

Observations from Outside

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(Based upon opening remarks made at the Second Biennial Conference of Research in California's National Parks, University of California, Davis, California, Sept. 5-7, 1984.)

As I look back over 15 years of association with National Park Service research I am astonished with the progress in NPS science programs. There are many indicators of the thriving state of NPS science. Quantity and quality of scientific staff are up. NPS scientists are participating in scientific meetings in greater numbers and publishing in refereed outlets. This participation and the better training is reflected in higher overall credibility for NPS science and staff among peers and lay public.

The quantity of science being conducted has increased and there are now Parks and research groups approaching critical scientific masses. Efforts at Sequoia-Kings Canyon, Channel Islands, and Redwoods in California reflect these concentrations as does earlier establishment of the center at Everglades.

The nature of NPS science is changing with greater attention to larger issues and to interdisciplinary approaches. Interactions with managers are typically good, despite occasional differences over priorities.

Many NPS scientists may be too close to appreciate the progress. NPS science has become an element to be reckoned with in the field of ecology, however, and I am telling scientific and managerial scientists so whenever I have an opportunity.

Yet, there are some aspects of NPS science that need to be strongly encouraged.

Relativities and Probabilities. There is increased attention to relativities in ecological science as opposed to absolutes. The null hypothesis approach to ecology has contributed to our progress but, unfortunately, it also has encouraged scientists to think in terms of either/or situations. Is the assertion that "competition is the process responsible for the structuring of communities" true or false? The answer is YES as it is for so many biological phenomena.

Many processes or structures are proving to be operative in ecosystems. It is their relative importance or even occurrence that varies drastically from ecosystem to ecosystem. This may seem very logical but you might not get that impression from the ecological literature where reputations are made defending the universality of a particular concept.

The increased appreciation of variation across ecosystems should result in more research comparing ecosystems in time and space. It should also result in greater consideration of stochastic elements, a recognition of probabilities, as when considering disturbances, such as fire, or regeneration behavior of two plant species. All of this effort would be directed to prediction of ecological responses.

Appreciation of Natural History. There is a resurgence of appreciation of the need for knowledge of the natural history of the ecosystems and organisms with which we are working. I have often been dismayed at the low esteem with which some scientific leaders have held such research; yet it has always seemed to me to be the essential core of any ecological research program. Some scientists have suggested that we avoid the need for such research through reliance on theory. Yet, the general models of the theorists provide us with little ability to predict ecological outcomes. As one noted ecological theorist commented several years ago, he relied on his knowledge of the natural history of involved organisms or ecosystems and not classroom theory whenever confronted

with a real-world ecological problem.

Of course, ecological theory can contribute to our concerns in applied ecology; it is a broad and varied field. The general constructs of theorists can stimulate us (e.g., the successional strategems of Eugene Odum) and there are ecological problems to which existing theory has application, such as in disease epidemiology or population biology.

Generally, however, we are going to have to know at least the general features of our ecosystems and constituent organisms. And we are going to have to construct models that use this information to provide probabilistic predictions. Fortunately, we see models emerging that do incorporate these features, such as the forest successional models JABOWA and FORET.

It is important that we recognize that technology cannot be substituted for an investment of time and dollars to obtain accurate and comprehensive natural historical knowledge. Sophisticated technology, whether computers, remote sensing or in some other form, can facilitate such work and we should use it to the fullest. But ecological diversity demands a thorough knowledge of individual park ecosystems and organisms for effective management, i.e., management which achieves its conservation objectives.

Holistic Viewpoints. Holistic viewpoints are increasingly emphasized in ecological research. This reflects increased appreciation that you cannot understand or manage species, processes or structures in isolation; the need is for understanding the relationships among the parts of a whole ecosystem. It also reflects the fact that the most interesting ecological questions (in my opinion) and the most relevant ecological questions (to society or park managers) involve whole ecosystems. We see this again and again, e.g., with regard to impacts of atmospheric pollutants, re-creation of an old-growth forest condition, or explanations of declines in a caribou herd.

The holistic perspective encourages collaborations between disciplines and between institutions. The breadth of knowledge required exceeds that of any single discipline let alone individual. Similarly, the breadth and depth of resources required often exceed the ability of a single institution or agency.

Long-term viewpoints are being encouraged after being in bad odor for many years. The National Science Foundation, reflecting a consensus in the ecological community, has recognized the need for long-term data sets and experiments and is providing approximately four million dollars a year to support it. Long-term studies are simply the only way in which many important questions in basic and applied science are going to be definitely resolved. Long-term data bases are necessary to identify trends, measure rates of long-term processes, provide baselines, develop an appreciation of and information on episodic phenomena, and provide the raw meat for formulating and testing hypotheses.

Unfortunately, the reward system for scientists has worked against a long-term orientation.

Implications for National Park Service Research

What do these trends have to do with future National Park Service research and, more broadly, future research in National Parks? Quite a bit.

We must begin by recognizing at the outset that the parks are unique as scientific properties. They are society's best baseline areas. They are the best examples of the structures, organisms, and processes found in natural ecosystems. To fulfill their potential function as baseline areas and to provide informa-

tion necessary to manage these properties, NPS science is going to have to work harder and NPS scientists are going to have to accept some special responsibilities.

There needs to be a greater emphasis on long-term research. This may take many forms — establishment of permanent plots, observation of ecological processes-organisms-structures over long time periods, establishment of exclosures and (yes!) of manipulative experiments. Past mistakes need to be avoided. Exclosures are a good example of an invaluable approach to some long-term studies but they have developed a bad name, largely because of inadequate documentation and pretreatment measurement. The new generation of long-term projects needs to have well-defined objectives, thorough documentation of procedures, adequate field marking, careful data archiving, and continuity in maintenance. NPS needs to develop an institutional commitment to such long-term efforts and an institutional memory to insure that such projects are not lost with loss of individual scientists or managers.

NPS science needs to identify the essential long-term programs and find the resources to carry them out. Persuade management; bootleg when you have to. This is your responsibility to the parks, and to the future generations of resource managers and scientists. Given the transient validity of much of the information we generate (ecological science is a science of successive approximations, not truths), the legacy in long-term installations and data may be your more permanent legacy.

There are NPS programs that provide excellent models. The acid rain baseline projects, for example, are one good start. Here is a program that is being used to serve both immediate and long-term objectives. Each of these projects is becoming the core of a major NPS long-term ecological research program. They are projects to which many other studies can be added in future years. A superior example of long-term monitoring is provided by the marine monitoring program being established at Channel Islands. It is biologically and statistically sound, well documented, and adequately marked in the field. I will guarantee that some very exciting science is going to come from that effort, in addition to the baseline data needed by management.

There needs to be greater emphasis on whole ecosystems. NPS scientific programs tend to focus on current problems, symptoms rather than causes, heroic species, conspicuous phenomena, and political concerns. These tendencies are always going to be present and for good reasons. The emphasis on research contracting and on yearly decisions for funding also tends to result in fragmented looks at ecosystems. It is important that NPS science and management staff collaborate in developing programs which provide a more comprehensive perspective.

NPS scientists have a special responsibility to take a holistic view of the ecosystems with which they are dealing. You need to provide leadership in planning and conducting interdisciplinary and interinstitutional collaborations. Such efforts are essential to resolution of so many critical resource issues and to documentation of baseline conditions. A part of this job is the responsibility for integrating the information that is developed for these ecosystems, all of the many bits and pieces. If you do not do the job of synthesis, who will?

NPS scientists need to become the recognized experts in their ecosystems. You need to be the leaders in conceptualizing and carrying out the projects. You need to be the authors of the definitive monographs.