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RISING IMPORTANCE OF THE LANDSCAPE PERSPECTIVE: AN AREA OF COLLABORATION BETWEEN MANAGERS AND RESEARCHERS

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Abstract. One area where basic researchers and managers have collaborated is in increasing the landscape perspective within their respective fields. Research and land management strategies have shifted towards greater consideration of landscape factors, a shift born from controversy over forest management. We learned several principles from our observations of this movement: many theoretical studies later held tremendous management value; controversy led to greater interest in science; competing demands on forested lands highlighted the need for landscape considerations; and there are great needs for new information. Controversy can be a great catalyst for researchers and managers to work together, and concerted efforts have brought advances in landscape understanding. Notable successes include a proactive model for understanding landscape processes. We are still far from effective landscape management. Most of the change has been in our thinking, not our actions. Managers and researchers at the Savannah River Site and on other managed forestlands have a great opportunity to forge a new "radical center" where collaboration is recognized as the route to greater understanding and action. The substantial history of collaboration between groups on the Savannah River Site to meet commodity production goals, conservation objectives, and research needs across diverse landscapes suggests that such a "radical center" is attainable.

Key Words: avian research, controversy, landscape, natural resource management, Savannah River Site.

One actively changing arena where Savannah River Site (SRS) management and researchers have worked closely together is landscape ecology. The SRS has been the site of several innovative landscape studies (Liu et al. 1995, Dunning et al. 1995, Haddad 1997), and research administrators in the Savannah River Natural Resource Management and Research Institute (SRI) have explicitly encouraged researchers to adopt landscape perspectives in their work. The Biodiversity Program of the SRI has funded landscape-level avian study since the late 1980s, including both computer simulation (Liu 1993, Liu et al. 1994) and field studies (Dunning et al. 1995, Kilgo et al. 1997). The Biodiversity Program also has encouraged a strong experimental program in landscape ecology with other organisms (e.g., butterflies [Haddad 1997] and small mammals [Anderson and Danielson 1997]). Because the successful implementation of such programs requires involvement of both researchers and managers, it is worth considering the landscape perspective from management and research points of view to provide several frames of reference for sponsoring a successful collaboration. In this paper, we examine the importance of the landscape perspective in management and research, and how attitudes towards this perspective have changed. We review the general differences in the perspectives of managers and researchers that have influenced our approaches to the landscape, giving examples of successes, lessons learned, and formulas for success.

THE IMPORTANCE OF A LANDSCAPE PERSPECTIVE

Why is it even necessary to discuss the importance of the landscape? In many regards, the importance of the landscape has become a cliche in management and research policy. Usually, however, few data exist on which to base landscape-level management. As much as we like to think that land management has taken on a landscape approach, impacts of many private timber sales and at least some public sales are analyzed without long-term projections of future landscapes that could be anticipated under the landowners' harvest programs, not to mention the ignoring of the neighboring landowners' programs. This is extremely significant because 80% of the timber harvest comes from private land, and private land constitutes 72% of the U.S. commercial timber acreage (American Forest Council 1991). Fifty-seven percent of all commercial forest acreage is non-industrial private forest lands (American Forest Council 1991). There are landscape plans for some industrial, state, and federal lands, but lands under active timber management without landscape plans are a major portion of the forest land base. Even where adjacent land managers are attempting to implement landscape plans, there can be serious impediments to coordination caused by different policies and management goals (Cortner et al. 1996). Researchers are designing investigations and analyzing completed studies without knowledge of the broader area in which their study sites are located. In spite of the apparent importance given to landscape issues, very little experimental research is designed to test landscape problems (Marzluff and Salabanks 1998). So even with the increase in time and paper that have been devoted to promoting landscape perspectives, most actions in the real world still consider a relatively small area. Thus the spatial scales of landscape analysis and planning still need to be increased. Mostly it is just our thinking that has changed.

The landscape approach is important because it is needed to research conservation problems appropriately, monitor environmental health, and manage the land. It is necessary for assessments of biological diversity (Probst and Crow 1991) and for natural resource analysis (Crow 1991). The distribution of habitats across complex landscapes needs to be considered when studying and managing animals that use a variety of habitats during migration (elk, neotropical migrant birds, and amphibians), seasonal or daily movements (Hunter 1997), or dispersal among parts of a metapopulation (Hunter 1997). Marcot (1997) suggested that not only the distribution of old stands may be important in landscape management, but that old forest elements between forest reserves are important for hidden species playing key ecological roles. Fragmentation is a landscape problem that increases edge, decreases interior conditions, and reduces viability of habitat for some species by isolating patches (Whitcomb et al. 1981). This isolation may slow or prevent dispersal of young (Hunter 1997).

Not only are the physical attributes of the landscape important, there are also ecological processes that operate at the larger scale (Dunning et al. 1992). Many processes are linked across landscapes, including effects from keystone species that travel between patches, nutrient cycling, and natural disturbance patterns like flood and fire (Carroll and Meffe 1994). There can be hierarchical linkage of processes through different scales (Allen and Starr 1982, May 1994), and different properties can emerge at different scales (Crow and Gustafsen 1997). Population-level processes such as predation are affected by the arrangement of organisms and habitats (Roff 1974). Natural disturbance regimes must be preserved at the appropriate scale to preserve the associated dynamics (Swanson et al. 1997). In summary, an increasing number of studies have shown associations between the landscape and birds (Marzluff and Sallabanks 1998).

Mistakes have been made when long-term landscape perspectives were not employed. A major conservation initiative of the 1960s and 1970s was the limiting of clearcut sizes and the scattering of harvest units across the landscape. Now, in our presumably more enlightened state, we find that perhaps it was dangerous to have asked for the small clearcuts, because we got them. The dispersed patch system (or "cookie cutter" approach) in forest harvesting led to increased fragmentation and a larger, perpetually drivable road system that was necessary to enable the creation and maintenance of these small harvest units. As we investigate the negative impacts of fragmentation, we can now realize that the larger-scale, landscape impacts of dispersed harvest systems were not given the same level of consideration as the small-scale, local habitat impacts when the cookie cutter approach was first designed.

PROGRESS MADE

To illustrate how much the perspective on landscape/local scales has changed, consider the transformation of research and land management paradigms in recent decades. The descriptive approach of the early naturalists evolved into the early experimental (or pseudo-experimental) approach of the ecologist and wildlife researcher. This experimental approach often focused on the density of animals in different forest stands and presumably examined habitat selection and habitat quality (Van Horne 1983). Now the avian researcher often designs studies investigating natural history information such as productivity, survivorship, and foraging habitat, but (in ideal situations) gathers this data in an experimental approach involving longer time and larger areas than used by the early naturalists that focused on these same topics.

The land manager (forest manager) of an earlier era was primarily concerned with sustained yield of timber and boosting forest productivity for wood products. Within this management framework, questions of habitat availability for wildlife were answered by providing a range of stand ages up to the maximum sustained yield rotation age. Next, the federal manager was concerned with multiple use, and though charged with maintenance of all species, commodity outputs still seemed to receive the focus. Most recently, the manager needed scientifically defensible management plans that provided for not only multiple use, but biodiversity, recovery of species, and ecological restoration. Ecosystem management at the landscape level is increasingly recognized by managers as a key to developing these plans (Risser 1988).

By the late 1980's and early 1990's, we were starting to think about landscape problems like never before. The researchers' input took on new value. An urgency for more information stimulated more support for research from U.S. Forest Service (USFS), for example, leading to requests for significantly greater funding. With support from the White House and Congress, the research budget increased between 1990 and 1995 while the USFS was moving toward ecosystem management (J. Toliver, Research Budget Coordinator, U.S. Forest Service, pers. comm.). While Congress has decreased the overall research budget (in real terms) since 1995, the USFS has held money constant in threatened and endangered species and ecosystem research programs by shifting funds from other research programs (J. Toliver, pers. comm.). This need for unbiased information was also one of the primary impetuses for the creation of the National Biological Survey (now the Biological Research Division of the U.S. Geological Survey).

Our progress includes several land management success stories. The multi-species habitat conservation plan developed for the California Coastal Sage Scrub was a landmark because it was the first comprehensive, ecosystem-based conservation plan drafted under the U.S. Endangered Species Act (O'Connell and Johnson 1997). The plan covers 15,240 km^2 in five counties, and integrates the efforts of numerous political jurisdictions. More than 120,000 ha of large blocks of habitat are expected to be preserved in conservation reserves (O'Connell and Johnson 1997).

The Coastal Sage Scrub plan evolved from the inadequacy of focusing management on single species and small-scale habitat planning. In 1995, Riverside County, California, completed an arduous, seven-year effort to create a habitat conservation plan for the endangered Stephens' kangaroo rat (*Dipodomys stephensi*). In the interim, however, three other species (a shrimp, a frog, and a bird) found in the same region were listed under the Endangered Species Act. Rather than start all over with plans for each of these additional species, planning officials in the affected southern California counties developed a comprehensive plan (called a Natural Communities Conservation Plan, NCCP) to protect the endangered ecosystem (O'Connell and Johnson 1997). An integral part of the NCCP is the development of a GIS database of land attributes that crosses political and ownership boundaries. With this database, planners can assess the landscape context of specific blocks of remaining habitat, identify important linkages between blocks, and determine proximity to core areas of conservation value (Stine 1996). Together with comprehensive population viability analyses of two of the endangered species (Price and Kelly 1994, Akçakaya and Atwood 1997), the California Coastal Sage Scrub NCCP is a benchmark for interagency, cross-boundary conservation planning.

Another success story resulted from controversy over an endangered species listing. In the 1980s, the U.S. Forest Service was charged with failure to comply with its own regulations under the National Forest Management Act (Gordon and Lyons 1997) in its management for the Northern Spotted Owl (*Strix occidentalis occidentalis*). The conservation strategy and recovery plan that was developed for the Northern Spotted Owl (Thomas et al. 1990, U.S. Department of Interior 1992) synthesized existing knowledge to lay the groundwork for a vast regional landscape management approach, later broadened to a multi-species plan (FEMAT 1993). Acceptance of this plan was not immediate, in part because it was developed by scientists, and thus managers felt little ownership in the final plan (Johnson 1997). The initial Spotted Owl effort did succeed in bring landscape science more effectively into management planning, but it was not a fully collaborative effort.

Finally, the Savannah River Site (SRS) is a current example of collaboration in both landscape management and basic research. How this landscape perspective developed is worth considering. The land that became the SRS was originally an agricultural landscape, converted to a largely forested condition by an aggressive tree-planting program in the 1950s and 1960s (White and Gaines *this volume*). This transformation was largely complete by the 1970s, before the interest in landscape ecology formed. Thus, there was little research done to monitor population or ecosystem response to the widespread changes. By the early 1980s, however, the Department of Energy (DOE, which funds virtually all research and management on the SRS) and the USFS (which implements the management) required information on how land use across the SRS impacted wildlife populations, endangered species, ecosystem functions, and other phenomena covered by DOE's mission.

Through the Savannah River Ecology Laboratory (SREL) and the Savannah River Institute (SRI), DOE had funded enormous amounts of high-quality, basic ecological research. But land managers sometimes complained that too much of the research was not focused directly to their urgent questions (J. Dunning, pers. obs.). The development of the landscape perspective in the 1980s provided a meeting ground for these potential antagonists.

To understand fully the impacts of timber operations on wildlife species of management interest, the managers within SRI needed to know how their land use affected species such as the

Red-cockaded Woodpecker (*Picoides borealis*) and Bachman's Sparrow (*Aimophila aestivalis*). Timber management changed the distribution of forest age classes across the SRS annually, and since both of these species were relatively poor dispersers, landscape-level impacts of these changes in habitat distribution were possible, indeed likely.

At the same time, ecologists at SREL and the University of Georgia were searching for an appropriate study system for testing landscape ecology theory. SRI agreed to fund a research program in which field studies to identify landscape influences were initiated, and a simulation model of timber management across the landscape scale was developed (Liu et al. 1995, Dunning et al. *this volume*). The modeling provided advice to the timber managers regarding potential impacts of their program on wildlife. The models themselves were an innovative application of a new ecological tool, spatially explicit modeling (Pulliam et al. 1992).

Both university ecologists and management-related biologists contributed to this collaboration. Timber management databases proved to be an invaluable resource for constructing current and past landscape distributions of habitats. The current 5-year and 50-year management plans gave the modeling project long-range forecasts of landscape change that could be built into the simulations. The ecologists contributed basic natural-history studies of habitat selection, demography, and dispersal for parameterizing the model. In conducting basic landscape research, the ecologists produced results that suggested the potential impacts of long-range management strategies. These results yielded practical gains to the development of management planning (Liu et al. 1995). Based in part on the success of this collaboration, additional studies of landscape effects on other birds, mammals, lizards, and butterflies were funded by SRI's Biodiversity Program, and conducted by ecologists from the University of Georgia, SREL, SRI and other universities (for example, Anderson and Danielson 1997, Haddad 1997, Kilgo et al. 1997).

We are finding, however, that even where there has been a great deal of research and landscape collaboration, there are still outstanding issues. With its large size (approximately 77,000 ha), the SRS would appear to be large enough to be independent of surrounding influences. We know from ongoing landscape analyses, however, that the SRS differs from adjacent land in characteristics such as human land-use practices, forest cover and type, urbanization, habitat fragmentation, and influences of exotic or feral organisms. Kilgo et al. (*this volume*) demonstrate that these differences translate to identifiable differences in bird populations. We have found that bird species associated with human land-use practices outside the SRS are present in greater numbers on the periphery within the SRS than they are in the interior (J. Dunning, unpubl. data), for instance. Thus, even large management units need to consider the impacts of landscape factors both within and external to the management unit itself. Data for such consideration are rarely available.

LESSONS FROM THE PAST

The greatest movement towards landscape considerations was born from controversies over forest management impacts on several species. These controversies influenced a redirection of resources and thinking toward the landscape approach. Ecosystem management, therefore, emerged in response to legal and societal demands, not science (Gordon and Lyons 1997). Red-cockaded Woodpecker issues in the East and grizzly bear (*Ursus arctos horribilis*) and elk (*Cervus elaphus*) issues in the West, followed by the Northern Spotted Owl issue, focused attention on large-scale questions. These questions involved population viability, the influence of adjacent habitats, dispersal, temporal scales, area sensitivity, metapopulation considerations, and the role of natural and manmade disturbances. The result of this focus was the movement toward ecosystem management, with its explicit emphasis on large spatial and temporal scales (Grumbine 1994).

There are several lessons here. One is that the basic scientific work of MacArthur and Wilson (1967), Levins (1969), and others that seemed so theoretical to some, eventually held tremendous management value. Another lesson is that it took third party catalysts to move the fringe ideas to the center through actions such as petitions to list species, and appeals and lawsuits on management decisions such as recovery plans and forest plans. These catalytic efforts brought science into the spotlight as societal and legal pressures have caused environmental advocates and land managers alike to reach out to science for answers (Gordon and Lyons 1997).

This emphasis on science, in turn, leads to the next lesson: that there is probably never enough information available to develop a land management plan thoroughly. It rapidly became apparent how little information was available. Land managers, especially with the USFS, needed the ability to manage the land to meet legal mandates for biodiversity and threatened and endangered species and to answer the charges of their critics. They became almost eager for long-term landscape analyses as these were seen as tools to help avoid appeals and to keep the timber program going. Habitat-specific density, basic productivity, juvenile and adult mortality, need for multiple habitats in close proximity, natural population fluctuations, source and sink habitats, and use of corridors were all issues or parameters that were critical to landscape analyses, but for which there was a lack of field knowledge (Conroy et al. 1995, Dunning et al. 1995).

Another lesson from observing this influence of controversy is that when a concerted effort was focused on a problem, great progress was made in our understanding of landscape influences. In three of the four catalytic species mentioned, application of the Endangered Species Act and the resulting "threat" to timber outputs motivated the effort. The coordinated effort that went into some of the elk research and management, however, holds a special model for bird researchers because it did not evolve from the level of crisis that the other three did. Because we could find no comparable avian model, we present some details of an elk research program.

The Montana Cooperative Elk-Logging Study (Lyon et al. 1985) was formed in 1970 by Montana Department of Fish, Wildlife, and Parks, the University of Montana, and the USFS. The program was later joined by the Bureau of Land Management and Plum Creek Timber Co., Inc. The effort evolved from discussions about a proposed timber sale and its potential effects on elk. Managers and biologists became acutely aware that predictions of the effects were highly speculative. Given the public interest in elk, predictions on how management would impact elk would be needed again and again.

Two oversight committees were organized to guide a widespread, long-term study of landscape management for elk. A steering committee of agency administrators was led by a chair position rotating annually between the agencies. This committee met at least annually to review progress, determine direction, and provide support. A research committee of scientists, also with representation from each agency, was led by a permanent chair. This committee standardized terminology and methodology to maintain credibility and acceptance. It developed the research program, selected study areas, prepared study plans for each project, conducted the research, and prepared annual reports on accomplishments, plans, and budgets for proposed work. Plans and budgets were submitted to the steering committee for approval. Funds were primarily redirected within existing programs and no agency gave up control of its funds, except through separate cooperative agreements between agencies or outside contracting. Once project plans and budgets were approved, each agency funded and managed the research projects it had committed to perform on its lands.

The Elk-Logging Cooperative developed research in seven different geographic areas, with research at a site lasting as long as 12 years. The original agreement was for ten years of research, but was extended to 15 years (Lyon et al. 1985). Anthropogenic and landscape-scale factors were analyzed to determine their relationship with elk habitat selection. Factors included the amount of traffic on roads, the density of roads, amount and quality of cover, topographic factors, and logging activity (intensity, duration, extent). This research led to a very good basic understanding of landscape patterns and regional landscape differences that influenced elk distribution, movement, and how elk were displaced by human activity throughout the year and from year-to-year (Lyon et al. 1985). The number of vested cooperators leant credibility to the results.

The research committee was also charged with technical transfer in three areas: public awareness, land-management application, and scientific documentation. It was required that management recommendations be included in the annual report beginning in the third year, and that recommendations be phrased in a positive manner and be based on research from within each state. It is interesting to note that the research committee was initially reluctant to present its findings, because to the scientists, it seemed premature. The steering committee insisted, however, and annually thereafter, the participating management biologists formed the cumulative results into operating guidelines for regions of the state. Specific situations such as long migration routes were addressed with specific recommendations. The management recommendations then went through a workshop to test their readability with interagency personnel working in timber, range, wildlife, and engineering.

This coordinated effort laid the foundation for other landscape studies, such as elk vulnerability to hunting and grizzly bear displacement. Perhaps more importantly, it led directly and indirectly to the development of a variety of elk habitat effectiveness models to aid land management decisions (Lyon 1983, Leege 1984, Wisdom et al. 1986, Ager et al. 1991). These models can quantitatively assess impacts of cover removal and of miles of road open to public traffic, enabling a comparison of alternative management plans in a landscape setting. Thus, like the SRS research program, the collaboration of western managers and elk researchers led to great improvement in our understanding of organismal response to landscape change.

LOOKING AHEAD

As previously mentioned, controversy can be a catalytic force. On the other hand, Hank Fischer (pers. comm.) of Defenders of Wildlife, who was intimately involved with two extremely contentious issues (grizzly bear recovery and reintroduction of wolves into Yellowstone National Park), has begun talking recently of the "radical center". He coined the term because so many natural resource issues have become so polarized that it now seems radical to think in terms of the middle ground. It is in this middle ground that manager and scientist can come together as a team to forge a collaboration that can be recognized as the route to greater understanding and action. It is here that we also find a contrast in the way researchers and managers approach their work. There are a few inhibitions to overcome before we are fully functional in the radical center, however, and we will elaborate on these.

We mentioned earlier that science is in the spotlight. Actually, it might best be characterized as scientists having been dragged out of seclusion into public debate (Noon and Murphy 1994). It is especially true that the debate over the spotted owl brought scientists into the fray (Gordon and Lyons 1997). While most scientists hail this spotlight on science as a positive thing in general, many scientists are not comfortable with the spotlight when it involves them personally and directly as an expert (Viederman et al. 1994, Hagan 1995). The expert is called upon to give endorsements or direction where information is limited and expert opinion is needed (D. Arrington, U.S. Air Force, pers. comm.). It can be horrifying to the research ecologist to see the work of many scientists over many years in many different areas boiled down to one simple linear relationship. The scientist works in the realm of 95% or 99% probabilities from experiments, not 70% or 80% probabilities from some Delphi approach. Some scientists are more comfortable pursuing some eccentric interest and complaining about the obscurity and loneliness of research pursuits.

Managers ask hard questions, some of which have never been answered directly by science. The managers must make important decisions based on whatever science can be brought to bear. Making decisions and politicking are certainly not the realm of the average scientist. The scientist often falls into the school of thought where uncertainty is the rule and therefore conservative management provides the only prudent course, whereas there are questions that can only be answered by the "hard experience" of adaptive management (Bunnell 1989). Biologists may fail to appreciate that their ideas and values must compete with others (Kochert and Collopy 1998) and that many proposals will fail if the plans are not skillfully defended.

The manager, on the other hand, is sometimes uncomfortable with researchers and is under pressure from the public. There is often a difference in educational background between managers and researchers (Hejl and Granillo 1998) and scientists are sometimes considered condescending towards managers (Hejl and Granillo 1998). The manager often suffers from stereotyping wherein he is viewed as more of a bureaucrat than a proponent of applied science (Hejl and Granillo 1998). The manager has production goals and deadlines to meet. But in sharp contrast to the researcher, the manager also has to satisfy a large, diverse, impatient public at the same time. Managers are, therefore, more interested in an immediate model of management options generated by a consensus of "experts" than in a proposal for a 10-year project to look at productivity of juvenile birds in different habitat types. What the manager may not appreciate is that models are fed with basic information from basic research. The obscure, glamourless work of the naturalist fuels these data-hungry endeavors. By now, many managers do realize that the sum of competing demands on the land requires a landscape perspective, especially where these demands exceed the land's capacity to produce, forcing compromise and optimization. The manager, hampered by the lack of integrating techniques for wildlife and forestry (Bunnell 1989), is looking for scientific, defensible methods to guide a balanced approach. So managers are increasingly turning to the scientist for answers and defense as never before. It is probably safe to say that "science-on-demand" will be sought heavily in the future (Gordon and Lyons 1997). What more could the researcher ask for?

There is strong potential for a team effort in which researchers can become indispensable to managers because their work can be seen as integral to the operation of the management area. Research is not a luxury that cannot be afforded, it is part of the adaptive management concept (Walters 1986). Managers, in turn, can be seen as indispensable sources of resources, local knowledge and expertise (not just funding) to researchers. Because the researchers on a team are often held more accountable for producing "useful" information and models, at least a substantial part of a broad research program is likely to be in tune with management needs. With the right mix, this mutualism becomes synergistic and more is achievable than ever imagined by the under-appreciated researcher and the besieged manager.

PRESCRIPTION FOR SUCCESS

Considering the lessons, the elk model, and benefits of team collaboration that we have presented, what is a reasonable prescription for successfully implementing a landscape approach for avian research and management that is synergistic? We believe that we must organize regionally, communicate effectively, participate in each other's jobs, and provide incentives to work together.

We must organize on a regional basis (not to be confused with agency regional areas) with steering and research committees, as did the elk-logging studies and as Partners-in-Flight has done to identify research needs and collaboration possibilities (Arnett and Sallabanks 1998). We need to outline specific research/management needs at all management levels (Arnett and Sallabanks 1998) with questionnaires and workshops. Arnett and Sallabanks (1998) have suggested, for example, that in general terms, our research needs to identify causes of avian population change, metapopulation processes, species at risk and the causes of their risk, natural variation, and species-specific habitat requirements. Research should be designed around adaptive management principles (Walters 1986) at a variety of spatial scales and longer time frames to better identify causal, rather than correlative, relationships (Marzluff and Sallabanks 1998). Research programs should preserve autonomous budgets and ownership in local projects, yet mutually decide upon common methodologies. We also need to focus on development of monitoring methods patterned after Hutto's (1998) suggestions so we may track our successes and failures and flag species in decline.

Researchers have not communicated their findings in an effective or timely manner outside of academic journals, leading to very poor accessibility of information (Hejl and Granillo1998). Often information is only published in journals that managers never see. We strongly suggest publishing in U.S. government agency technical publications because they are relied upon fairly heavily by a broad spectrum of managers and scientists. When possible, these publications need to be responsive to the managers' preference for information in a "cookbook" format rather than in-depth reports (Arnett and Sallabanks 1998). For the format of information to be most useful, researchers should give managers implementable tools (models, management scenarios, and species and habitat priorities for management; Hejl and Granillo 1998, Kochert and Collopy 1998) that can be used both in planning and cited as "products" for management support. While providing management scenarios, the researcher should show both the advantages and disadvantages for the management alternatives (Faaborg et al. 1998). All of these reports should undergo intensive peer review to maintain credibility and readability (Johnson 1997), and we should ensure that authors are given credit for peer-reviewed publication. Academic tenure and promotion policies must be modified to make these publications worthwhile efforts for non-government scientists.

Information needs to be timely. We should encourage publication of annual briefs and management recommendations beginning after two or three years of data collection. In addition to publications, workshops for managers and press releases for the public should be widespread as soon as results are suggestive of management action.

Researchers and managers should participate in each other's jobs. Managers should help formulate research goals and design research. Specifically, managers have skills in planning manpower and logistical support, and can provide suggestions for adaptive management strategies. Researchers should participate with managers by developing project alternatives (Hejl and Granillo 1998), by participating in background landscape analyses for decision documents, and by being involved in public hearings and briefings of congressional and state officials (Kochert and Collopy 1998). This involvement would allow scientists to witness information and modeling needs. Researchers need to understand the social, political, and economic factors involved in land management (Hejl and Granillo 1998). Scientists also need to understand that the best hope for avoiding land management driven by special interest groups is through the participation of knowledgeable individuals (Ganey and Dargan 1998). Mutual involvement by managers and researchers should close the large management/research gap identified by Finch and Patton-Mallory (1993).

Finally, we should provide incentives to work together, such as awards for collaborative efforts and information transfer (Hejl and Granillo 1998). Universities have initiated annual awards for research teamwork as a way of encouraging interdisciplinary work. We should consider awards for research/management teamwork, to be presented at the annual meetings of professional societies (both ornithological and management). Perhaps these awards can be jointly sponsored by research societies, management agencies, and professional societies. Award recipients should receive either a personal cash award or an extra budgetary award, such as expense money for extra travel and publication costs for dissemination of results to help encourage collaboration.

CONCLUSION

We are making progress. We have some relatively new-found principles that we can apply to help us meet wildlife management goals for the landscape. For example, we have simulation models that project impacts of timber harvest patterns on population viability (Franklin and Forman 1987, Li et al. 1993; Liu et al. 1994, 1995) that help us understand how we might improve on the cookie cutter pattern by approaching timber management differently. However, we need to expand our knowledge of how these theoretical ideas hold up in real-world situations. The October 1997 issue of the journal *Ecology* includes a special feature on positive interactions in ecological communities (Kareiva and Bertness 1997). These seven papers detail a new appreciation of the role that facilitation and mutualism play in structuring natural communities. This facilitation is a good role model for conducting ecological research. Facilitation between managers and researchers can yield a more organized approach to topics such as landscape influences than is possible by independent (and potentially competitive) approaches.

Landscape research and management on the SRS is a good example of the potential of facilitation. A basic research program on landscape influences has been supported by both research-oriented laboratories (SREL) and management agencies (SRI). Landscape considerations have been incorporated into management plans (e.g., U.S. Forest Service 1992) that have major impacts on commodity production, while basic ecologists have been called upon to help managers design conservation reserves across the SRS (e.g., set-asides; White and Gaines *this volume*). The degree to which the landscape perspective has been incorporated to date shows the potential for collaboration between researchers and managers and between groups at different ends of the "applied" versus "basic" science gradient.

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