Policy Sciences **31**: 55–80, 1998.

Closing the gap between ecosystem management and ecosystem research

JOYCE BERRY,[a] GARRY D. BREWER,[b]* JOHN C. GORDON,[c] & DAVID R. PATTON[d]

[a]*Environment and Natural Resources Policy Institute, Colorado State University;* [b]*Frederick A. Erb Environmental Management Institute, University of Michigan;* [c]*School of Forestry and Environmental Studies, Yale University;* [d]*College of Ecosystem Science and Management, Northern Arizona University;* **Correspondence should be directed: Professor Garry D. Brewer, Erb Environmental Management Institute, Dana Building, University of Michigan, Ann Arbor, MI 48109-1115, U.S.A.*

There exists a large gap between the operational needs of those responsible for ecosystems management and the knowledge required to meet those needs. The gap exists generally despite numerous calls to align resource and environmental management with essential natural and biological scientific realities. More surprisingly, the gap persists even where notable efforts have been made to close it.

Describing a vision for ecosystem management and research and then comparing this to reality reveals several basic problems. A poor appreciation for the durability of human organizations and routines, and a consequent need to plan and execute their termination, is commonplace. Inconsistent, unreliable, or questionable environmental measurements of such key concepts as ecosystem health or sustainability do little to inform, much less improve, managerial decisions and performance (Calow, 1995; Lele and Norgaard, 1995). Inadequate attention to specific contextual details, including differences between ecosystems and changes occurring within them over time, is another serious matter.

Moving beyond honest diagnosis to find ways to close the gap between ecosystem management and the knowledge it requires to operate is a demanding task. It is also absolutely essential if science is indeed ever going to serve in the fashion it must.

What's the vision?

The concepts of ecosystem management and ecosystem research are at once both simple and remarkably complicated, the latter point being made in a growing body of literature in many different disciplines and fields. Complications and differences aside, simple definitions are possible and general principles can be derived to clarify a vision linking ecosystem management and research (Gordon, 1994).

Ecosystem definitions

The term ecosystem was early defined as the whole complex of physical factors forming the environment (Tansley, 1935). More recent definitions clarify the concept:

> Any unit that includes all of the organisms in a given area interacting with the physical environment so that a flow of energy leads to a clearly defined trophic structure, biotic diversity, and material cycles is an ecological system or ecosystem (Odum, 1971).

Basically an ecosystem is an energy-nutrient processing system. Ecosystems have physical *structures*, as for example soil, plants, and animals, as well as *functions* of energy flow and nutrient cycling involving biotic and abiotic components. In addition, ecosystems have time and space attributes. A *system* is a set or arrangement of related or connected things forming a unity or whole (Bormann and Likens, 1979). The 'eco' part of ecosystem directs attention to the fact that the systems of concern are natural. Scientists and managers are lately beginning to realize that ecosystems also have an important human dimension. Humans are ecological dominants because they are able to alter the environments of other species. Many of these alterations are unplanned, while a select few are the product of conscious thought and active intervention.

Within this small set of planned and intentional acts, ecosystem management stands out, mainly in proposals to use it as a new framework for administering wildlands, forests, watersheds, and the like. There are many competing and changing definitions of ecosystem management, but the following are in use and help illustrate the concept.

> A strategy or plan to provide for all associated organisms, as opposed to a strategy or plan for managing individual species (Forest Ecosystem Management Team, 1993).

> The careful and skillful use of ecological, economic, social and managerial principles in managing ecosystems to produce, restore, or sustain ecological integrity and desired conditions, uses, products, values, and services over the long term (Overbay, 1992).

> Restoring and maintaining the health, sustainability, and biological diversity of ecosystems while supporting sustainable economies and communities (U.S. Environmental Protection Agency, 1994).

> The integration of scientific knowledge or ecological relationships within a complex of sociopolitical and values framework toward the general goal of protecting native ecosystem integrity over the long term (Grumbine, 1994).

Ecosystem management is a philosophy and a way of doing business. It involves coordinating resource planning at the local level, forming partnerships, communicating benefits and cost, and educating people – all the while using the best scientific and technical information available (Dombeck, 1995). Ecosystem management considers a unit of land as a system, e.g., a watershed (Wilkinson, 1997). The parts of the system are the plants, animals, soil, climate, water, air, topography, and human influences comprising the whole. Any given factor in the system can affect or be affected by other factors in a cycle of events. Understanding the cycle of events in specific settings over time well enough to respond appropriately and adapt successfully is the core concept of ecosystem management.

Ecosystem management is adaptive because our knowledge is limited (Holling, 1978). Uncertainty overcomes our capacity to understand and thus limits predictability. Managers nonetheless must still make decisions, typically based on far from complete or even reliable information (Ludwig, Hilborn and Walters, 1993).

Even more basic issues, such as the appropriate size of an ecosystem to be managed, remain unresolved. The Society of American Foresters (1993) suggests that 100,000 to 1 million acres might be right. Landscape, watershed, and other scales are often promoted as well (Noss, 1983; O'Neill et al., 1986). Whatever the scale, a nearly inevitable feature of ecosystems is the need to manage them across existing boundaries, with a resultant heightening of concerns about property rights, public and private disputes, conflicting laws, and a general lack of trust among stakeholders and those having decision-making responsibilities.

The scalar problem in ecosystem management is usually settled by linking natural components together in sufficiently large areas to be of use to humans. The human component is also linked to the capacity of the system to produce and the willingness of people to accept that limits exist and that the system will certainly change through time and perhaps even in space.

No single operational definition of ecosystem management exists, although its basic principles are understood. A comparable situation occurs with respect to research to inform and improve ecosystem management, i.e., no generally accepted definition, but ample concrete instances. Research about ecosystems meant to inform and serve managers includes questions posed by all disciplines about the application of ecosystem management in actual settings as well as all research focused on understanding relationships among ecosystem components, including people.

Users of research

Research is a process used to create new knowledge. In a common guise, such knowledge accumulates by using the scientific method consisting of experiments with controls and replications. Peer reviewed publications and independent validations add confidence in research results as well as extending the possible scope of application for the new knowledge.

Ecosystem management research pertains to and most benefits those having decision-making responsibilities in specific settings. In the U.S. context this includes managers in the Forest Service, Bureau of Land Management, Minerals Management Service, the Park Service, Bureau of Indian Affairs, large private companies, tribes, farmers, and other private landowners. Regulatory agencies, such as the Environmental Protection Agency and the Fish and Wildlife Service, make extensive use of ecosystem management research.

Research is most often focused on individual system components, such as plants, animals, soil, and water. Consequently, many different professionals are employed to administer and conduct the research. Duplication of effort is common, while the establishment of research priorities is problematic. Relatively rarely are researchers tasked to integrate, synthesize, and interpret the results from individual component studies, despite the growing needs of managers and policymakers for detailed ecosystem information at the local, regional, national, and international levels. The importance of integration and synthesis of accumulating knowledge, especially its interpretation with respect to specific places and problems, cannot be overemphasized.

Other, more diffuse, uses of research knowledge occur, as in improvements in basic educational curricula and in the level of public understanding of environmental matters. As important as these other uses probably are, the basic task is to close the gap between the operational needs of those responsible for ecosystems management and the knowledge required to meet those needs.

Setting priorities and evaluating quality

Deciding what one needs to know and then assessing the quality of information produced as a consequence are basic problems against which equally basic and clear criteria must be brought to bear. Five helpful criteria are quality, merit, utility, the potential to improve science, and the potential to improve management (National Science Foundation, 1994).

Quality encompasses the capability of the researcher, the technical soundness of the proposed approach, and the adequacy of the institutional resources available. Merit concerns the likelihood that research will lead to new discoveries, foster advances, or have other valuable scientific impacts. Utility goes beyond a specific field or discipline to include achievement of policy or managerial goals or the development of products or processes. More robust criteria, such as the potential of research to improve science or to improve management, are also worth consideration, especially the latter given our present purposes.

Estimating the potential of a given research project to improve management is absolutely vital. Any attempt to manage an ecosystem depends mightily on a constant input of timely and high-quality scientific information. Without a research product tied directly to ongoing decision-making needs, no ecosystem can be managed very well – if at all.

Setting priorities for ecosystem management research means asking and

answering several questions in addition to those posed when evaluating ordinary research (U.S. Department of Agriculture, 1994):

- Is the research problem appropriate to the mission? Will research results inform managers and improve operations?
- Can the research be done? Can it be done in a timely and cost-effective manner?
- Are the results likely to be used? What are the likely outcomes and effects of using the research?

These questions apply ordinarily to ecosystem management. But when governments, state and federal agencies, face shrinking budgets and dwindling personnel, extraordinary attention must focus on setting priorities, evaluating quality, and managing more effectively.

Curiosity-driven, or basic, research continues to play a role in ecosystem management, but it is not a primary or high priority one at all. The accumulation of knowledge in general, and its interpretation and transfer to specific settings, will of course continue and have value. What we emphasize is an increasingly urgent need to balance information required by ecosystem managers against that traditionally sought by scientists and the science disciplines.

What's the reality?

Commissioned by the U.S. Environmental Protection Agency and constituted in June 1995 as the Independent Review Group (IRG), we were asked to assess the current state of ecosystem management research in the Pacific Northwest region and to recommend ways to improve it. For the purposes of the project, the Pacific Northwest includes Oregon, Washington, Northern California, and the portions of Idaho and Montana included in the Interior Columbia Basin Assessment (Thomas and Baca, 1994). The result is not a comprehensive scientific critique of research. Rather it focuses on problems of research management, a choice driven by the clear and consistent findings we obtained in interviews and a survey of scientists, ecosystem managers, and stakeholders throughout the region. Additionally, from our conversations and interviews with Congressional and other sources outside the region, it is unmistakable, 'business as usual' is over. What should replace the current arrangements and how to make the changes are not as readily apparent.

Our approach

Three basic questions motivated our work: What is the role of research in ecosystem management? What types of research are being conducted and which institutions are involved? And, what is the size of the research effort in terms of funding and personnel?

During the 12-month field portion of the project, which ended in May 1996, we engaged in four different activities to gather information to answer these questions and upon which to base our recommendations:

- Literature and document search: Searched Current Research Information System (CRIS) reports, bibliographies, synthesis publications, and records of prior assessments.
- Interviews and surveys: Solicited the opinions of key scientists, science managers, resource managers, interested groups, tribes, and national policymakers on the current state of ecosystem management research though in-person and telephone interviews and surveys.
- Focus groups: Used a structured process to elicit, rank, and analyze priority issues in two formal focus group sessions and several follow-up group discussions.
- Feed back: Sought review of our provisional findings and recommendations from those we interviewed and surveyed, as well as selected other knowledgeable reviewers.

We also sought 'lessons learned' and potential models for improvement from other parts of the United States, e.g., the Front Range Study in Colorado and the Southern Appalachian Assessment. We are also aware of large changes occurring or impending in the relationships between science and society in the country as a whole. As Byerly and Pielke (1995: p. 1531) put it:

> The postwar ecology of science isolates research from both practical applications and the very environment which today presses it to demonstrate efficacy with respect to the solution of practical problems. This pressure stresses the structure of postwar science policy, creating the crisis.

The imperative to implement ecosystem management, the practical problem, has obviously stressed the structure of natural resource research policy in the Pacific Northwest. Our findings reflect the outcomes of this stress, and our recommendations are intended to alleviate it.

Sources of information

The literature and document search, including several electronic data bases and sources, proved inadequate to answer our questions. Electronic sources yielded information of variable quality, even in the face of repeated, increasingly focused keyword searches. At best these data bases contain project descriptions and information about individual projects with respect to principal investigators, institutions, and the general status of the research. Typically, the data bases did not contain budget information, at either the individual project level or summarized by projects or agencies.

Puzzled by the lack of such information, and optimistic that someone must

know the whereabouts of basic ecosystem-wide management data, we wrote and telephoned individuals in all of the relevant resources management agencies in the PNW. Optimism soon faded: *No main source of ecosystem management research information exists in the Pacific Northwest region.* In fact, what information we did obtain came in bits and pieces during personal interviews with agency administrators and individual researchers. It also came from intensive investigations during two separate focus group sessions and as a reaction from those to whom we circulated a preliminary draft of our recommendations. The results of each are next presented.

Interviews and focus groups

During this project we were fortunate to have been able to interview a general cross section of researchers and practitioners in the Pacific Northwest and elsewhere. Not only did we benefit from their experience, but we were also able to calibrate our own emerging sense for the problems and opportunities in the region. We wish to acknowledge the generosity and help of our various respondents while simultaneously reassuring them of our pledge of confidentiality for their specific responses.

The following summarizes the general themes and ideas discovered in our interviews into several broad categories. A basic, topical questionnaire helped to structure all interviews and to ensure comparable coverage in the topics discussed. Additional detailed information generated during the interviews which illuminates specific important matters is also noted, as appropriate. To convey a sense of what we heard, but without compromising the identify of individual informants, editorial license has been taken in paraphrasing some responses. Where possible, actual quotations are provided and are noted as such, but without specific attribution.

Polarization and fragmentation: From beginning to end and from nearly every respondent an overwhelming theme is the evident polarization of stakeholders and the related fragmentation of organizations and research efforts. As one respondent put it: 'Everything is one giant collage with everyone pushing their own agenda.'

Polarization comes in different forms, several of which result in conflict and confusion as well as the fragmentation just noted. Basic research competes with applied and policy-directed efforts, often with basic researchers having to camouflage their work underneath the 'ecosystem management' banner to win support. Everyone's admitted inability to determine total research expenditures and categorical breakdowns between applied and basic efforts is indicative.

Polarization between use and preservation values related to resources is well known, divisive, and enervating throughout the region. Little evidence exists of effective efforts to break the logjam it has created or to imagine changes in land and management practices to improve current practices and cool the heated rhetoric. Many respondents described the problem and some of its consequences.

Two or three exercised imagination by suggesting new arrangements for the land between public and private management regimes: For instance, lands for protection and preservation of natural systems, e.g., national parks; private lands for commodity production, but with adequate environmental safeguards; and public lands managed for amenities and wildlife, but from which timber production could happen if needed.

Compared to these on-the-ground problems, those perceived by many federal decision makers are somewhat different, but equally difficult. 'Ecosystem management studies seem to be proliferating, with funds directed for a variety of things without a clearinghouse [or some means] for discussion, direction, and prevention of duplication.' This duplication and overlap 'produce dusty reports' when what is really needed is 'an entity to direct and coordinate, … to decide what makes sense, how to go to work quickly, how to get the attention of the agencies, and how to give the big picture overview.'

From a national perspective being able to direct and coordinate has obvious benefits, not the least of which being 'an ability to pull diverse views together' and to make 'tough choices about diverse values.' Indeed, ecosystem management has often been touted as a means to achieve just such human and political ends, in addition to more readily apparent biological and physical ones. Two consistent themes followed from this general line of discussion.

The 'human dimension' is at least as important as the biological one; however, human concerns in ecosystem management are often treated casually, as an afterthought, or simply ignored. Harmful consequences follow, as with forest plans, 'where 80 percent of the problem of implementation comes because we didn't tie the research recommendations to institutional implementation. People do not recognize the importance of institutions in trying to apply ecosystem management.'

The term 'ecosystem management' is increasingly burdened with heavy political connotations, 'ecocentric management' in the words of one knowledgeable respondent. To another, efforts to define the term have become a substitute for actually trying to do it. 'We need to demonstrate what ecosystem management is, what people like or don't like about it. We need a "I-know-it-when-I-see-it" operational definition to stop wasting so much time and effort.'

Indeed, determining 'What's in a name?' turns out to be one of the most general themes we encountered in all of our interviews. Equally prevalent were concerns about coordination, especially as between federal/state and public/private interests from a management perspective and between basic and applied efforts from a research viewpoint. These general topics are elaborated below.

What's in a name?: The key concepts of ecosystem management (EM), adaptive management (AM), and ecosystem management research (EMR) are far from being consistently defined or generally understood, at least by those we interviewed. This might not create problems, except for the fact that our informants are responsible for the concepts' implementation. Clearly the concepts mean to identify something new and different from whatever had been happening before. Change is the goal. Not so clearly are the concrete ways in which the

concepts are made operational. Or, in the plain words of one respondent, 'EM and EMR need to respond to real world management needs, to link research with policy and management, to find forums to identify issues sooner and more clearly.' With respect to adaptive management, a comparable deficiency and added confusion exist. Is it a research approach or a policy regime?

> I've got no quarrel with this concept. Someone should be responsible for testing ideas and integrating them into management. Some researchers should be doing this, but not all because they may lose credibility by becoming advocates for policy positions.

The confusion was restated often but we heard few constructive remedies for it. One of these exceptions focused on operations, doing things to identify and solve concrete problems, such as

> Finding areas of emphasis in EM, with more careful problem analysis. Integrating problem solving to avoid the stove-pipe mentality of separating air and water or according to the disciplines. Involving the right groups – getting them involved and finding approaches that work to solve real problems.

The concepts mean many different things to those involved, and these differences are sometimes used as an excuse not to work for change. In the view of one highly placed federal official, this is critically important because

> We now have a once-a-century opportunity to make monumental changes in people's relationships to nature. We are beginning to have a science of natural values similar to Native American values, and multiple use is becoming multiple values.

To impose such a heavy burden on EM/AM/EMR may well doom these concepts to failure. Treating them as grist to be consumed in the political mill is not without precedent, however fanciful:

> 'When I use a word,' Humpty Dumpty said in a rather a scornful tone, 'it means just what I choose it to mean – neither more nor less.'
>
> 'The question is,' said Alice, 'Whether you can make words mean so many different things.'
>
> 'The question is,' said Humpty Dumpty, 'which is to be master – that's all' (Carroll, 1892): p. 124.

Mastery, in the real world of ecosystem management, often boils down to struggles over who is in charge. Such contests are waged in an arena called coordination.

Coordination: 'There is undoubtedly a need for better coordination of EMR in the Pacific Northwest,' said one senior Forest Service official. To another, 'coordination in the Pacific Northwest is a horrible example.' And so the story goes throughout our interview sample.

Coordination between changing federal and state entities is obviously one dimension of the problem. Budgetary upheavals and political changes in Washington, DC are notable and will continue to affect research and operations in the states and regions. 'Making ecosystems a line item decision [in the Forest Service] caused chaos. Instead of creating new boxes, they should have looked at what programs accomplish and classify them that way. Focus on what we are trying to accomplish.' Political changes matter every bit as much as budgetary ones, or in the blunt assessment of one DC official, 'There is little support for the term ecosystem management in Congress except for [former] Senator Hatfield.'

Changes at the state and local levels matter, too, as the following comments from a regional official suggest.

> How do we improve water quality management and incorporate it into the President's Plan and REO [Regional Ecosystem Office, Portland, OR]? We need to think more broadly… to deliver services at the community level and [to include] big state programs in Washington and Oregon, which are not well integrated and coordinated with EPA and each other as they should be.

Under these less than ideal circumstances, in the words of one federal official,

> The only way to make coordination work is to have the money in the hands of the coordinating body. You can't have the money trickle down from the agencies. The [coordinating] group will figure out how to spend the money, pick the projects, and make sure the results get out. *The key is the power to control the money.* [Emphasis added.]

Unstated, in this or any of the other interviews, is how this coordinating body might be constituted or legitimized and where to locate it – all topics we consider later.

Coordination problems occur in a second, quite different, form when public and private conflicts emerge. The implications for research and action are beginning to come clear.

> It is important to know more about public/private issues, takings, and their implications for broader public policy… . We need to link agencies, universities, and private research sources. State and interagency forums that now exist could help in this role. We need to connect with NGOs [to improve] issue identification and research capability.

The role of private industry in ecosystem management and research is not well defined, although several in our sample saw the need for more industry involvement to improve coordination and accountability. This from a Washington, DC official, 'Private industry can and should be pulled into an improved coordinating mechanism.' A university-based researcher pursued this line:

> A coordinating board has to involve managers and stakeholders from the private sector. The board wouldn't hamper creativity. It would give you accountability while not limiting ways to address issues. There has not been much accountability in terms of products. If you [researchers] were ever productive, then there were no rewards. I think privatization is a good idea. It is time to make changes.

Once again, how to make change occur is the key question, the answer to which is having the power to control the money. To make coordination work the money must be controlled by the coordinating body. Trickle down from on high in Washington, DC does not work, nor does reliance on coop units or university based centers or institutes.

> Coordination with state and among state and federal agencies has gotten worse. Current [state] Department of Natural Resources director is decoupling from traditional clients, becoming ecocentric. There is also a deterioration of relations between DNR and the university.

> Universities don't have much to say regarding federal money or much influence over how coop units spend money. Universities do support initiatives with Congress.

This same point another respondent noted is the hallmark of research relationships in the Pacific Northwest:

> The history of the Northwest is earmarks. There is a perception in Congress that only earmarks work because things are so busted. [As a consequence] no one has any idea of how federal agencies, universities, anyone, is doing. There appears to be no systematic analysis of what needs to be done or where resources might be.

Basic and applied research: When policy and program needs are not well understood, there is little reason to expect research to be well tuned toward them. In the case of basic research, this is probably an acceptable situation; in the case of applied research it clearly is not. The matter is far from cut and dried for ecosystem management research, where questions such as 'What is research?' are even being debated.

> 'What is research?' One can't look just at research appropriations because probably 15 percent or more is spent on administrative studies that are like research. Also some evaluation of the links between research and management are underway, but this has been difficult.

The fabled 'Two Cultures' first identified in World War II by C. P. Snow (1959) allegedly persist in EMR:

> The Columbia River Basin and President's Plan made the 'two cultures' problem apparent. The National Forest Service is oriented to specific, finite tasks with definite deadlines and budgets. Researchers allocate the time and money it takes to solve a specific scientific problem in a more deliberative process designed to achieve a very high confidence level in results. This makes the process slow.

The gap between research and action is frequently recognized when clients and users complain that 'researchers aren't working on things that are important to us,' or when 'private sector stakeholders criticize government and university scientists for being irrelevant or 'unable to satisfy our needs.'

These problems, one needs to be reminded, are chronic and have not been generally well managed anywhere. Nonetheless, it is still possible to state several criteria which, if achieved, could result in a better match between research capabilities and program needs. One of our respondents put the matter in these terms:

> Don't turn scientists loose with unlimited budgets. Don't have scientists identify questions, they should only refine them. [However,] scientists need to be independent, to be provided finite money and time. They should get their questions from management, and the success of matching science with programs must be monitored by keeping track of implemented decisions.

In a very basic sense these criteria are yet another plea for improved coordination – better communication between all the relevant stakeholders. 'Using existing forums and mechanisms better, adding user-researcher interactions to agendas. Focusing research on emerging issues.'

While hard to prove in any rigorous fashion, the realization that the major constraint limiting ecosystem management is human or institutional, not scientific, continues to taunt. It may even have more specific effects. In the view of one senior manager, 'There is not much science going on now because everyone is so involved with process. The research community is in as much chaos as the managers. Academics are going back to little problems they know how to confront, not the ones which are relevant to ecosystem management.' When managers don't know what they want, it's hard to blame researchers who 'bootleg their own work along with applied research.' Since 'no one knows how well [applied] research is serving the clients or satisfying their needs,' what's the harm?

The harm is clear enough as measured in increased public cynicism and heightened political conflict and mistrust. From this perspective of trying to avoid such harmful outcomes, one respondent offered several criteria (which we elaborate) to assess the effectiveness, efficiency, and 'quality' of ecosystem management research.

- What are the linkages between the research and emerging public policy issues? Are they numerous and solid and becoming more so? If not, why? Who is responsible, for better or worse?
- What means and lines of communication exist between decision makers, researchers, and other stakeholders? Are efforts in place to ensure and/or improve such means and lines? If not, why not? Who is responsible, for better or worse?
- From the ongoing interaction of ecosystem management and ecosystem research are questions suitable for basic scientific inquiry generated? If so, are adequate funds and appropriately qualified individuals given responsibility for the identified work? If not, why not? Who is responsible, for better or worse?

These topics all stood out prominently in two separate focus groups we convened to elaborate and probe the following four questions more deeply. 1. What are the barriers to ecosystem management research? 2. What are the opportunities? 3. What is working well with the Interior Columbia River Basin Project? 4. What needs improvement? Questions #1 and 2 guided work at Portland, Oregon on June 30, 1995 and Questions #3 and 4 were asked in Walla Walla, Washington on September 13, 1995. Using the nominal group method, each of the 22 participants was asked to answer the questions. These responses were summarized and afterwards each person was asked to distribute a total of 10 points to the item or items of most importance. One-to-one discussions then followed to clarify and amplify the meanings associated with the items most highly valued.

The following topics emerged, in descending order, to characterize barriers:

- There is no clear picture of public expectations or understandings. No one knows about society's long-term wants and needs.
- There are not enough resources being spent on linking management to learning – about ecosystems, including the human dimensions.
- Institutional inertia, e.g., turf battles and legislative mandates from the past, impede. So, too, do different organizational cultures.
- There is no long-term policy, budgetary, or research perspective.
- Politics influences the research, which needs to be protected as a result.

Different topics emerged as possible opportunities flowing from current efforts to conduct ecosystem management research (again, listed in descending order):

- The Northwest Forest Plan is a chance to do Adaptive Management, to facilitate information exchange, and to help create links and bridges between researchers and managers.
- Genuine research and management partnerships can be forged.
- Anticipatory research to provide information for future management decisions can be produced.

In each instance, the opportunities were yet to be realized, although respondents could see their potential in efforts being expended generally on behalf of ecosystem management and specifically through the Northwest Forest Plan.

The Walla Walla focus group was made up of participants in the inter-agency, Interior Columbia River Basin Project. As such each had made a significant professional commitment to ecosystem management and to discovering better ways to link research to its needs. Question #3, 'What is working well?' thus highlighted the positive experiences of this diverse group and, in so doing, generated a long list of individual replies from which these basic themes emerged:

- Outstanding public involvement at all stages improved every aspect of the work of the Project. Involvement and communication have resulted in notably increased public trust and confidence.
- Working at the 'right' scale, of the relevant regional ecosystem, has lead to increased efficiencies in data collection and use, in relevance of the research, communication between agencies and researchers, and quality of the research product.
- Working as a team has improved research productivity and efficiency while simultaneously improving the product's timeliness and use to managers. Professional development and growth (learning) were consistently noted as unexpected benefits.
- Leadership at the top and the personal commitment of the large majority of those involved in the Project stood out in the collective experience.

Not a perfect operation, the Interior Columbia River Basin Project stimulated suggestions for improvement, too. Or, as replies to Question #4 'What needs improvement?' these themes emerged:

- While the regional spatial dimension is an improvement, the time frame for the project is too short. This pertains to the funding time horizon as well as to the ecosystem processes being incorporated into the management analyses and plans. The pressure to generate quick results is not in keeping with the long-term nature and difficulty of the problems.
- The research is not connected to the realities of the policy process – including problem definition, alternative generation and estimation, decision making, and implementation.

One needs to be reminded that the Walla Walla group was not typical but rather was selected as a pioneering effort to explore better ways of doing ecosystem management and research. Now concluded, the ultimate fate of this effort and of those directly involved is still undetermined.

The overarching themes of polarization and fragmentation noted earlier in our interview responses are more typical and in fact point out serious common problems of accountability for ecosystem management and ecosystem research. At least this is the strong consensus judgment. It is a difficult charge either to ignore or deny, which is exactly what seems to be going on. And the unanswered question of accountability of course reminds us of another yet question early posed: 'The question is,' said Humpty Dumpty, 'which is to be master – that's all.' Unfortunately, the answer is 'no one is master.' The system is out of control.

Findings: A system out of control

This strong consensus judgment is clarified and better understood in terms of the main findings from our assessment.

No one knows the level of effort being expended on ecosystem management research in the Pacific Northwest, and if no one is knowledgeable there, where would they be? Accurate budget figures are not available either from public or private organizations in the region or from federal agencies and offices in Washington, DC. Based on our interviews and direct requests to the U.S. Forest Service (USFS), the Office of Management and Budget (OMB), and the Office of Science and Technology Policy (OSTP), our best guess is that somewhere between $35 and $100 million per year is being spent by federal agencies in the Pacific Northwest for ecosystem management research. No one knows for sure.

No one knows and there is no way to determine whether the money spent on ecosystem management research is achieving stated public management goals. Inadequate monitoring programs are partly at fault, but the lack of money and people and a single institutional focus to provide accountability are more so.

There is a strong consensus among scientists, managers, and policy makers to create a more effective, accountable and reliable system for coordinating and managing ecosystem research in the Pacific Northwest. No single office or agency provides leadership or is responsible for this system.

Having no such system, the implementation of adaptive ecosystem management and supporting research in the Pacific Northwest, especially on federal lands, is flawed and essentially failing. There is even reason to doubt that conventional 'business as usual' is much changed, this despite clear needs and highest level demands to move beyond it to embrace the ecosystem management approach (Thomas, 1994; Espy and Babbitt, 1994; National Research Council, 1992). The vision and the reality are different and still far apart.

Why the differences?

Organizations often 'say one thing' but actually 'do another.' The real or operating goals reveal what an organization and its leaders are in fact doing, legal prescriptions and public pronouncements aside (Perrow, 1961). Individuals operating within governmental organizations are not immune from the problem, as Ascher and Healy (1990: pp. 177–178) have noted. In various combinations their motivations are to enhance the standing of agencies in which they work, promote their own careers within these agencies or elsewhere, adhere to the highest professional standards (either for the sake of professionalism per se or to attain respect from professional peers), to pursue partisan political objectives, or to pursue a particular policy objective at any cost. Some or all of these possibilities might explain why ecosystem management and research are still so far from reality.

The Pacific Northwest is not the only instance where high-level visions and prescriptions for improved resource planning and management failed to come true. Might there be lessons waiting to be learned about these experiences from the past?

Lessons from the past

Elaborate planning and complex procedural remedies to ecosystem research and management problems may only make matters worse, at least that is the experience of both the Forest and Rangeland Renewable Resources Planning Act of 1974 and the National Forest Management Act of 1976 (RPA and NFMA, respectively). Congress thought that the RPA and NFMA would improve decision making and reduce conflict about use of the nation's forests and ranges. The basic assumption that more information is good can be seen underlying both pieces of legislation. Some at the time expressed hope that the Acts would encourage a long view and rational debate, rather than the myopia that usually attends legislative crises or the troublesome dictates of federal courts.

Other pertinent lessons can be learned from the RPA/NFMA experience, since these parallel as well as condition current efforts to use science and scientific information in ecosystem decision making (Sample, 1989). The most striking similarity, the unfulfilled need to build public trust, could well be overlooked. Or, as McQuillan states it with respect to the intent of RPA and NFMA, 'By sensitive application of rules of reason exposed to public scrutiny Congress hoped that the agency would demonstrate the reasonableness of its management decisions to a skeptical public and, as a consequence, restore public trust in the management professionals' (McQuillan, 1989: p. 71).

It did not work this way, unfortunately, as the Forest Service took the Congressional directive and turned it into a massive data collecting and technical planning exercise, the results of which are not impressive (DeBonis, 1991). And, according to Behan (1990: pp. 20–25):

> As of early 1990, 14 years after the passage of the NFMA, 92 of the 94 completed forest plans were under formal appeal. Five plans were in the courts, one had been declared illegal, and the others were in the administrative appeal process. There were 332 active appeals, brought by conservation organizations, timber and mining interests, off road vehicle interests, state and local government, native American interests, and private citizens.

All of the information and analysis afforded by the RPA/NFMA did not bring the interests to easy agreement, nor did it replace conflict with a reasoned search for optimality (Ascher and Healy, 1995: p. 8).

The political process seeks consensus, but the very act of exploring options and trying to attain consensus heightens awareness of the many interests and values at stake. It may very well be impossible to achieve agreement and specificity at the same time in such situations (Brewer and de Leon, 1989: p. 181). Furthermore, as disagreement escalates to conflict, as it often does, those having decision-making responsibility may only make matters worse by trying to exert power to regain control (Hrebiniak, 1978: p. 236).

Ritualistic, top-down actions supported by 'objective analysis' and 'facts' become tools in a contest to exert or regain control over 'unreasonable' or 'uninformed' opponents. Under the circumstances, admission of error, openness, and a willingness to embrace uncertainty in the interest of learning all receive little play or even attention. At least these appear to be key lessons from the RPA and NFMA experiences of the past.

Termination: Old business before the new

Those favoring new models of ecosystem science must also be mindful of old realities. A poor appreciation for the durability of human organizations and routines, and a consequent need to plan and execute their termination, is a commonplace lapse. Termination concerns the adjustment of policies, programs, or organizations that have become dysfunctional, redundant, outmoded, or unnecessary. Since existing commitments are real, involving people whose lives are invested in past decisions and practices, termination often involves resistance, hostility, and other passionate behaviors of those who stand to lose.

People naturally resist thinking about termination. Images of death and destruction are not too far off the mark. Resistance takes several forms: 'Why should I do things differently, I'm doing my job pretty well right now? I've only got a couple of years until retirement, why do I have to learn all this "new perspectives" stuff any way?' Another form goes as follows: 'Isn't ecosystem management what we've been doing around here for years? What's the big deal?' Expect, in any event, inertia of these and other frustrating forms. The basic problem is that the innovation one promotes is imaginary; the life one seeks to change is real. The costs of change are concrete and calculable; the benefits are ethereal and promissory.

From a base political perspective termination is difficult because it violates one of life's few reliable social laws: the political cost-benefit calculation. Politicians are motivated to act when doing so benefits them and their constituents, now, while the costs get paid later, by someone else. Think of this law as it affects even the best intentioned efforts to change to an ecosystem management regime. The calculation is all backward. Costs are going to be paid now by people who will be laid off or forced to change behaviors tried and true. The benefits are ethereal and promissory.

Starting with a blank piece of paper is a luxury few organizational leaders experience, especially in times of tight budgets. Indeed, getting any change at all often means innovation by substitution or replacement. But how does one proceed? Firing the recalcitrants might help speed changes, but firing is brutal and demoralizes everyone else in the process. Rearranging organization tables is the business-as-usual solution, but business as usual is exactly what must be changed.

Two quite different circumstances characterize problems of termination in the case of ecosystem management: The first is where ecosystems are valuable and healthy, such that many different people have an interest, and the second is where the ecosystem is degraded, often to the point where no one cares. One's approach to 'old business' will differ accordingly.

High value ecosystems, such as Yellowstone National Park and its adjacent national forests, present problems of the first sort (Primm and Clark, 1996). Degraded ecosystems, such as heavily polluted rivers and harbors, strip mines, or heavily deforested and eroded regions in the tropics are different and less problematic. Under these conditions, who literally cares? When degradation gets to the point where no one cares, there is good news, bad news, and potential disaster. The good news is that redefining political and economic constraints is easier since interested stakeholders are far fewer. The bad news is that there may be expensive and time consuming ecosystem rehabilitation to accomplish before any routine and productive uses can be realized. The worst case occurs when irrepairable harm has been done. Whether the current situation in the Pacific Northwest is perceived as, and thus better classified as, high-value or degraded may hold a key to the success of future efforts to change to a science-based ecosystem management regime.

The prevailing, high-value view appears to limit desired change. The numerous, overlapping and competing authorities can hardly result in a sensible ecosystem approach to management and research, especially if each authority is allowed to continue with business as usual. A first logical step, therefore, is to consolidate – to end or terminate whole organizations, particular jobs, and cherished ways of doing things from the past. Under these conditions, how optimistic can anyone be about getting constructive change?

An emerging sense of alarm that the ecosystems of the Pacific Northwest are distressed and becoming more so may allow change to occur (Lee, 1989; Muckleston, 1990; Lee, 1993; National Research Council, 1996). As evidence of failures, distress and crises mounts so, too, will popular demands to 'do some-

thing' to stem the tide or make amends. The practical dilemma is that ecosystem management may be most readily embraced in circumstances where there is very little valuable left to manage. A further complication in the degraded or crisis setting is that one must be extremely aware of the institutional and decision processes which are responsible for, which literally created, the mess at hand. Cleaning up and restoring, without assessing and correcting the historical reasons for the degradation, may end up making matters no better at all.

The matter of context

One of the simplest and most essential problems is to define the ecosystem. The scientific vision described earlier is not entirely consistent or helpful in specific settings. This is not to fault scientists; they have made notable progress here (National Research Council, 1986). The problems managers face usually boil down to ensuring that scientific definitions hold and are consistent for the periods of time that matter for planning and operating. There are two obvious components of ecosystem definition: Generally, what constitutes and thus defines something we call a wetland, a barrier beach, a watershed, and so forth? There is some consensus here, but it is hardly universal. Specifically, does the unique place we have before us 'fit' any of these general definitions? And to what degree? The *perfect* wetland is a fiction, the practical matter is trying to decide if 'this' wetland is close enough to the norm to qualify (Knickerbocker, 1991).

This is a far from trivial exercise or pedantic pursuit. For starters, stability in definition is required for longer term biological treatments, e.g., mitigation, refoliation, channeling, reclamation. The point here is that biological standards, including baselines from which all subsequent progress can be assessed, must be stable. If the definitions or baselines are not clearly spelled out or do not exist, how can plans be made or performance evaluated? Secondly, most biological processes are operating on much slower clocks, with longer time horizons, than political or economic ones. If the biological conditions and frameworks are not carefully spelled out and consistently defended, there is little reason to expect longer terms plans or programs to succeed. Political expediencies and short-run economic planning horizons will prevail.

Small-scale ecosystems may be more successful, from a bureaucratic standpoint, than huge ones. Large-scale ecosystems have the likely prospect of involving many more interests, jurisdictions, authorities and so forth, most all of which one will have to engage and overcome to be successful. From a political and bureaucratic, risk-averse point of view this means zeroing in on the smallest possible scale so to minimize, first of all, immediate termination problems and, over time, coordination headaches.

Scalar effects could thus favor timidity in system boundary definition. To clean up and manage the Columbia River Basin or the 'Owl Forests' of the Pacific Northwest mean having a perspective and authority for the entire ecosystem – daunting as it may seem. Piecemeal efforts do not add up, except from

a risk averse and bureaucratic point of view. Whatever the specific outcome and consequences, scalar effects in ecosystem definition will likely be important. The point is that the logical natural boundary definitions may reduce chances of managerial success from a bureaucratic or political standpoint.

National rules, regulations, and guidelines usually mean that local contextual details get short shrift. Ideal, uniform, equitable standards and expectations for implementation all over the country seldom produce workable or effective outcomes when applied to the messy realities in a specific site. What's right in Pittsburgh may be inappropriate in Peoria. What worked last year in the Columbia River Basin may be ineffective this year or next, depending on thousands of poorly understood or controlled ecological changes in that specific setting.

We are entering a new era in environmental management in which myriad interacting factors must be taken into account, monitored, and integrated into rich portraits representing ecological health as well as economic progress and political standing. The challenges are to define and measure the ecological concepts and then draw them together with the existing economic and political ones we already know quite well (Bormann and Kellert, 1994).

To be more concrete here, consider the Columbia River Basin, which is a highly stressed, multiple-use system, subjected to abuse for years. Several species of salmon inhabiting it are endangered, and upwards of 15 to 20 more are being sponsored for the endangered species list (Lee, 1989; National Research Council, 1996). This particular ecosystem presents many more and far different challenges, with fewer evident avenues to success, than managing a wilderness tract for endangered species or other uses. The context makes a huge difference in other words. Among other differences is the degree of uncertainty and risk involved. In highly stressed settings, any action is a gamble, whose consequences must be monitored carefully to minimize losses while learning more about circumstantial details that matter. The movement toward adaptive management in the Columbia River Basin and in other distressed Pacific Northwest ecosystems follows specifically from these realistic points (Lee, 1993; Brewer, 1997).

In short, one should expect variance from setting to setting and within a given context over time. One should also expect discrepancies from expected averages or norms, such as those contained in national statutes and in the rules and regulations established to carry them out. Discrepancies will likely be more pronounced in high-value ecosystems and in those already suffering from excessive human encroachment or other abuses. Risk increases in these circumstances as well. Decision making becomes more experimental than predictable, with surprises and failures occurring routinely (Holling, 1973). National norms tolerate none of these things very well.

These observations call attention to the overarching importance of situation, setting or context, especially with respect to wholesale changes ecosystem management and the research supporting it demand. A healthy respect for a variety of institutional means to deal with ecosystem-based problems is required, too. The next generation is not likely to be dominated by government solutions

versus business transgressions. Rather an array of collaborations and experiments, varying according to changing contextual details and demands, will become common.

What should be done?

Major change is necessary to improve ecosystem management and research in the Pacific Northwest. Ample evidence leads to the conclusion that no one is either in charge or accountable. No one even knows or can provide a basic enumeration of the participants, roles, and resources currently expended on behalf of ecosystem management and research.

Quick fixes aren't enough

Tentative or one-time efforts, such as the Interior Columbia River Basin Project or the Regional Ecosystem Office, even though well meant are virtually doomed to come up short or fail for the following reasons:

- They are one-time affairs when the problem calls for continuous, long-term treatment.
- The scale or scope of their activities does not encompass all of the Pacific Northwest. Piecemeal efforts do not add up, except from a risk averse and bureaucratic point of view.
- The role and purpose of research – 'knowledge' – are not specified and aligned with management needs.
- Urgently needed authority to allocate resources and to enforce quality standards is not vested in these bodies.
- Lacking permanence and authority, such efforts will always remain quite literally incredible.

Pacific Northwest prototype

From a simple management standpoint and in the interest of basic accountability we recommend creation of a single organization to manage and coordinate ecosystem management research in the Pacific Northwest. We suggest a step-by-step approach, doing the simpler and obvious things first to confront the most conspicuous deficiencies while gaining knowledge about and confidence in the overall strategic course for change we recommend: The Board for Ecosystem Management Research. The Board requires an explicit mandate to coordinate and manage all ecosystem management research, initially for the Pacific Northwest Region but eventually for the entire nation. The Pacific Northwest serves as the initial prototype and demonstration from which lessons learned will be applied and extended nationwide.

First steps to be taken are in the Pacific Northwest and include a full enumeration of people, places and projects – a stock taking – of current *agencies*, management goals, and research activities in the region. This information collection activity would result in better synthesis and in time to the identification of duplications, overlaps, and needs for the region. This operating *clearinghouse* could help set clear and consistent standards for environmental measurements of baselines and trends to improve monitoring and assessment. Standards of excellence for ecosystem management research could also be identified and recommended for practice.

A next step is to constitute an *independent science oversight group* to work through the Board. It must be appointed independently of the existing management structures of the agencies and should report periodically to Congress, the President, and the public on the quality and effectiveness of ecosystem management research in the region. Lessons learned in the Pacific Northwest, especially successful ones, eventually would be promoted by this group in other regions and nationally as well.

At this point a 'triangulation' between the management agencies, the clearinghouse, and the science oversight group will be established for the Pacific Northwest. These are necessary, but insufficient, first steps toward improvement. The long-term, next steps are very large ones, but warranted in our opinion given the stakes involved and the deficiencies evidenced by the conventional means.

The board for ecosystem management research

Incorporate under an independent Board of Directors the three key elements of agency research managers, scientists providing oversight, and specialists in information collection and management into the Board for Ecosystem Management Research. Initial geographic responsibility will center on the Pacific Northwest region, although extension to other regions and to the nation as a whole is the ultimate goal.

The Board of Directors, including representatives of federal, private, and nonprofit organizations, sets policy, funding, and broad technical and scientific guidelines for BEMR. The Board also hires and assesses the Chief Executive Officer and the Project Management Team (Figure 1).

The Project Management Team operates as a profit center. It would collect research, development and outreach funds from agencies and cooperators having ecosystem management responsibilities and would be responsible and accountable for their allocation to research organizations and for all results. The Management Team would also synthesize all ecosystem management research it supports, making the results available to funding sources, the scientific community, the Congress, and the public. Performance incentives, established and administered by the Board of Directors, will guide all activities of the CEO and the Project Management Team.

Science managers in agencies and collaborating institutions ('Agencies and

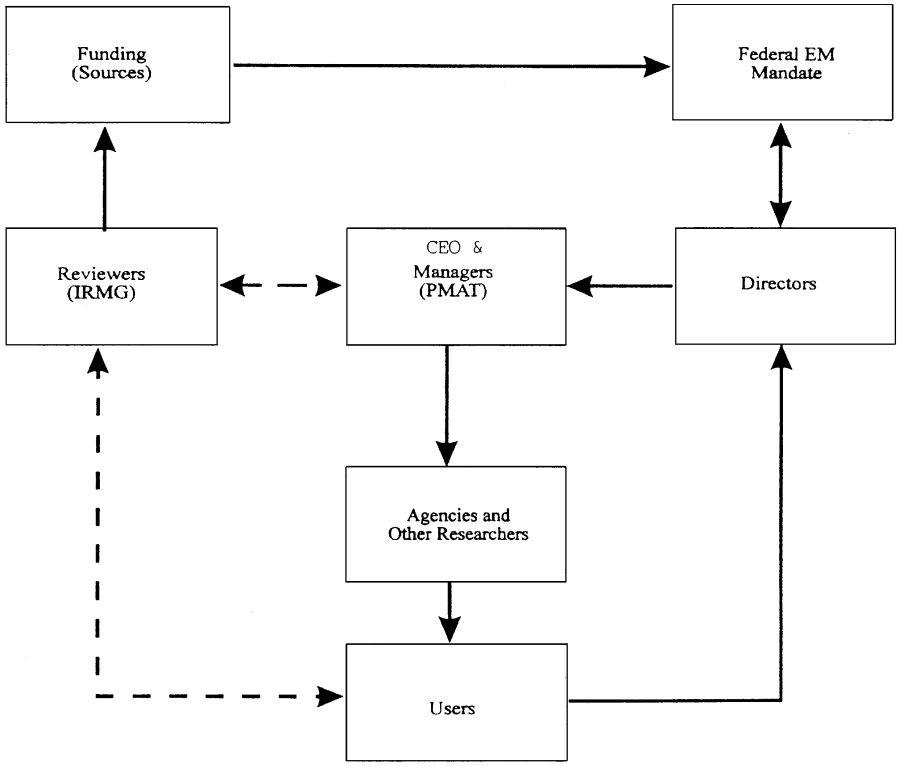

Fig. 1. Board for ecosystem management research.

Other Researchers,' Figure 1), are responsible to the Project Management Team through assessments for specific studies and projects. All forms of research related to ecosystem management will be supported: Specific studies and projects, adaptive management, applied research, assessments, and outreach. Science managers and their products will be reviewed systematically by the Board's science oversight group. They will participate in the incentive system, too. Research would be carried out by the existing federal laboratories and other qualified institutions, but subject to independent outside scientific review and the incentive system provided by the BEMR. This recommendation does not envision or require the addition of new research organizations or facilities. Instead, we seek to make the existing ones more responsive to ecosystem management needs and more responsible to the public.

We need to be clear about the roles and functions the Board for Ecosystem Management Research should perform since they depart so far from conventional business as usual in the governmental research realm.

The organization we envision is a means to achieve less government and more accountability. It would be a partly autonomous, non-governmental authority having its own independent board of directors, professional staff, and independent outside scientific advisors. Membership on the board might be patterned after other scientific oversight bodies or commissions where open positions would be matched to scientific and managerial needs. Potential members would be matched to each slot and nominated to the President by the National Academy of Sciences or a comparable, independent scientific, authority. Subsequent appointment to the board by the President for fixed terms at compensation levels adequate to attract high calibre talent would then follow. Establishing and maintaining scientific credibility, managerial excellence, and independence are highest order priorities.

The Board would function to collect, integrate and keep account of all available research funds – tasks which are simply not possible in the current circumstances. It would also help to organize and set priorities for research expenditures, tasks made possible by its control of federal funds designated for ecosystems research and management, some proportion of the total which would be made available for reallocation on a competitive, peer-reviewed basis – again, something which does not occur nearly enough in current circumstances.

Some caution that consolidation of these tasks will stifle debate and creativity. We believe the opposite to be true. This proposal should enhance research innovation at the same time as it promotes both organizational efficiency and overall effectiveness. In any event, trying out the idea as a prototype would generate the evidence needed to settle the issue.

In short, we strongly recommend a near-privatization of all existing federal ecosystem research funding in the form of the Board for Ecosystem Management Research. Nothing short of such a major change in the current system, in our professional view, stands a chance of breaking the hammerlock held on the system by the numerous agencies and their constituents who now dominate.

References

Ascher, W. and R. Healy (1990). *Natural Resource Policymaking in Developing Countries.* Durham, NC: Duke University Press.

Ascher, W. and R. Healy (1995). 'Knowledge in the policy process,' *Policy Sciences* 28 (February): 1–19.

Behan, R.W. (1990). 'The RPA/NEFMA: Solution to a nonexistent problem,' *Journal of Forestry* (May): 20–25.

Bormann, F. H. and S. R. Kellert, eds. (1994). *Ecology, Economics, and Ethics: The Broken Circle.* New Haven: Yale University Press.

Bormann, F. H. and G. E. Likens (1979). *Pattern and Process in a Forested Ecosystem.* New York: Springer-Verlag.

Brewer, G. D. and P. de Leon (1989). *The Foundations of Policy Analysis.* Homewood, IL: Dorsey Press.

Brewer, G. D. (1997). 'Science to serve the common good,' *Environment* 39 (6): 25–28.

Calow, P. (1995). 'Ecosystem health: A critical analysis of concepts,' in D. J. Rapport, C. L. Gaudet, and P. Calow, eds., *Evaluating and Monitoring the Health of Large-Scale Ecosystems.* New York: Springer-Verlag, pp. 33–41.

Carroll, L. (1892). *Alice's Adventures in Wonderland.* New York: Macmillan.

DeBonis, J.(1991). 'The forest service inside out,' *Wildlife Conservation* 10 (May/June): 88–89.

Dombeck, M. P. (1995). *From Commodity to Community: A Common Sense Approach to Understanding Ecosystem Management.* State College, PA: Pennsylvania State University, College of Agricultural Resources.

Espy, M. and B. Babbitt (1994). *Record of Decision for Amendments to Forest Service and Bureau of Land Management Planning Documents within the Range of the Northern Spotted Owl.* Washington, DC: U.S. Government Printing Office.

Forest Ecosystem Management Team (FEMAT: 1993). *Forest Ecosystem Management: An Ecological, Economic, and Social Assessment.* Washington, DC: U.S. Department of Agriculture/U.S. Department of Interior.

Gordon, J. C. (1994). 'From vision to policy,' *Journal of Forestry* 92 (7): 16–19.

Grumbine, R. E. (1994). 'What is ecosystem management?' *Conservation Biology* 8 (1): 27–38.

Holling, C. S. (1973). 'Resilience and stability of ecological systems,' *Annual Review of Ecology and Systematics* 4: 1–23.

Holling, C. S. ed. (1978). *Adaptive Environmental Assessment and Management.* New York: John Wiley & Sons.

Hrebiniak, L. G. (1978). *Complex Organizations.* New York: West Publishing.

Knickerbocker, B. (1991). 'Wading in the Wetlands Morass,' *The Christian Science Monitor* (December 6): 13.

Lee, K. N. (1989). 'The Columbia River Basin: Experimenting with sustainability,' *Environment* 31 (6): 6–11, 30–33.

Lee, K. N. (1993). *Compass and Gyroscope: Integrating Science and Politics for the Environment.* Washington, DC: Island Press.

Lele, S. and R. B. Norgaard (1996). 'Sustainability and the scientist's burden,' *Conservation Biology* 10 (2): 354–365.

Ludwig, D., R. Hilborn, and C. Walters (1993). 'Uncertainty, resource exploitation, and conservation: Lessons from history,' *Science* 260: 17–36.

McQuillan, A. G. (1989). 'The problem with economics in forest planning,' *The Public Land Law Review* 10: 55–72.

Muckleston, K.W. (1990). 'Striking a balance in the Pacific Northwest,' *Environment* 32 (1): 10–15, 32–35.

National Research Council (NRC, 1986). *Ecological Knowledge for Environmental Problem Solving.* Washington, DC: National Academy Press.

National Research Council (NRC, 1992). *Science and the National Parks.* Washington, DC: National Academy Press.

National Research Council (NRC, 1996). *Upstream: Salmon and Society in the Pacific Northwest.* Washington, DC: National Academy Press.

Noss, R. F. (1983). 'A regional approach to maintain diversity,' *BioScience* 33: 700–706.

Odum, E. P. (1971). *Fundamentals of Ecology.* Philadelphia: W. B. Saunders, Co.

O'Neill, R.V. et al. (1986). *A Hierarchical Concept of Ecosystems.* Princeton, NJ: Princeton University Press.

Overbay, J. C. (1992). 'Ecosystem management,' Speech delivered to national workshop on taking an ecosystem approach to management in Salt Lake City, UT, April 27. Washington, DC: U.S. Forest Service.

Perrow, C. A. (1961). 'The analysis of goals in complex organizations,' *American Sociological Review* 26: 854–866.

Primm, S. A. and T.W. Clark (1996). 'The greater Yellowstone policy debate: What is the policy problem?' *Policy Sciences* 29 (May 1996): 137–166.

Sample, V. A. (1989). 'National Forest policy-making and program planning,' *Journal of Forestry* 87 (1): 17–25.

Snow, C. P. (1959). *The Two Cultures and the Scientific Revolution.* New York: Cambridge University Press.

Society of American Foresters (SAF, 1993). 'Sustaining long-term forest health and productivity,' *Journal of Forestry* 91 (7): 32–35.

Tansley, A. G. (1935). 'The use and abuse of vegetational concepts and terms,' *Ecology* 16: 284–307.

Thomas, J.W. (1994). 'Forest ecosystem management team: Objectives, processes and options,' *Journal of Forestry* 92 (4): 12–17, 19.

Thomas, J.W. and J. Baca (1994). *Interior Columbia Basin Ecosystem Management Project Charter.* Walla Walla, WA: USFS/BLM, January 21.

U.S. Department of Agriculture (USDA, 1994). *An Ecological Basis for Ecosystem Management.* Ft. Collins, CO: U.S. Forest Service, Rocky Mountain Forest and Range Experiment Station, RM-246.

U.S. Environmental Protection Agency (1994). *Integrated Ecosystem Protection Research Program: A Conceptual Plan.* Washington, DC: U.S. EPA.

U.S. National Science Foundation (1994). 'Proposal brochure NSF 94-2.' Arlington, VA: NSF.

Wilkinson, T. (1997). 'An upstream solution to river pollution,' *The Christian Science Monitor* (May 19): 4.