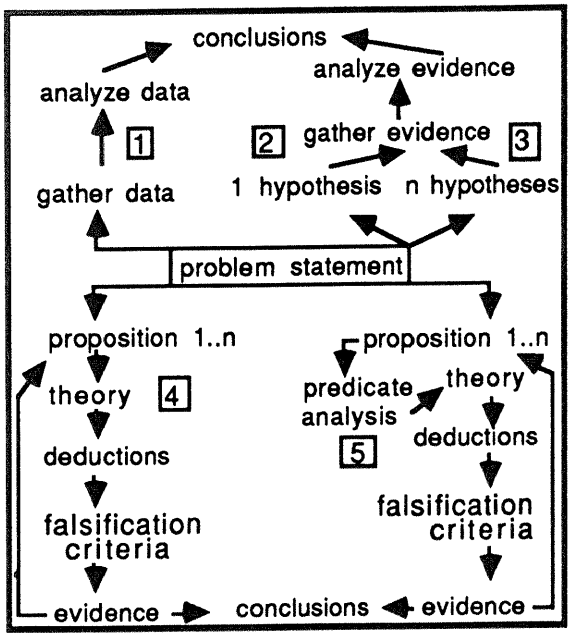
Quality and quantification: This is a tempting characteristic. The argument is that scientists in the more mature sciences, such as physics, do higher quality research than scientists in, say, the social sciences, because the concepts of physics are quantified, the propositions are nearly always expressed as mathematical equations, and the theories of physics are expressed as one or more mathematical equations. One time many years ago I tried to represent the degree of maturity in a special branch of ecology (population dynamics) by organizing the representing constructs according to the mathematical methods used in their expression:

algebraic equations
...
differential equations
(ordinary, partial)
...
integro-differential equations
...
integral equations

The trend here is toward an increased capacity of the mathematical construct to represent complexity in the process under study. Nonlinear ordinary differential equations have the potential to represent more complex system behavior than algebraic equations. Likewise, partial differential equations have the potential to represent system behavior in more than just the time dimension commonly used in ordinary differential equations. Integro-differential equations accommodate historical effects. Integral equations are used to express very general over-riding "principles" governing the behavior of systems (Volterra 1959). Degree of quantification is an indicator of quality. However, the more important indicator seems to be a progression, a change, down the scale. Clearly, some scientific disciplines do not have relationships expressed as differential equations. Thus, what signals quality is a change from relationships expressed as algebraic equations to ones expressed as differential equations, in this case of population dynamics.

Quality and creativity or originality: This is certainly an appealing characteristic. However, creativity and originality are such vague concepts in their own right that they should not be used in an attempt to clarify "quality," which is itself a vague concept. Another point about creativity is that it may be more important in the crisis period, or the pioneering phase of a scientific tradition, than in the normal science period.

Quality and scientific method: Given a question statement, one may attempt to determine the answer by following one of several sets of interrelated steps. I have suggested five different sets (loop numbers) as shown below.



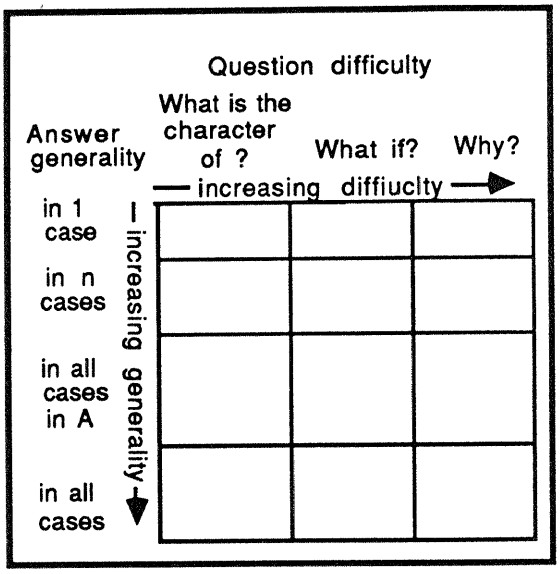
(Source: Leary 1981). Loop 1 is essentially the discovery phase of science. The "conclusion" for loop 1 is one or more hypotheses. In loops 2 and 3 the process begins with at least one hypothesis. Loops 4 and 5 include loops 2 and 3 but have added analyses of the concepts (predicates) involved in the hypotheses (propositions). The rationale for this suggestion is that a scientist who consistently follows loop 5 steps reveals a depth of understanding not evident from, say, one following loop 2 steps. Further, by expressing this understanding, the loop 5 scientist may well advance the science more than others. This characteristic is quite immune to abuse and, if followed, would move scientists in a desirable direction, I think.

Bunnett (1984) argues that quality in research shares some of the same characteristics as quality in manufacturing:

reliability — experience shows that the results can be repeated or confirmed,
design — an investigation is to be admired if it achieves a major objective by means of a few adroit experiments, or provides several kinds of important information simultaneously,
workmanship — care is taken to verify observations made.

Special to research, however, is what he calls "intellectual analysis of the work" — a high quality study evaluates critically the observations made, indicates their limitations and features deemed unchallengeable, and points out the significance of findings (Bunnett 1984).

Quality and productivity: Recently I proposed a framework for assessing a scientist's research productivity (Leary 1985). It consists of combinations of general classes of question difficulty and answer generality.



Answers to easy questions that apply to few objects are considered to constitute less productivity than answers to difficult questions that apply to all known or possible objects of a class. Productivity of a scientist cannot be judged entirely by the two coordinate axes because some sciences do not have universal explanations, or anything approaching them. There is more likely a "frontier" of knowledge for each problem area in each discipline that may have a lower-left to to upper-right slant. Productivity, then, must be judged by how close the scientist's work is to the frontier. Is the scientist pushing back the frontier of knowledge, or filling holes left by the pioneers? To me this framework helps to clarify the difference between quality and quantity of a scientist's research productivity. It also shows clearly that research

productivity is relative to the scientist and to the scientific discipline.

Quality and closure: In science, as opposed to mystery writing, we can never say for sure that "the case is closed." However, we would like to be able to say on occasion, "now we know this." Somehow, doing high quality research means settling the issue once and for all (as far as can be determined with present knowledge). The alternative is frequently reopened cases in light of "new" evidence, a form of running in place. The measure by which we "close" cases is the capacity we have to detect alternative hypotheses (if they were true) when conducting our experimental studies. If we know we have a very good probability of detecting the alternative and we fail to do so, this lends a great deal of confidence in the original hypothesis. This measure is the power of statistical tests. Power is more than reproducibility. In fact, the latter seems of marginal value in demarcating science from nonscience. How many reproductions are sufficient?

Quality and maturity of a science: Philosophers and scientists have assembled criteria that, they argue, indicate the maturity of a science. Roughly speaking, a science, or a special area within a science, is more mature if it has breadth (it covers the range of phenomena studied in the science), depth (it makes use of more than one level of organization in its constructs), and cogency (forcible, clear, or incisive presentation) (Bunge 1968). A discussion of characteristics of more mature sciences is relevant because it may be desirable for a scientist's career (ontogeny) to parallel (recapitulate) the generic change that has matured a discipline (phylogeny).

Bunge (1968) lists changes one should expect as a science matures:

empirical concepts should give way to transempirical concepts.
: Advanced sciences deal with concepts that refer to things and properties of things that are not observable.

information packages should give way to hypotheses.
: Curve fitting summaries of data are replaced by conjectured change equations that, when integrated, produce the observations.

black box representations give way to representations with a mechanism.
: In mature sciences mechanisms underlying patterns are known or strongly conjectured.

subsumptive explanation yields to interpretative explanations.
: Mature sciences not only have covering laws by which to explain events, but they have relations based on more than one level of organization (either the same level or both higher or lower).

unorganized theories are converted to axiomatized theories.
: Mature sciences are better organized than immature ones. Assumptions are well spelled out. Concepts and propositions are identified and expressed appropriately. Theorems and corollaries are deduced.

Practically speaking, the concepts in more mature sciences are:

relation and quantitative concepts instead of class concepts,

removed from Margenau's plane of perception,

of higher degree and order.

The propositions of more mature sciences:

are law-like relations,

are often expressed as implicit functions,

have few, if any, unknown numerical constants in their mathematical equations,

are expressed using higher level quantitative mathematical equations.

The theories of more mature sciences:

answer Why questions,

are better organized than those of less mature sciences,

may be axiomatized.

The research methods of more mature sciences involves:

forming at least one hypothesis and using alternatives to confirmation,

identifying the factual predicates in the propositions (hypotheses), and analyzing them.

Quality and the paradigm: The concept of a paradigm has played a large role in science since its use by Kuhn (1970) in describing scientific revolutions. Here, I consider a paradigm to be, roughly speaking, a set of shared beliefs about how some aspect of nature operates. During periods of what Kuhn calls "normal science," the paradigm functions somewhat as an arbiter of research quality. Studies that build, enhance, or strengthen the paradigm will naturally be judged higher in quality than those that do otherwise.

Paradigms are not static, but they seem to change more slowly than one might expect. To some degree paradigms may tend to be fashions, especially in areas of methodology research. For example, Hoch (1985) reviewed the primary tools used in the analysis and discourse sections of articles appearing in two agricultural economics journals in 1950, 1966, and 1983. The primary tools shifted radically from those based on English, descriptive statistics, economic geometry, and economic algebra and calculus, (used 28 times in 1950, 23 times in 1966, and 6 times in 1983) to those that use econometric models, programming and simulation models, other econometric models and mathematical economics (0,11,26 for the respective years). Thus, the analytical paradigm shifted considerably in the 33-year period. Clearly, a paper submitted to either journal in 1983 with descriptive statistics (farm budgets, distribution of variables, index numbers, etc. Hoch 1985) as the primary analytical tool would have been rated low in quality.

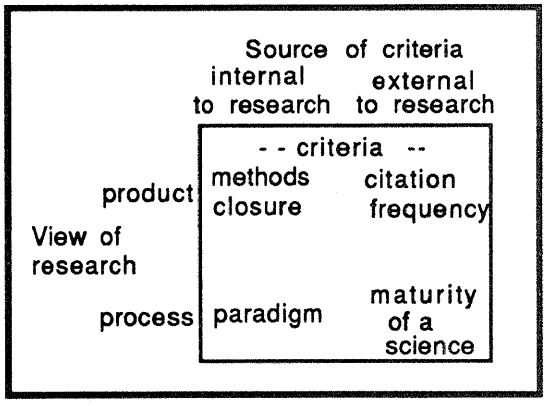
ORGANIZING THE CHARACTERISTICS

Listed below are the characteristic of research that seem to bear on quality.
scholarship
citation analysis
society
answers or questions
quantification
creativity or originality
scientific method
productivity
closure
maturity of science
paradigm

Most characteristics fall into two general categories. One category has to do with the temporal view behind the characteristic. Are we looking at research in the very short term, as an event or product such as a scientific manuscript, or in the much longer term, as a process, where something is being changed? For example, one tests different research methodologies on a different time scale than one accomplishes the maturing of a science.

The other category deals with the source of the criterion. There are two general categories of sources, one internal to the research area in question, the other external to the immediate research area.

The framework formed by these two organizing, supra-characteristics is as follows:



The framework suggests that in the short term methodology, closure, and citation frequency may be appropriate measures of research quality. In the longer term support for the paradigm and contribution to the maturing of the science seem important measures of research quality. Based on internal criteria, methodology, closure, and paradigm seem to fit together; for external criteria, citation frequency and maturation of the science seem to be similar.

If one cell in the grid is more crucial that others, it is most likely the internal — process cell where I placed paradigm. Recall, we are speaking of the period of normal science, where the paradigm is for the most part respected and revered. A piece of research that runs counter to

the paradigm may have difficulty making it into print. However, paradigms are the fashion part of science. I suspect that a study of the history of a bit of science would show that some paradigms actually set back the maturation of that bit of science. Yet, in the shorter term, these very ideas probably weeded out what was acceptable for publication.

The two off-diagonal cells reflect the fact that what is judged high quality research is just as much a social process as science itself. The two main diagonal elements reflect that one cannot judge what constitutes research quality without digging into the questions and methods themselves.

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RESEARCH REWARDS IN THE USDA FOREST SERVICE AND THEIR ROLE IN THE SCIENCE VS. TECHNOLOGY TRANSFER DILEMMA

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Abstract. Research rewards are discussed from a systems point of view. Three classes of rewards are described: internal, informal, and formal. Internal rewards originate from within the scientist. Attributes, such as creativity, lead to rewards such as a sense of fulfillment from developing new concepts. The scientific environment can either reinforce or weaken these rewards.

Informal rewards, such as research freedom, ego satisfaction, peer recognition, and user acceptance stem from the scientific environment. A mismatch between the goals of a research organization (publish) and an individual's reward value system (doesn't like to write) leads to difficulties (perish).

Formal (material) rewards are provided by the research organization. They come in three forms: research budgets, salary, and employment. The Forest Service formal reward system is oriented to two factors: rate of production (including administrative tasks) and the quality of science. The reward system is not well-suited to technology transfer; hence it receives less emphasis than other activities.

A scientist's time will be filled in proportion to his/her individual ranking of various rewards, constrained by organizational requirements. Thus, a reward system can enhance or inhibit the attainment of organizational goals. In the latter case, research managers may be unaware of the impact. Although evaluating and rewarding research is a difficult challenge, everyone benefits from developing better systems.

The goal of the research reward system in the USDA Forest Service is to reward and encourage scientific productivity. To quote Paul Waggoner (1983), in "evaluating basic and [applied] research, the director . . . . marvels at scientists who produce both, appreciates those who produce either, and worries about those who produce neither." Rewards offered by the Forest Service include those common to most institutions: promotions, certificates of merit, cash awards, and similar honoraria. The institutional reward system is part of a larger reward structure that motivates scientists to tackle difficult problems, generate new information, and publish their findings. This larger reward system involves, in addition to the rewards cited above, the characteristics of individual scientists and the environment in which the scientists work. The interplay of these components can have a pronounced effect, not only on scientific productivity, but also on our ability as a Government agency to respond to the needs of the people we serve.

The needs of various user groups have traditionally been addressed through the technology transfer efforts of scientists. Whether through "how to" pamphlets, workshops, or videotapes, scientists have tried to pass on knowledge generated from their research to land managers in a usable format. But the rewards for these activities are different from those for scientific productivity. Hence the dilemma: How does a scientist balance these two important activities — science and technology transfer?

To explore the role of research rewards in the science vs. technology transfer dilemma, we present our view of the research reward system in the Forest Service. We then discuss the dilemma posed by the system and describe its impact on our research and technology transfer activities. We conclude with suggestions that may enable the Forest Service to better fulfill its commitment to solving land management problems and effectively applying research results

while maintaining (if not improving) scientific productivity.

THE RESEARCH REWARD SYSTEM

To better understand the research reward system and how it functions, we identified 16 rewards and grouped them into three classes: internal, informal, and formal. We define internal rewards as self-generated psychological benefits that scientists derive from their work. These rewards are an outgrowth of a scientist's intrinsic nature and quest for knowledge as well as a reflection of his or her unique personality, values, perceptions, and skills. Informal rewards, on the other hand, are generally intangible and, in some cases, psychological benefits that scientists receive from the environment in which they work and from the response of others to their research. Finally, formal rewards, perhaps the most familiar, are tangible benefits that scientists receive from the research organization and from their clientele. Because the value of a reward is linked to an individual's perception, each scientist responds differently to each reward. Progressive research managers recognize this and tailor the reward system under their control to encourage each scientist. This requires an understanding of the types and sources of rewards as well as their impact on individual scientists.

Internal Rewards

Successful scientists possess characteristics that, when exercised through research, can yield internal rewards (Table 1). Perhaps foremost of these characteristics is curiosity; scientists often want to know why things happen or how they work. Their reward is intellectual satisfaction stemming from satisfying their curiosity and taking a step closer to the truth.

Scientists can also be creative. Not content to repeat or extend someone else's research, they develop new hypotheses and theories through which a discipline is advanced by a step or by a leap. Creative scientists derive fulfillment from developing new ideas, testing them, and demonstrating their efficacy.

Table 1. Qualities and skills of scientists and associated internal rewards.

Qualities and skills of scientists	Internal rewards
Curiosity	Intellectual satisfaction
Creativity	Fullfillment
Research skills	Pride in workmanship
Persistence	Sense of acheivement
Objectivity Skepticism	Confidence Self-assurance
Personal growth	
Long-term scientific contributions	

Another characteristic of many scientists is persistence. More often than not, a brilliant concept doesn't work the first time that it's tried. A scientist must have self-confidence to persevere despite initial failures. A reward for persistence is a sense of achievement that accompanies any effort in which obstacles have been overcome. There is a corollary to persistence—objectivity. It's a key ability to recognize that evidence is overwhelming a hypothesis and to change direction accordingly. This is what Huxley (1966) called, "The great tragedy of science . . . the slaying of a beautiful hypothesis by a series of ugly facts." Objectivity leads to confidence in the correctness of a solution and in an ability to do "good" science.

Most scientists are also skeptical, constantly asking, "Is it really so?" As James Horsfall (1983) said, "Scientists who accept things as they are rarely set up new hypotheses to test." Skepticism often drives scientists, rarely satisfied with an answer, to generate new questions and pursue research to answer

them. A reward for well-founded skepticism is self-assurance. It comes from producing significant research results and having the potential to advance a discipline, in defiance of conventional wisdom.

Finally, scientists are skilled at research activities such as planning studies, making observations, and analyzing data. They are concerned with details — knowing that one small mistake can ruin a study. Thus, many scientists take justifiable pride in the workmanship of a well-designed and well-executed study.

These examples illustrate some internal rewards stemming from a scientist's intrinsic characteristics. For brevity, we have linked only one reward to each characteristic. It should be clear, however, that each characteristic can lead to many different rewards, according to the value system of each scientist. When integrated, these rewards lead to personal growth stemming from the knowledge that one has made a substantial contribution to a discipline and established a foundation for future research.

Informal Rewards

Although we often think of the research organization as a source of formal rewards, it also provides informal rewards such as testimonial letters or public praise for a job well done. Informal rewards are generally intangible and sometimes psychological; an essential feature is their external source (Table 2).

Table 2. The relations between external sources, informal rewards, and internal rewards. Informal rewards can be transformed to internal rewards, providing additional incentives to the scientist.

External sources of rewards	Informal rewards	Internal rewards
Research organization	Praise	Pleasure
	Research freedom (task)	Sense of recognition
	Research freedom (time)	Sense of importance
Land managers	Application	Sense of accomplishment
Peers	Recognition and respect	Pride
Publication record	Publications	Self-esteem

Sources include the research organization or environment in which scientists work, their clientele (land managers) and peers, and, finally, their publication record (fig. 1).

One important informal organizational reward is research freedom. As Sterling Hendricks (1983) stated, "Freedom to inquire into the nature of things is a rewarding privilege granted to a few by a permissive society." In the Forest Service, for example, we have "pioneering projects" with fewer administrative constraints than other research projects. These are headed by

outstanding scientists, working on the cutting edge of science, who have proven their ability. Such scientists know that they are recognized for being capable of excellent work and judgment.

Freedom also comes in the form of time to do research. Enlightened research managers view their task as removing administrative obstacles to research. This allows scientists more time to conduct research, thereby enhancing scientific productivity. In such instances, scientists sense that their work is important to the organization or the discipline. If administrative rules hinder research, scientists sometimes respond by diverting energy to overcoming the rules, adopting an organization's standard of average performance, or, ultimately, leaving the organization for a less restrictive atmosphere. At the least, creativity cannot flourish in an atmosphere of frustration, and without creativity, there is little meaningful research.

Land managers are also part of the informal reward system. Scientists feel a sense of accomplishment when they see their ideas put into practice. Making things happen, changing the way things are done, and making tasks a little easier can be very satisfying.

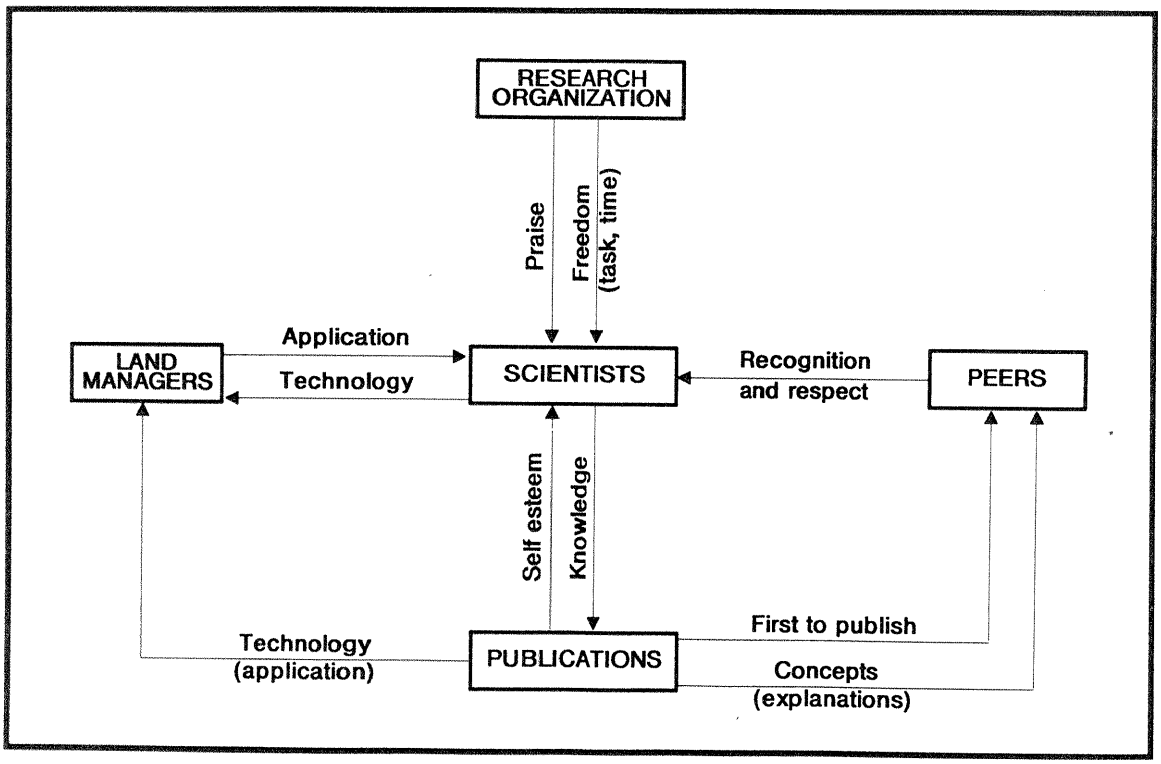

Figure 1. Interactions between scientists and external sources of informal rewards.

Because other scientists judge the value of research, peer recognition and respect are also important informal rewards. The primary route for peer recognition is through publications. The highest form of flattery to some scientists is to be told by their peers that their research was well done and that it produced concepts and explanations that advanced the state of the art. Further, science is competitive. Journals list the date on which a manuscript was received, so that if two scientists complete similar research at about the same time, publishing delays won't affect who gets credit for being first. As with any competitive endeavor, scientists can feel the "thrill of victory" associated with winning. Perhaps the ultimate peer recognition is a paradigm that bears your name in the literature.

Publications are the tangible output of science. They tell land managers how to apply research; they allow peers to evaluate research; and, of course, publications are what research managers count as a measure of productivity. But publications are also an informal reward in and of themselves. Seeing that definitive article in print with one's name on it can boost one's self-esteem. Scientists who value this reward will do well in most research organizations. Conversely, scientists for whom writing is difficult or who are little motivated by this reward, will have problems. Understanding the importance of internal and informal rewards is essential to managing a creative research organization. Many scientists are often driven by what might be called the "joy of research." Management neglect of internal and informal rewards runs the risk of evolving a thoroughly average organization.

Formal Rewards

Here we limit the discussion to the research organization most familiar to us. There are four formal rewards that the Forest Service provides to its scientists: salary, awards, a research budget, and employment (fig. 2). All but the highest salaries are determined by the Experiment Station. A scientist's salary is influenced by two factors — productivity and science. Annual performance appraisals are a key determinant of incremental (within-grade and merit) salary increases. These appraisals focus on the number and types of research tasks accomplished, including publications, as well as on timely and accurate completion of administrative reports and management functions. Although scientists are sometimes bothered by publication counting, the Forest Service merely reflects science as a whole. For example, Keith Thomson (1984) comments on the proliferation of trivial papers published in trivial journals, "But 'less is more' may be hard to attain. . . . Publish or perish is deeply embedded in the subculture of science (and God forbid that we should have to find some more valid criterion in order to judge promotions)." By default, administrators are doing, as best they can, what science has been unable to do for itself.

In the Forest Service, a scientist's grade is evaluated every 3 years by a Research Grade Evaluation and Advisory Panel. For a scientist motivated by material rewards, the panel system is very important. Panel members evaluate the scientist's research assignment, supervision received, research originality, and scientific contributions. Based on these four factors, they recommend promotion, retention in grade, or demotion. Although the first three factors are important, scientific contributions constitute 40% of the total score. In essence, a good publication record is generally sufficient for a good panel rating, whereas a weak publication record has to be strongly bolstered by other contributions to obtain a high rating. High ratings result in an essential recommendation for promotion and a higher salary.

Annual appraisals and panel evaluations focus on different attributes of the research process. Scientists must be attuned to the production/science dichotomy and balance their activities to perform well with respect to both systems. Station management must also balance the evaluation process, however, and recognize that production-oriented criteria are ill-suited to innovationand creativity (with their attendant risk of failure).

Although primarily determined by the Experiment Station, awards are granted at all organizational levels. Awards, whether medals, certificates, cash, grants, or some combination thereof, are presented to scientists who have produced research of outstanding quality and/or originality. Such awards publicly recognize scientists for their efforts and creativity as well as enhance career opportunities, and sometimes financial standing.

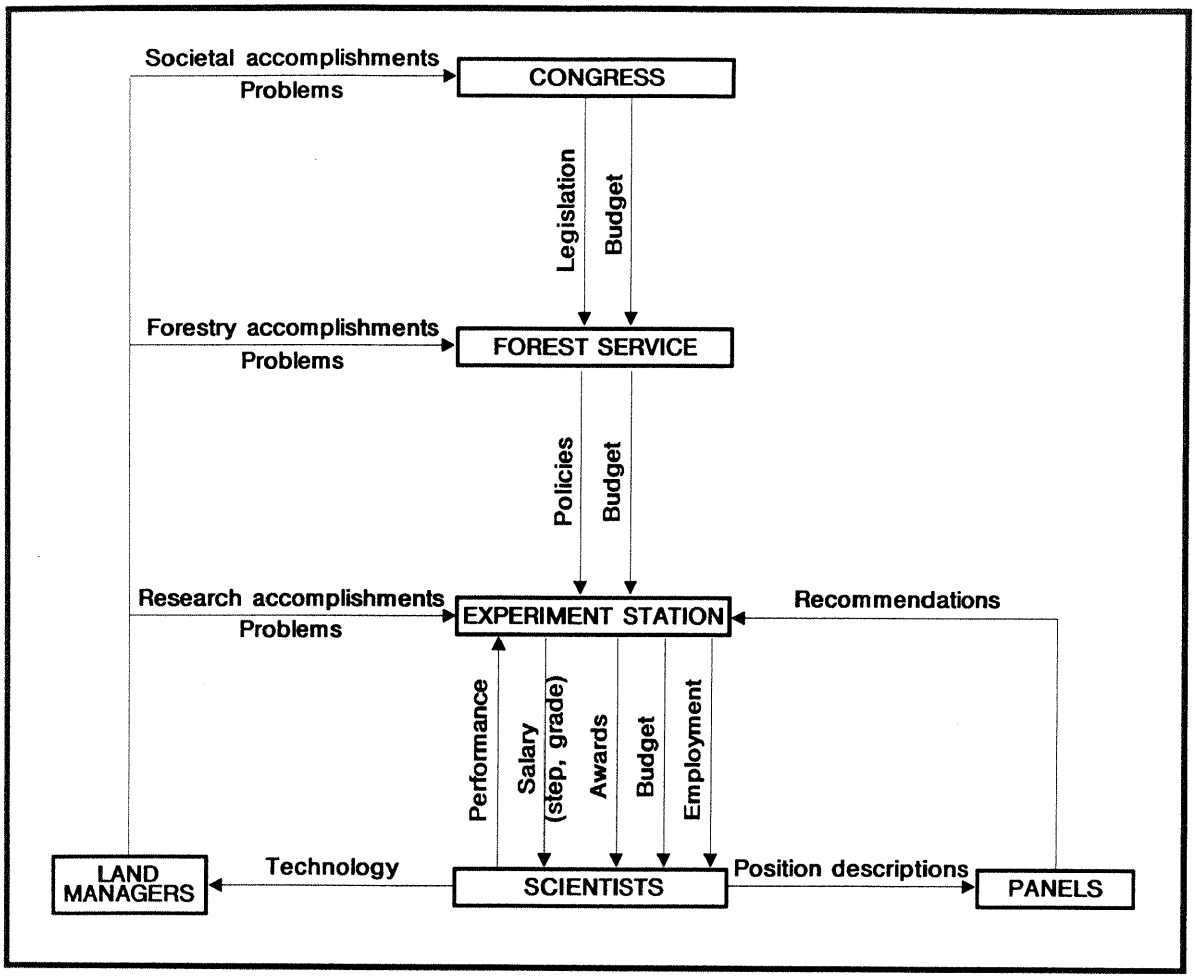

Figure 2. The formal reward system in the USDA Forest Service.

The research budget pays for facilities, equipment, and support personnel, without which scientists are severely handicapped. The research budget is also linked to freedom. There is little point in granting freedom to "seek the truth" but providing insufficient funds to do so. An innovative scientist, conducting world-class research, will not tolerate provincial-class funding for long. To many scientists, quality research facilities, adequate equipment, and sufficient support personnel are as important (sometimes more so) as salary increases.

The research budget is also intimately linked with the last formal reward — continued gainful employment. This is both the ultimate control for research management and the ultimate motivator for many scientists. As specialists, scientists may have limited employment opportunities outside of the research organization. Alternatively, having invested much of one's professional career in an organization makes separation difficult. Many scientists go to great lengths to avoid such a prospect. An important subset of the employment reward is the ability to choose one's location or remain in place after establishing roots.

Unlike most rewards, employment is all or nothing; it can not be applied in increments. Nor are its positive and negative impacts evenly balanced. Starting at neutral, lack of worry is necessary to the creative process. Conversely, varying degrees of concern ranging from the general malaise of "uncertain times," through apprehension caused by seeing associates uprooted, to the threat (real or perceived) of imminent personal

relocation or removal, result in negative impacts on scientific productivity, ranging from slow down to shut down. Management insensitivity on this issue can lead to much wider ranging, longer lasting, and greater negative impacts on research programs than would be expected, based on the magnitude of the original action.

Although the Station has some flexibility in allocating resources, research budgets and decisions to start, move, or terminate research generally originate at higher levels. Land managers play a key role in such decisions, although they exert their influence by circuitous and not always recognizable paths. We must, therefore, view the research environment in a broader context (fig. 2). The interaction of land managers at the Station level focuses on the technical aspects of regional research accomplishments and problems. Competition for resources is within research functions (e.g. fire). Scientists generally relate to this level best.

The next higher levels are concerned with the Forest Service as a whole. Research is evaluated not only in a technical sense, but also in terms of its priority relative to current Forest Service needs, its relevance to the Forest Service mission, and, ultimately, its responsiveness to congressional mandates. Individual projects are subsumed within functions. Competition for resources is first between research functions and then between research and forest management. National priorities may supersede regionally important needs. The interaction of land managers at this level is in terms of accomplishments and problems related to forest management and policy, with research viewed as a supporting function (fig. 2).

Support from land managers for research is especially important when budgets are cut significantly. Management support (or lack of it) for a particular research program can be a critical factor in deciding where cuts must be made. After decisions to cut research programs are announced, it is generally too late to reverse them. Thus, land managers must not only support research, but do so before decisions are made, and their support must be known to the decision makers. Therefore, continued program support requires that scientists 1) conduct relevant research, 2) communicate with land managers, and 3) insure that the existence of management support is recognized throughout the organization. Forest Service scientists not in tune with higher level concerns risk unpleasant surprises during their careers.

At the highest level, research becomes an instrument of public policy—a perspective far removed from that of the scientist. Forestry issues compete with national defense, trade deficits, and unemployment for public attention and, ultimately, legislative attention. Although most government scientists conscientiously serve the public good, there are many public goods, each of which is important to some constituency. Scientists who retreat into the security of their disciplines and make no effort to package their research results into something useful to their clientele, do so at some peril. In the words of Norman Borlaug (1983), "Our research must be good but it must be good for something."

THE SCIENCE VS. TECHNOLOGY TRANSFER DILEMA

The necessity of producing and communicating results that benefit our constituents, is a key dilemma facing Forest Service researchers. They are confronted with the conflicting demands of a research organization that, on one hand, requires and rewards scientific productivity while, on the other, advocates technology transfer for which it offers limited incentives or rewards. Likewise, scientists, who respond to rewards from land managers for technology transfer activities (e.g., application of research results) must somehow balance these rewards with their associated costs —lower scientific productivity, less time to conduct research, and, ultimately, forgone rewards for research productivity.

To describe this dilemma, we begin by defining technology transfer and to

expand the discussions of why and how Forest Service researchers do it. Technology transfer and application have been defined by Philpot (1985) as the implementation and use of research results and development efforts. Krugman and Creighton (1985) view it as the "adaptation of existing knowledge or technology to serve a new purpose, or its adoption and use by a new group of people. . .This, and related meanings, are the foundation for most of the Federal technology transfer effort. Technology transfer programs tend to be built around the idea of sharing the knowledge or resources for the benefit of the total population."

Along the same lines, Essoglau (1985) notes, "In the public sector . . . technology transfer occurs, at least in theory, when the 'public good' warrants it. . . . Public servants are expected to engage in activity promoting technology transfer because it is presumed to be in the public's interest. It is their legal duty to do so because their technology has been generated with public funds." This has not always been the case, at least in the Forest Service. According to Philpot (1985), "Forest Research historically considered itself a research organization with little or no responsibility for development and application. Considerable change has occurred in the past 20 years and a strong commitment to problem solving and user assistance has been established."

In response to this commitment, researchers often translate the scientific content, style, and format of journal publications into simplified descriptions of how to apply research results. Once these publications are disseminated, managers may be able to extract useful information from them. Scientists have also taken another route to technology transfer—presentations at meetings, conferences, and workshops geared specifically to managers. If a meeting has proceedings, so much the better, because the material is then available to more people than those attending the meeting. Finally, scientists participate in a wide range of technology transfer activities such as consulting and developing training materials and prescriptive guidelines.

Solving practical problems and assisting clientele would seem to be a top priority in most forestry research groups (Wagar 1987). But, in spite of the Forest Service's commitment to technology transfer and scientists' efforts to conduct it, Wagar (1987) concludes that "our reward systems, limited budgets, and perhaps longing for respectability all seem to shift our priorities and energies toward being scientific." And, he notes, "The reward systems of most forestry researchers push us primarily toward publishing, preferably in scientific journals, and away from the hands-on development, testing, and tinkering often needed to make results useful."

One reason for this is the Forest Service emphasis on publications. Historically, according to Philpot (1985), the primary product of research has been and still is "published, peer-reviewed information and knowledge. Publication, the end product of the scientific method for centuries, documents the research so that any competent scientist can repeat the work and duplicate the results. This documentation usually falls far short of providing sufficient information to transfer the knowledge to a practitioner for use in solving a problem." However, as we noted earlier, it can result in numerous internal, informal, and formal rewards for scientists. It also provides the Experiment Stations with tangible, measurable results of their efforts — hence, the heavy emphasis given to numbers and types of publications.

To some degree, this emphasis can hinder a scientist's performance (and consequent rewards) as well as interfere with the very technology transfer that the Station advocates. For example, responding to Station publication pressure, good scientists produce more publications. But, time spent writing superfluous manuscripts is time away from research. Many scientists publish predominantly in journals because such publications carry more prestige for panel evaluations. Unfortunately, land managers are unlikely to see the information, and if they do, it will be written

in the language of science, a language sometimes difficult to comprehend. Scientists may also prefer to attend meetings with proceedings rather than without, as the former include a "counter." Finally, creativity and inventiveness both involve a risk of failure—an unacceptable risk if each study must result in one or more publications. The end product of excess emphasis on numbers of publications may be mediocrity, lesser publication significance, fewer real contributions to knowledge, and, of course, fewer rewards for doing quality science.

Another reward system pressure towards publishing and away from technology transfer is the Forest Service panel system. As indicated earlier, scientists are evaluated by scientific peers using components of the scientific method, impact on future research, scientific and professional reputation, and publication quality and quantity (Philpot 1985). As of this writing, the panel process is evolving toward increased consideration of and credit for technology transfer activities. Many technology transfer subheadings have been added as well as references throughout the position description. The evaluation guide, however, remains science oriented. It is unclear how panels will respond to the new dilemma of evaluating revised position descriptions using unmodified guidelines. Although the evolutionary outcome is not yet forseeable, just starting such a process is a significant step in dealing with the science versus technology dilemma.

Given the historical precedent to publish, Station pressures, lack of incentives to do otherwise, and the panel system's bases for evaluation, it is not at all surprising that scientists opt to conduct research instead of technology transfer. Therein lies most of their rewards whether internal, informal, or formal.

RESOLVING THE DILEMA

Are there possible solutions to the science versus technology transfer dilemma? An obvious approach would be to change the reward system to encourage communication between scientists and their clients and, as Essoglou (1985) notes, to "reward problem solving directed to modifying and adapting technology . . . at least as much as that connected with creating or inventing a technological innovation." It is not only in the practitioner's interest to do so but in the scientist's as well for, without the needs and support of land managers, Forest Service research has no purpose. It is, therefore, in our best interest to decrease the gap between researchers and managers. According to Krugman and Creighton (1985), senior research managers must bridge the gaps between technology generators and users with incentives. Such incentives will not only encourage scientists to reach beyond traditional assignments, thereby increasing the public's return on their investments (Wagar 1987), but also to seek active participation by users in their research and development (Krugman and Creighton 1985).

In contrast, changing the reward system to encompass technology transfer assumes that this is a desirable activity for scientists. Another view, espoused by Philpot (1985), asserts that, rather than changing the current research reward system, the research organization itself should change. The problem, he contends, is that Forest Service Research "has not made the appropriate distinctions between research and development in its organizational structure, administrative procedures, accountability, funding, and user interfaces." Because the need for development (which he defines as the process of combining, modifying, and integrating existing and new knowledge from research, experience, engineering laws, and judgments into a product ready to implement) has not been identified within the Forest Service, no appropriate organization exists to accomplish it. Instead, researchers do it as part of their technology transfer, clients do it, or no one does it. Consequently, this has both diluted the research effort and inhibited effective application (Philpot 1985).

These and related problems, including the isolation of user assistance and extension functions primarily in State and Private Forestry, could be resolved

if, as Philpot (1985) suggests, the Forest Service established a Research and Development organization that included research, development, application, and extension responsibilities in one branch. Under such an organization, scientists in the Research Division would be responsible strictly for research, while the Development Division, relying on the expertise of staff in an in-house professional pool for assistance, would deal directly with land managers and be responsible for training users and for testing, evaluating, and applying its products. Scientists would no longer be confronted with the conflicting demands of a research organization that requires and rewards scientific productivity but not technology transfer, even though it advocates the latter and delegates the responsibility for it to the scientists.

Until these or other solutions become reality, however, the Forest Service research reward system will remain as it is—a complex interplay of rewards not only from within scientists, but also from external sources such as publications, peers, the research organization, and, of course, land managers. Realizing the dilemmas created by such a system, we, as scientists, face a challenge: to honor two masters—science and its ultimate beneficiary, the people whom we serve. Knowing the benefits of serving each, we must choose how best to serve both, fully recognizing the costs as well as the rewards of our decision.

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Risk Analysis Proposed for Evaluating Impacts of Scientific Research on Economic Decision Making

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Abstract. Risk analysis is proposed as a quantitative method for evaluating impacts of scientific research results on economic decision-making criteria. Scientific research is assumed to produce new scientific knowledge, including new data, theory, and discovery. Such outputs have two kinds of impacts that are assumed to be important in rational economic decision making: change in the possible modes of economic activity and greater certainty about economic outcomes of selected activities or events. These two impacts are described quantitatively as changes in probability density functions, as shifts in the range and likelihood for possible outcomes of events and related economic activities. Risk analysis based on Monte Carlo simulation provides a quantitative method for estimating how decision-making criteria are affected by such changes in the probability distribution of possible outcomes. Conceptual examples are drawn from the area of forestry research and forestry decision making.

INTRODUCTION

Economic analysis usually begins with an assumption that individuals make economic decisions based on rational criteria such as maximization of consumer utility or producer profits. As a whole, economic behavior may not be entirely rational, depending on whether one adopts a belief that decisions result in a rational allocation of resources or a belief that such decisions simply result in an irrational arrangement among individuals with conflicting goals. Nevertheless, when economic decision making is supported by rational evaluation of possible outcomes, quantitative and rational criteria are often employed. Criteria such as the anticipated levels of profits, costs, product performance, or returns on investment are commonly used to support economic decision making by both producers and consumers.

The general area of evaluating scientific research has been a problem for economists. It is impossible for anyone to evaluate specifically all possible economic impacts of research, simply because it is impossible to anticipate all future applications of scientific knowledge. As such, scientific research will intrinsically have future economic impacts that presently cannot be fully appreciated. Thus, any evaluation of scientific research is limited.

This paper is not concerned with how to evaluate scientific research in general but simply with how to evaluate current and foreseeable impacts of scientific research on economic decision-making criteria. According to conventional economic theory, economic decision making is the vehicle through which economic impacts of research are thought to be arranged.

SCIENTIFIC RESEARCH AND ECONOMIC DECISION MAKING

When economists have attempted to evaluate economic impacts of scientific research, they have typically evaluated impacts of research on production through technological advances that are traced as "spinoffs" to the scientific research (e.g., see Bengston, 1985). As such, economists have tended to highlight noteworthy scientific discoveries and technological innovations and the associated economic impact of modal change in economic behavior. The outputs of scientific discovery and technological innovation certainly allow for selection of new modes of activity in economic decision making. However, scientific research produces outputs other than discovery and innovation, including new data and more precise theory, much of which serves in economic decision making to reduce uncertainty about outcomes of economic activities or events.

As such, outputs of scientific research are applied in economic decision making not only toward selecting new modes of economic activity, but also toward reducing uncertainty about consequences of economic activities. This paper proposes a method for evaluating impacts of scientific research on economic decision-making criteria, considering together the research impacts of reduced uncertainty and availability of new modes of economic activity. The method is based on a simulation approach to economic analysis known as risk analysis.

Risk analysis is a technique used for simulating economic outcomes of events and activities in cases where there is some uncertainty about the possible outcomes (Hertz and Thomas, 1983). In such cases, the quantitative knowledge that does exist about possible outcomes is summarized in probability density functions. Monte Carlo simulation is then used to obtain a large random sample of possible outcomes from the known distribution of possible outcomes. The sample is used to simulate a probability distribution of possible economic consequences. Simulation is used to show the anticipated probability distribution for economic decision-making criteria, such as the probability of achieving certain returns on an investment or the range and likelihood for total costs of a project. Results can be used to show the likelihood of economic success or conversely the risk of economic failure associated with particular events or economic activities. By using risk analysis and focusing attention on how new scientific knowledge affects underlying probability distributions of events, scientific research output can be evalutated in terms of its simulated effects on economic decision-making criteria.

ILLUSTRATING OUTPUTS OF SCIENTIFIC RESEARCH

This paper assumes that economic impacts of scientific research derive from new knowledge about possible outcomes of economic activities or events in nature. In general, such knowledge is probabilistic, having some degree of associated randomness or uncertainty. New knowledge produced by scientific research, including new data, theory, and discovery, provides new information about the range and likelihood of possible outcomes. Scientific research can produce new knowledge that increases certainty about specific outcomes or that reveals information about outcomes that were never previously anticipated. Knowledge about outcomes of economic activities or events in nature may be illustrated generally as probability density functions. Evensen (1977) and Kislev (1977) used probability distributions of possible outcomes to describe how agricultural research contributed new knowledge, using the example of research on sugarcane varieties and how it shifted the distribution of possible economic values that could be obtained from available sugarcane varieties.

Figure 1 illustrates two general and distinctly different impacts of new knowledge on anticipated outcomes of events and economic activities. First, new knowledge has an impact on perceived randomness or uncertainty about outcomes. For example, by increasing certainty, new knowledge may narrow the estimated probability density functions for outcomes of economic activities or events (Figure 1a). In this case, new knowledge has not changed our understanding of the most likely outcome but has greatly increased the precision with which we can predict and select that outcome. Second, scientific research may lead to discovery of alternative outcomes that were not previously anticipated. This is illustrated in Figure 1b by a shifting of the probability density function mode from A to B. Both impacts of scientific research may occur together and both are valuable in economic decision making. It is valuable to increase certainty about outcomes of economic activities or events, and it is valuable to discover or develop alternative outcomes for economic activity. In summary, the output of scientific research is new knowledge that provides greater certainty about specific outcomes or an understanding of unanticipated outcomes.

A conceptual example of these impacts is provided by forestry research and its

impacts on forestry decision making. Forestry research provides new knowledge that increases certainty about performance of existing silvicultural practices and promotes development of new techniques and silvicultural materials. Greater certainty in silviculture is provided by research in such areas as forest mensuration, growth modeling, forest survey, and research on forest ecology. The application of such research leads to greater certainty about outcomes of economic activities and events in forestry, as illustrated conceptually in Figure 1a. At the same time, research in forestry results in innovation and discovery of new techniques and materials for application in forestry, as with development of new hybrid tree species and discovery of biological mechanisms. As illustrated conceptually in Figure 1b, innovation and discovery have shifted available modes of economic activity in forestry. Both effects can have profound implications for economic decision making in forestry.

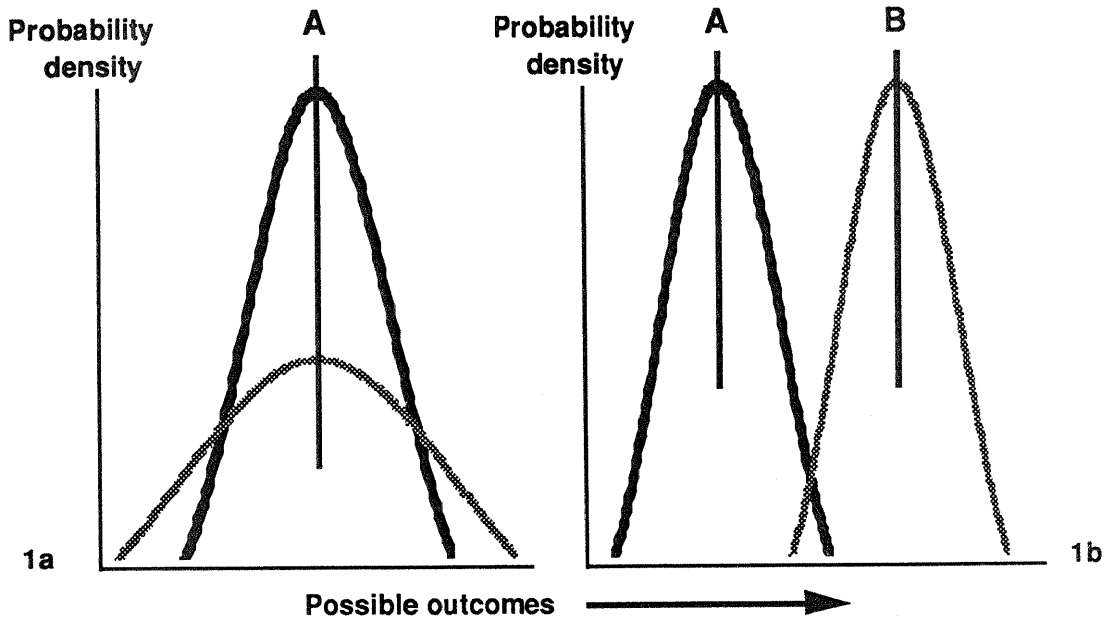

Figure 1. Possible impacts of new scientific knowledge. 1a. New knowledge can narrow the probability density for anticipated outcomes. 1b. New knowledge can lead to a discovery of alternative outcomes.

EVALUATING ECONOMIC IMPACTS OF SCIENTIFIC RESEARCH USING RISK ANALYSIS

This section outlines a stepwise procedure by which risk analysis and Monte Carlo simulation can be applied to the problem of evaluating the impacts of scientific research on economic decision-making criteria. (This paper does not attempt to describe methods of risk analysis or Monte Carlo simulation; the text by Hertz and Thomas (1983) or other literature on the subject can provide further details.) Figure 2 illustrates the proposed evaluation steps.

•Step 1. After selecting an area of scientific research for evaluation, identify the kinds of practical new knowledge that the research produces or is expected to produce.

For example, if one were evaluating scientific research in forest ecology, one might identify practical new knowledge about factors that constrain or influence timber growth and timber mortality in particular forest ecosystems.

•Step 2. For each kind of practical new knowledge identified, identify events related to economic activities about

which the new knowledge provides greater certainty, innovation, or discovery about possible outcomes.

For example, in the area of forest ecology research, one might identify biological events, such as timber growth and mortality, that are related to economic activities such as investment in timber stand improvement, investment in timber regeneration, or investment in forest protection. New knowledge about those biological events can provide greater certainty, innovation, or discovery about possible outcomes of those economic activities.

•Step 3. For the principal events underlying the economic activities, describe preexisting knowledge about possible outcomes by estimating the probability density functions for event outcomes as they are anticipated prior to the scientific research.

For example, in the area of forestry, one could define preexisting knowledge about biological events such as timber growth or timber mortality by estimating the probability distribution for possible outcomes of those events as they are understood or anticipated prior to the scientific research. The probability distributions would show the range and likelihood of various outcomes that are anticipated with prior knowledge about factors influencing those events.

•Step 4. Use a rational economic decision-making criteria to describe consequences of economic activities, by defining an economic function across the range of possible event outcomes that are anticipated prior to the scientific research, and use Monte Carlo simulation to simulate the range and likelihood of economic consequences. Simulated economic consequences will show the likelihood of economic success and the risks of economic failure that are anticipated for economic activities prior to the scientific research.

For example, in the forestry area, one could use a traditional economic decision-making criterion such as net present value of forestry investment activities, with the economic criterion expressed as a function of event outcomes such as timber stand mortality and rate of growth. Once the appropriate mathematical function is defined, Monte Carlo simulation would be used to predict the range and likelihood of various economic consequences based on the probability distribution of events estimated in Step 3. Results will show the anticipated range and likelihood of net present values for forestry investments as they exist prior to the scientific research.

•Step 5. Describe impacts of new knowledge, typically as either a narrowing or shifting of the probability density functions for outcomes of events, by estimating the new probability density functions for possible outcomes as they exist or are anticipated to exist after the research is completed. New outcome distributions will reflect new scientific knowledge about outcomes, including greater certainty and development of alternative outcomes.

For example, in the forestry area, one could define new (postresearch) knowledge about possible timber stand mortality or rate of growth by estimating new probability density functions for possible outcomes of those events. The probability density functions would show the new range and likelihood of events that are possible with new research knowledge, including greater certainty about outcomes and discovery of alternative outcome possibilities for timber growth and mortality.

•Step 6. Use the same economic criteria and Monte Carlo simulation technique as employed in Step 4 to simulate the range and likelihood of economic consequences associated with outcomes anticipated in Step 5. Simulated economic consequences will show the risks of economic failure and the likelihood of economic success that are possible for economic activities after the research is completed.

In the forestry area, one would use the same functional relation defined in Step 4 between economic criteria, such as net present value, and outcomes of underlying events. One would substitute the new distributions of possible outcomes of

events defined in Step 5 and then repeat the simulation as performed in Step 4. Results will show the possible range and likelihood of net present value for forestry investments as they would exist after the scientific research is completed.

•Step 7. Compare results of Step 6 with results of Step 4 to estimate the impacts on economic decision-making criteria of the scientific research. Impacts should be analyzed in terms of reduced risk of economic failure or increased probability of economic success, by comparing the estimated probability distributions of economic consequences derived from Steps 4 and 6.

Using this procedure, the impact of scientific research on economic decision-making criteria can be evaluated, regardless of whether the impact is to increase the certainty of knowledge about possible outcomes or to provide knowledge about previously unanticipated outcomes. Both impacts will influence the risk of failure or likelihood of success for economic activities. As such, results of

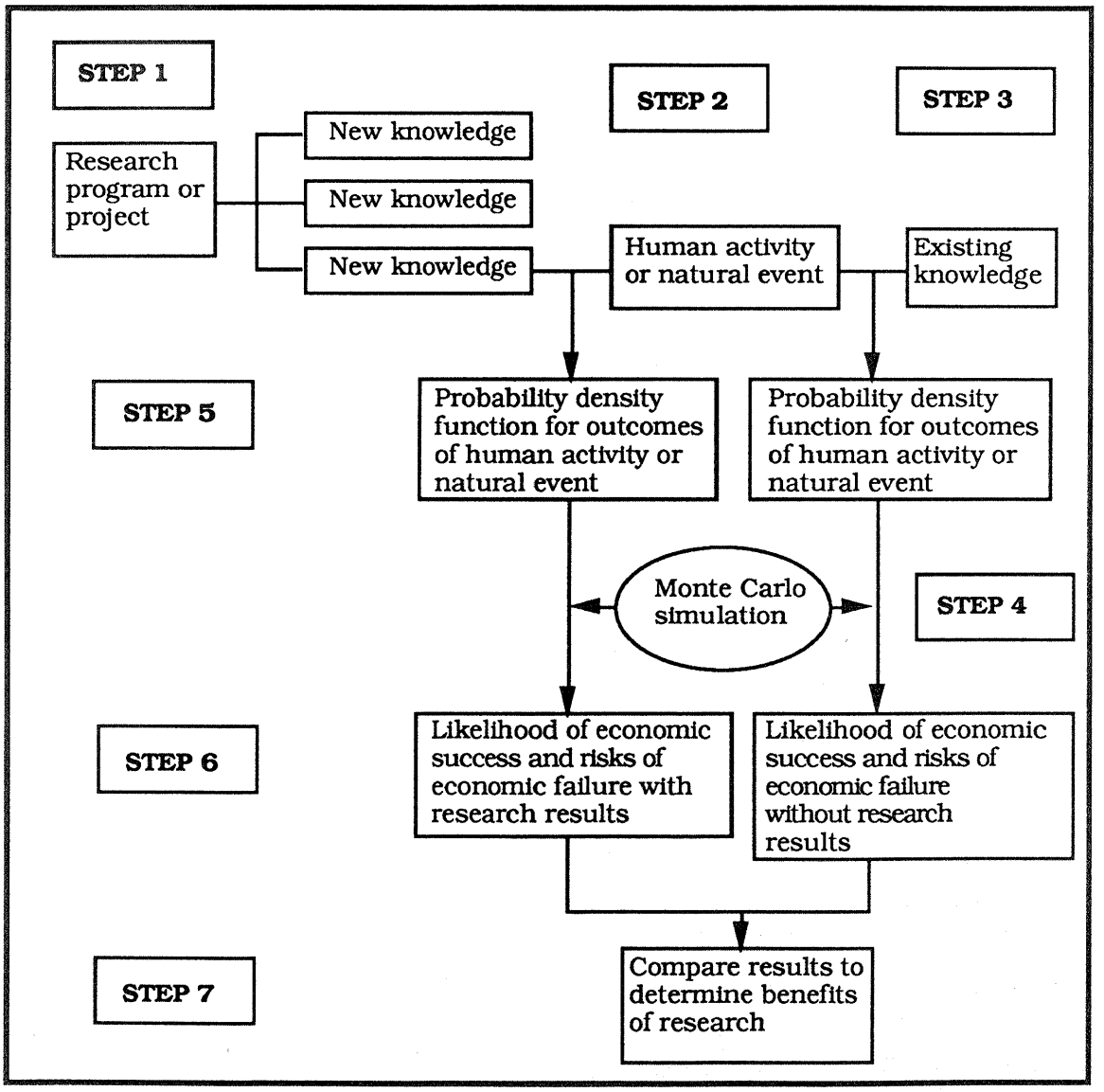

Figure 2. A method for evaluating the impacts of scientific research on economic decision-making criteria.

analysis will illustrate the economic decision-making consequences of scientific research in terms of both greater certainty about economic consequences and selection of new modes of economic activity.

CONCLUDING REMARKS

Outputs of scientific research may be evaluated by examining both the impact of greater certainty about outcomes of events and the impact of discovery of unanticipated outcomes. Knowledge about possible outcomes of events in nature or outcomes of human activities are represented by estimated probability density functions. Such functions may be described both as they exist prior to scientific research and as they exist or would exist after scientific research is completed. Given economic decision-making criteria that depend rationally on those outcomes, the methods of risk analysis and Monte Carlo simulation can be used to estimate the range and likelihood for economic consequences both before and after the research is completed. The impact of research on decision-making criteria may be evalutated by comparing risks of economic failure or probabilities of economic success between situations before and after research is completed.

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EXPERIENCES WITH A SCIENTIFIC RESEARCH STUDY GROUP

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Abstract. In November 1982, personnel with the North Central Forest Experiment Station at East Lansing organized a scientific study group. The general objective was to discuss and evaluate "Ideas and Methods Leading to More Effective Research." Since then, the group has met at 2- to 3-week intervals with a fluctuating attendance of from 5 to 18 people, depending upon the topic discussed and the members' schedules.

Three elements had to be present before forming the study group: complete backing and participation of the work/project supervisors; a conscientious group leader; and a nucleus of four to eight interested members. These elements were essential not only for establishing the study group but also to keep it going.

Subject material has covered a diversity of topics including (in no particular order): methods of formulating problem statements and hypotheses;quality research circles; "creation science"; using a major library to advantage; science within government, research quality awards, hunting Big Foot — legitimate science?; a defense of ontology (metaphysics); scientific rewards; implications of archetypal symbology; scientific strategies; and partial to complete coverage of four books that emphasized the meaning and definitions of science, the foundations of behavioral research, and the practice of social research.

Topics are chosen by group consensus. Some group members are most comfortable within a well-structured syllabus, almost a classroom situation. Others become bored with a rigorous structure and opt for changing topics from session to session. One of the duties of the group leader is to strike a topic balance. Additional duties of the group leader include appointing a discussion leader for each session, finding outside speakers when available, posting minutes and agendas, and making background readings available.

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Presents fifteen papers and four abstracts in five topic areas: The research process, Forestry constructs and innovations, Interdisciplinarity, Emerging research areas, and Assessing research productivity, quality, and motivating scientists.

KEY WORDS: Research methods, research strategies, interdisciplinarity, research productivity, research rewards, Delphi, organization theory.

Our job at the North Central Forest Experiment Station is discovering and creating new knowledge and technology in the field of natural resources and conveying this information to the people who can use it. As a new generation of forests emerges in our region, managers are confronted with two unique challenges: (1) Dealing with the great diversity in composition, quality, and ownership of the forests, and (2) Reconciling the conflicting demands of the people who use them. Helping the forest manager meet these challenges while protecting the environment is what research at North Central is all about.