Ecological Research and Educational Programs to Support Protected Area Management: Lessons From the United States Experience

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Abstract—Ecological research is needed to provide a foundation of knowledge for appropriate management of protected areas. Basic ecological research on the phenomena that exist in protected areas is important, as is applied research that will contribute to protection of these resources. Research on animals, plants, soil, ecological processes and their interactions, as well as threats to these entities are all needed. Using research on recreation ecology as an example, this paper describes how a foundation of knowledge was built and suggests important lessons that can be applied to development of protected area research and educational programs. In particular, it seems important (1) for researchers to be protected area researchers first and disciplinary specialists second, (2) for research to be cumulative and long term, (3) for research to be conducted and integrated across varied scales, and (4) to engage and integrate a broad range of disciplinary specialties.

Protected areas are designated because they provide a wide array of benefits to society, including the benefit of preserving nature for its own sake. The goal of protected area management should be to maximize these benefits. This requires understanding the resources within protected areas, increasing societal benefits that accrue from these resources, and protecting these resources from threats. Research, education, and management are all necessary. The purpose of this paper is to discuss how ecological research can best contribute to the education of protected area managers and the improved management of protected areas, drawing particularly from the perspective of experience gained in the United States. I will begin broadly, describing the breadth of research that seems worthwhile, offering remarks about where progress has been substantial and where it has not. Then I will use examples from my personal experience, conducting recreation ecology research, to make more specific observations and recommendations regarding development of the knowledge base for high quality protected area educational and management programs.

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In: Watson, Alan; Sproull, Janet, comps. 2003. Science and stewardship to protect and sustain wilderness values: Seventh World Wilderness Congress symposium; 2001 November 2–8; Port Elizabeth, South Africa. Proc. RMRS-P-27. Ogden, UT: U.S. Department of Agriculture, Forest Service, Rocky Mountain Research Station.

Basic Ecological Research in Protected Areas

Traditionally, most of the research conducted in protected areas has involved basic studies of the biological phenomena found in protected areas. In most of the National Parks in the United States, for example, there have been studies of individual animal species and of plant communities. At Yellowstone National Park, individual scientists have spent much of their career studying grizzly bear populations, bison, and vegetation patterns. This type of research is probably most common because it is most consistent with the disciplinary organization and orientation of academia. Most students are trained within departments that emphasize basic rather than applied research and single rather than multidisciplinary studies.

This research has contributed significantly to the education of protected area managers, the management of protected areas, and the benefits that accrue to society. It should be encouraged and continued, as there is much more that needs to be learned about ecological phenomena in protected areas. Beyond being receptive to such research, however, there may be little need for protected area management programs to promote such research. Scientists working within traditional biological disciplines will continue to work in protected areas, when they are the best places to study certain phenomena. However, there may be more need to promote basic research that (1) is of a long-term nature, (2) is interdisciplinary in scope, and (3) is designed to make use of wilderness as a baseline or reference, for comparison with more manipulated and disturbed landscapes. All of these types of research are critical to realizing the values and benefits of protected areas, but none are common. Existing institutional structures and funding mechanisms tend to present formidable barriers to research of this type.

Threats-Based Ecological Research in Wilderness

Recently, substantial ecological research has focused on threats to the integrity of ecosystems in protected areas, direct and indirect effects of human activities both internal and external to protected areas. This research is critical to developing the knowledge base needed to truly protect lands designated for protection. Research is needed to understand a wide variety of threats to diverse protected area resources.

Significant threats to wilderness lands in the United States, for example, include recreation use, fire management, livestock grazing, introduction and invasion of alien species, diversion and impoundment of water, emission of atmospheric pollutants, and management of adjacent lands (Cole and Landres 1996).

Research conducted by specialists from a wide variety of disciplines needs to be applied to the development of this knowledge base. Moreover, as is the case with basic research, there is particular need to design long-term studies that utilize interdisciplinary approaches. In the rest of this paper, I will discuss how research related to one of these threats—recreation use—has developed, particularly in the United States. Based on this perspective, I will offer some recommendations regarding how research can best contribute to the effective education of protected area managers.

Development of Knowledge in Recreation Ecology

The field of recreation ecology is primarily concerned with assessing the responses of ecological systems to recreational use and disturbance, usually for the purpose of understanding how to manage recreation within protected areas. Although recreation ecology research can be traced back at least to the 1920s (for example, Meinecke 1928), it was in the 1960s and 1970s that interest in recreation ecology intensified. Management agencies—such as the Forest Service and National Park Service in the United States—recognized that increasing recreation use threatened the integrity of both their facilities, such as campgrounds, and the biophysical resources of the areas they were to protect. Increasingly, they funded research projects that described recreation impacts and suggested means of mitigating those impacts.

Initially, studies were short-term, descriptive case studies that provided a wealth of site- specific insight for the funding agency but that did little to develop general knowledge. In the mid-1970s, the first syntheses of recreation ecology were compiled (for example, Liddle 1975; Speight 1973; Wall and Wright 1977). Not surprisingly, these syntheses were largely confined to description of the impacts of different types of recreation on different impact parameters. There was little rigorous documentation of how impacts were changing over time, and little understanding of how impacts relate to factors that influence the magnitude of impacts. Consequently, little of this research was capable of developing general principles and strategies to guide management.

This began to change in the late 1970s and 1980s with the development of long-term, cumulative recreation ecology research programs, first in the Forest Service and then in the National Park Service. With these programs it was possible to develop long-term studies to identify trends over time, as well as to replicate studies in different places to assess the generalizability of results. Multiple methodologies were applied, providing richer insights than those provided by single methodologies. For example, in their study of campsites at Delaware Water Gap National Recreation Area, Marion and Cole (1996) assessed changes over a 5-year period on long-established campsites, recently opened campsites, and recently closed campsites, as well as on plots subjected to experimental trampling.

By the late 1980s, general principles of recreation ecology were emerging. In a synthesis paper, for example, I (Cole 1987) postulated that the magnitude of impact was largely determined by amount of use, type of use, season of use, and environmental conditions, and provided empirical examples of the effects of each of these influential variables. I developed simplified models, in which it was possible to manipulate variables one at a time, to show the general influence of these variables on amount of impact (Cole 1992). Subsequently, Kuss (1986), Leung and Marion (1996), and Liddle (1991) have summarized information about how impact varies with environmental conditions.

Equally significant has been work that relates empirical research findings to general management strategies. The factors that influence magnitude of impact can each be modified by management so that impact is minimized. A handbook that summarizes the likely effectiveness of alternative management strategies, based on empirical research has been developed (Cole and others 1987). In their recent synthesis of recreation ecology, Leung and Marion (2000) organized much research around these management strategies. The content of low-impact (Leave No Trace) educational messages have also been based on the results of empirical studies (Cole 1989). Experience with descriptive studies of impacts has been used to develop monitoring methods, most notably in the work of Marion (1991). Finally, recreation ecology has provided a foundation for developing indicators and standards for contemporary recreation planning frameworks, such as Limits of Acceptable Change (Stankey and others 1985).

Once a discipline matures to the point where general principles emerge and management applications of those principles have been explored, education becomes much easier and more effective, and transfer of information to managers is facilitated. Starting in the late 1980s, now that principles derived from recreation ecology research had been postulated and interpreted within management contexts, more generally useful recreation ecology textbooks were written (Hammitt and Cole 1987; Hendee and others 1990; Liddle 1997). The availability of textbooks and also popular books on low impact education for the public (Hampton and Cole 1995) further increase the effectiveness of education.

Important Findings of Recreation Ecology

To illustrate the linkage between recreation ecology research and management of protected areas, six of the most important general principles derived from recreation ecology are described below:

1. Many of the impacts of recreation are positive feedback loops. For example, trampling eliminates vegetation cover, which in turn reduces inputs of organic matter into the soil and alters the microorganisms that live in the soil. Because soil organic matter and microorganisms are critical to the establishment and growth of vegetation, vegetation may be extremely slow to recolonize damaged recreation sites, even in the absence of further trampling. Managers must sever this positive feedback

loop by artificially amending soils with the organic matter and microorganisms that vegetation needs to establish and grow.

2. Impact is inevitable with repetitive use. Numerous studies have shown that even very low levels of repetitive use cause impact. Therefore, avoiding impact is not an option unless all recreation use is curtailed. This means that managers must decide on acceptable levels of impact and then implement actions capable of keeping use to these levels—the approach at the core of the Limits of Acceptable Change and related planning frameworks.

3. Impact occurs rapidly, while recovery occurs slowly. This principle underscores the importance of proactive management, since it is much easier to avoid impact than to restore impacted sites. It also suggests that relatively pristine places should receive substantial management attention, in contrast to the common situation of focusing most resources in heavily used and impacted places. Finally, it indicates that rest-rotation of sites (periodically closing damaged sites, to allow recovery, before reopening them to use) is seldom likely to be effective.

4. In many situations, impact increases more as a result of new places being disturbed than from the deterioration of places that have been disturbed for a long time. This principle also emphasizes the need to be attentive to relatively pristine places and to focus attention on the spatial distribution of use. It suggests that periodic re-inventories of all impacted sites are often more important than monitoring change on a sample of established sites.

5. Magnitude of impact is a function of frequency of use, type and behavior of use, season of use, environmental conditions, and spatial distribution of use. Therefore, the primary management tools involve manipulation of these factors.

6. The relationship between amount of use and amount of impact is curvilinear (asymptotic). Where use is light, even small differences in amount of use can have profound effects on amount of impact. Conversely, where use is heavy, even substantial differences in amount of use have little effect on amount of impact. This principle has numerous management implications and is also fundamental to many Leave No Trace educational messages. It suggests that it is best to concentrate use and impact in popular places and to disperse use and impact in relatively pristine places.

Recommendations Based on Lessons Learned

Over the past several decades, advances in the educational and management utility of recreation ecology research have been made. The progress that has been made is largely a result of (1) researchers framing their issues in terms of protected area management rather than more traditional issues of ecological research and (2) researchers being given the opportunity to conduct sustained research programs that are accumulative. Progress would have been even greater if (1) research spanned a broader array of scales and (2) more disciplinary specialists were involved. Therefore, my recommendations for a protected area research program in support of education and management revolve around the following four points:

1. Researchers should be protected area researchers first, with their allegiance to an academic discipline of secondary importance. What is critical is that the questions being addressed through research are framed within the context of protected area management rather than those of an academic discipline. For example, many of the biologists who first studied the relationship between amount of use and amount of impact, found that magnitude of impact was linearly related to the logarithm of amount of use (for example, Dale and Weaver 1974; Hylgaard and Liddle 1981). The primary goal of their research was to describe the relationship between use and impact. Trained to present their results in as succinct and elegant a manner as possible, they showed that their data fit a log-linear relationship—providing the most parsimonious description of the relationship—and they were done.

In contrast, I first studied the relationship between amount of use and amount of impact—not for its own sake, but to understand the relative effectiveness of concentrating versus dispersing use (Cole 1982). In my work, I have also consistently found that a log-linear relationship can be fitted to data relating use to impact. However, by describing the relationship as curvilinear or asymptotic, rather than log-linear, the management implications of the relationship are much more apparent. Research leads more directly to the development of principles and to management application.

A protected area researcher is also more likely to work in an interdisciplinary fashion and to seek opportunities to work collaboratively with management. Many protected area issues require integration of both social and ecological science. In addition, research can often contribute most to management if there is an iterative relationship between the two. Clearly, scientists within traditional academic disciplines can contribute to the knowledge base needed to manage protected areas. However, I think that progress would be greater if resources are allocated first to scientists whose self-identity is highly associated with protected areas.

2. Researchers should be given the opportunity to do accumulative work. Often research funding sources are such that scientists must jump around from one short-term, site-specific, low-budget project to another. Along with the tendency of many scientists to be opportunistic and to enjoy dabbling in many different areas, much research does not accumulate into a well-established body of knowledge that can be generalized into principles or applied to significant management issues. The limitations of earlier research cannot be overcome unless scientists have the opportunity to refine their techniques in subsequent projects. Ideas generated by earlier work are frequently lost when funding for additional work is not there. The projects undertaken tend to not be very ambitious or innovative.

For example, the opportunity I have been afforded to pursue a career in recreation ecology has allowed me to conduct long-term studies and to identify long-term trends in the conditions of protected areas. I have been able to work at multiple spatial scales and to use multiple methodologies as a means of triangulation. I have been able to replicate studies in different environments to assess the generality of earlier findings and to gain insight into how to adapt monitoring methods to different situations. I have been able to study all the various factors that influence amount of impact, including amount of use, type of use, and environmental conditions. All

of these opportunities increase the ability to move from research results to overarching principles and management application, making transfer of knowledge through education to managers much easier.

3. Research should be conducted at multiple scales. Most scientists have focused on mesoscale phenomena, those that are readily observed. They have tended to study individual animals and species, populations of plants and animals, and communities. As a result, we have generally done a good job of describing the impacts that occur at the human scale. However, lack of research at other scales hampers our ability to restore damaged sites or to gain perspective on the extent to which recreation impacts impair the integrity of large protected areas.

Relatively little work has been done at the microscale, looking at soil biota, for example, or at soil-plant interactions. Many protected area managers have closed damaged sites and are attempting to restore them. But the physical, chemical, and biotic properties of soils on these sites have been altered. Physical impacts—such as soil compaction—can be temporarily alleviated through scarification, but soils are prone to being compacted again in the absence of vegetation and desirable soil biota. Vegetation can be planted on damaged sites, but plant growth can be poor and mortality high if soils are compacted and lack desirable soil biota. Solving these problems is hampered by inadequate research at microscales.

Inadequate research at large spatial scales is equally problematic. Our current inability to develop general principles regarding recreation impacts on wildlife may largely result from this inadequacy. Hundreds of short-term, site- and species-specific studies of behavioral responses to recreational disturbance have been conducted. However, the lack of research at population or community scales makes it impossible to know how significant these impacts are. Similarly, the intense impacts on vegetation and soil caused by recreation at the scale of the site may be insignificant when viewed at larger scales.

4. Researchers, with tools from a wide array of disciplines, need to contribute to protected area research. Resources to be protected include air, water, animals, vegetation, soil, and rock, not to mention cultural resources and human experiences. These resources are threatened by a wide array of influences, of which recreation is just one. A much wider array of scientific expertise needs to be devoted to these issues than is currently the case. Within recreation, for example, the preponderance of researchers with botanical training means that our understanding of impacts on vegetation is much greater than our understanding of impacts on soils, animals, or water.

Conclusions

Effective management of protected areas is dependent on managers obtaining adequate training from educational institutions, with both education and management grounded in a solid research program. Both basic and applied research is needed. Protected area research in the United States has made some advances over the past few decades, but is still woefully inadequate for dealing with the array of issues that confront protected area managers. It has been most successful where (1) researchers are protected area researchers first and disciplinary specialists second, (2) research has been accumulative and long-term, (3) research has been conducted and integrated across varied scales, and (4) a broad range of disciplinary specialties have been involved and integrated.

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