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Importance and Justification of Long-Term Studies in Ecology

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Numerous, irrefutable examples of the central role that long-term studies play in ecology now exist (see, e.g., Likens, 1983; Strayer et al., 1986). There is increasing recognition that, because most important questions in ecology ultimately deal with predicting ecosystem responses, testing the correctness of ecological concepts and predictions by observing the future is essential. There are many sophisticated predictive models and general constructs, but few have actually been tested against data. In the final analysis, the most convincing validation comes only from such tests against reality.

We also have convincing evidence of the importance of long-term studies in identifying and resolving environmental issues from regional to global levels. A significant North American example was the use of data from the Hubbard Brook Experimental Forest in New Hampshire to demonstrate the occurrence of acid precipitation.

So, what additional statements are needed regarding the importance of long-term studies in ecological science? One problem area is suggested by the Institute of Ecosystem Studies' (IES) analysis of long-term ecological studies (Strayer et al., 1986): all successful projects encountered in their survey were the fortuitous combination of a leader (dedicated scientist), an opportunity (site and idea), and funding (often patched together from many sources). Given the central importance of long-term studies to ecological science and to the identification and resolution of societal issues, can we tolerate such a serendipitous approach?!

Current approaches to long-term ecological studies and a consideration of some alternatives are the major topics of this chapter. I begin with a brief review of the importance of long-term studies, emphasizing their significance to central ecological concepts. I then consider elements of a broader and more systematic approach to long-term ecological research and briefly review some existing long-term programs. I conclude with personal views on the ecological profession's responsibilities with regard to a systematic program of long-term studies. A premise of this paper is that short-term approaches cannot substitute completely for direct long-term observations of ecological phenomena.

G. E. Likens (ed.), *Long-Term Studies in Ecology*

Ecological Phenomena for Which Long-Term Studies Are Important

Classes of ecological phenomena for which long-term studies are recognized as essential include: slow processes, rare events or episodic phenomena, processes with high annual variability, subtle processes, and complex phenomena (Likens, 1983; Strayer et al., 1986).

Slow Processes

Many ecological processes take place over relatively long time periods (Fig. 1.1), typically much longer than the normal grant duration of three years. Succession, particularly in terrestrial ecosystems, is a prime example. There are numerous illustrations of the need for long-term observational and experimental studies on succession in forests (e.g., Christensen and Peet, 1984; Peet and Christensen, 1987), grasslands (e.g., Risser et al., 1981), and old fields (Tilman, 1988). Continuing studies of vegetation development at Glacier Bay (now extending over 60 years) are providing new insights and correctives for earlier predictions (Wood, 1984).

The population dynamics of long-lived organisms is another example of a slow process of major significance to ecologists. Included are the population cycles of most vertebrates and higher plants. The long-term study of wolf (*Canis lupus*) and moose (*Alces alces*) interactions at Isle Royale has provided dramatic evidence for the misleading nature of short-term results. A complex pattern of relationships has emerged after 29 years of study (Fig. 1.2), in which life-long survival of individual cohorts of moose appears to be the key in a cycle about 38 years long (Peterson et al., 1984; Peterson, 1987). A conclusion that wolf and

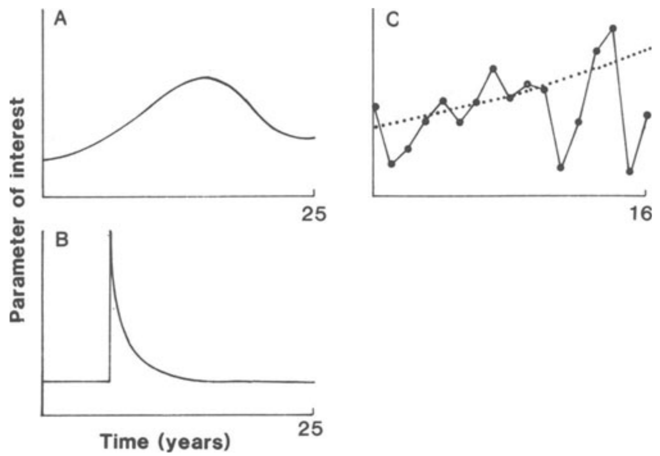

FIGURE 1.1. Examples of some ecological phenomena which require long-term study (from Strayer et al., 1986).

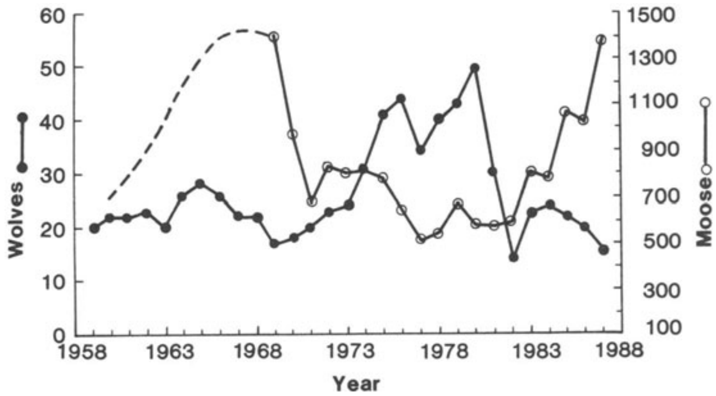

FIGURE 1.2. Wolf and moose populations on Isle Royale, Michigan (from Peterson, 1987).

moose populations were unrelated was one of several erroneous conclusions reached at earlier points in the study. Many smaller as well as larger vertebrates fall into the category of species requiring long-term studies. Gill et al.'s (1983) research on the red-spotted newt (*Notopthalmus viridescens*) illustrates that even a small animal may require an extended period of study; ten years was necessary to demonstrate that individuals require six years to mature and return to their original pond for breeding.

Population dynamics of vascular plants, especially trees, clearly requires long-term observations. For example, a 36-year study of a 450-year-old conifer forest in Washington State revealed little change in forest composition and structure; extinction of the Douglas-fir (*Pseudotsuga menziesii*), a shade-intolerant species lacking reproduction, was projected to require another 750 years at the current rates of mortality (Franklin and DeBell, 1988).

Additional exemplary slow ecological processes include soil development and wood decay. Soil formation is typically described in terms of decades or, more often, millenia (Buol et al., 1973). Rates are generally in the range of 20–100 years/cm. Decay rates for coarse woody debris (standing dead trees and boles on the forest floor) can be very low depending upon the tree species and environment (Harmon et al., 1986); half-times of up to 230 years have been described for such woody structures.

Rare Events or Episodic Phenomena

Any ecological phenomena with a return time of more than a few years falls into this category. The most important way to learn the frequency and ecological significance of such events is to observe them over long periods. Indeed, only in this way will some be discovered to occur!

Reproductive patterns for long-lived organisms are a common and important example of this class of ecological phenomena. Many species are highly episodic reproducers as a consequence of environmental variables, seed production, or disturbance patterns, particularly on habitats stressful or marginal for the species. Ponderosa pine (*Pinus ponderosa*) forests in northern Arizona provide a well-known example; between 1908 and 1945 pine reproduction was abundant in only one year, a combined consequence of a good seed crop and favorable moisture conditions for seedling establishment (Pearson, 1950).

Disturbances are another major class of episodic phenomena. Examples include wildfires, floods, intense or unseasonably low temperatures, droughts, outbreaks of pests and pathogens, windstorms, icestorms, and volcanic eruptions. Disturbances of moderate intensities, such as windthrow gaps in forests, may also be important episodic events. All such events are important because they reset ecosystems developmentally and cause mortality of long-lived and dominant organisms (see, e.g., papers in West et al., 1981). Disturbance events may receive substantial attention when they occur because they provide exotic and exciting opportunities for scientists (Lewin, 1986), but without long-term studies the context is lacking to interpret their importance in ecosystem development and life cycles of organisms. For example, the importance of pest outbreaks has been consistently overestimated and misunderstood because of this infatuation with unusual events (Wickman, 1980).

Processes with High Variability

Some ecological processes show very high levels of year-to-year variability. Many environmental parameters are of this type; the long-term record of ice-over at Lake Mendota, Wisconsin provides an excellent example (Brock, 1985) (Fig. 1.3). Obviously, biological phenomena strongly linked to such physical parameters are also likely to exhibit high annual variability. Productivity in desert ecosystems is an outstanding example, because in many deserts it is strongly related to the level of precipitation (MacMahon, 1980; MacMahon and Wagner, 1985). High annual variability may occur even in very moderate environments, as illustrated by litterfall in mature deciduous hardwood forests (Gosz et al., 1972) and in old-growth Douglas-fir forests in the Pacific Northwest (Art McKee, personal communication).

Subtle Processes and Complex Phenomena

Subtle processes and complex phenomena require long-term studies in order to separate pattern from "noise." Subtle processes are those that are changing over time but in which the year-to-year variance is large compared to the magnitude of the trend (Fig. 1.1). Examples include subtle changes in the acidity of rainfall (Likens, 1983) and changes in the level of nutrient losses of an aggrading forest ecosystem (Bormann and Likens, 1979).

Complex ecological phenomena involve many interacting factors. Long-term studies are important in such cases to sort out the relative contribution of multiple factors by obtaining a sufficient statistical sample. Several examples of multivariate analysis of a long-term record exist in determining factors important to population dynamics in aquatic ecosystems (e.g., Ricker, 1975; George and Harris,

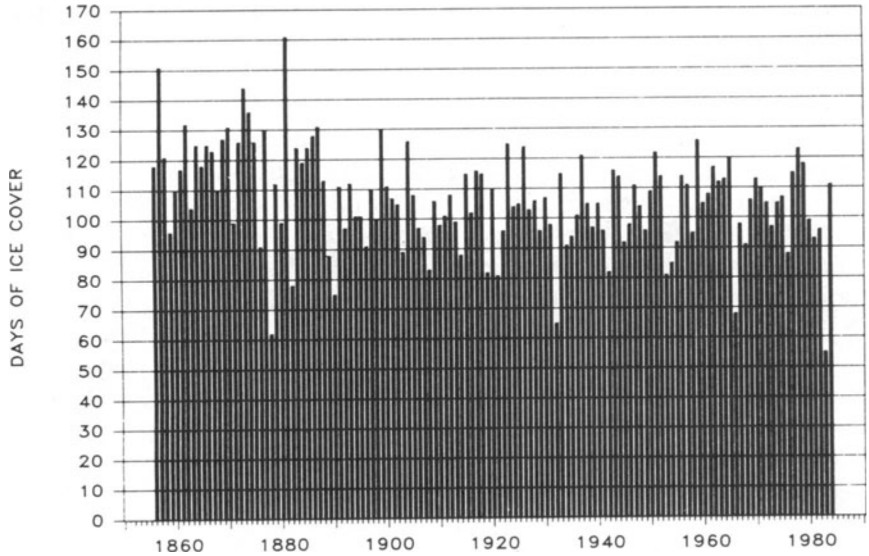

FIGURE 1.3. Annual variation in period of ice cover on Lake Mendota, Wisconsin (Brock, 1985).

1985), and similar use of long-term records in vegetation science has been suggested (Beeftink, 1979; Austin, 1981).

Formulating and Testing Ecological Theory

I think that the aforementioned categories are useful and that many convincing examples of the need for long-term studies now exist. These examples clearly illustrate that short-term studies are often misleading (see, e.g., Tilman, 1988; Likens et al., 1977; Wickman, 1980). It is also clear that without long-term data we lack a context for interpretation. Hence, the difficulty of interpreting phenomena such as the red spruce (*Picea rubens*) dieback (see, e.g., Scott et al., 1984; Vogelmann et al., 1985).

Unfortunately, ecologists often appear insensitive to the central role of long-term studies in formulating and testing basic ecological concepts. As Likens (1983) has observed, "Qualitative and quantitative observations over long periods are vital to *formulate* meaningful, testable hypotheses in ecology. Routine observations or analyses provide . . . *experience* to develop meaningful hypotheses. I stress meaningful because it is my view that much of what is called ecological research today involves the pursuit of hypotheses that are not well founded in empirical information."

Long-term data sets are obviously essential to testing most of the concepts or theoretical constructs central to ecology because most important ecological phenomena are of the type already mentioned. Furthermore, I assert that most of these concepts are not being tested, because their definitive test requires systematic, long-term observations. Many of these concepts are dogma, accepted on the basis of extrapolations from short-term studies, theoretical constructs, or studies using limited sets of organisms; furthermore, the organisms are often selected either because they are suited for studies limited in time and space or are likely to confirm the theoretical construct. Consequently, there is a large and often unacknowledged residuum of unanswered questions in ecology, a litter of hypotheses and notions of which we know neither validity nor robustness.

The failure to test validity and robustness of basic concepts is, in my opinion, one of the most grievous sins of modern ecology in general and ecological theorists in particular. In reviewing manuscripts and research proposals and preparing proposals we are more interested in new and intriguing ideas than in supporting long-term and workmanlike testing of concepts. A common reviewer comment is, "We already know that." Concepts are accepted and rejected with little empirical basis. Furthermore, the obvious need for examining the spatial and temporal validity or application of a process, structure, or mechanism, once identified, seems to have almost no support among ecological peers.

Ecological Concepts that Require Long-Term Validation

What are some of the concepts, central to ecological science, needing long-term data for definitive testing? Included are concepts related to successional processes, ecosystem changes associated with succession, predator/prey interactions, controls on productivity, patterns of mortality in long-lived plants, competitive interactions, geomorphic processes, including weathering of parent materials and erosion, and ecosystem responses to atmospheric inputs, including pollutants.

Succession is a topic rich in concepts that should be tested by long-term observations and experiments. Examples include: the relative importance of relay and initial floristics in seres (Egler, 1954); the occurrence and relative importance of Connell and Slatyer's (1977) successional mechanisms throughout a sere; the importance of gap phenomena in creating and maintaining compositional diversity in plant communities (see, e.g., articles in Pickett and White, 1985); the effect of herbivory on short- and long-term productivity of terrestrial ecosystems (Schowalter et al., 1986; Seastedt and Crossley, 1984); and the development and persistence of asymptotes or equilibria in community or ecosystem characteristics. The role of biological legacies – living organisms, organic materials (including coarse woody debris), and environmental imprints (such as patterning in soil physical, chemical, and microbiological properties) – on paths and rates of recovery following catastrophic disturbances is an emerging topic (see, e.g., Franklin et al., 1985). There is also the much debated issue of the potential for alternative successional pathways and end points.

Concepts of ecosystem change associated with succession are important in both basic and applied ecology. Significant efforts have been made to develop theoretical constructs (e.g., Odum, 1969), but testing has been limited primarily to chronosequences (see, e.g., MacMahon, 1980) rather than by long-term observations of the same system. Obviously chronosequences cannot be used to identify cause and effect. Many of the constructs, such as the purported relation between diversity and stability, have been widely debated and simultaneously proposed as the basis for management programs! Long-term patterns of organic-matter accumulation and loss and the theorized relationship between nutrient leakage and degrading ecosystems (Vitousek and Reiners, 1975) have received limited study despite their relative importance.

Predator–prey relationships are an important and controversial topic where long-term tests are both essential and lacking. Short-term studies can be clearly misleading, as noted in the case of the Isle Royale moose–wolf studies (Peterson, 1987). Predator–prey relationships are likely to be complex with long-term cycles, particularly in the case of larger vertebrates. Similar needs exist in understanding plant–herbivore interactions, including the balance between short-term damage and long-term stimulation, and long-term roles of pests and pathogens in ecosystems (Schowalter et al., 1986; Wickman, 1980).

Variations in and controls on ecosystem productivity are poorly known for natural ecosystems. Few ecosystems exist for which sustained measurements of productivity have been made. Long-term studies are essential, particularly in view of concerns with sustained productivity and the effects of global change in climate and pollutant loadings (see, e.g., Strain and Cure, 1985).

Relatively few theoretical constructs concerning the population dynamics of long-lived organisms exist, perhaps because the empirical base in long-term data is so weak. As an example, the rates, causes, and spatial patterns of death in tree species is poorly known despite a long history of forest mensurational research (Franklin et al., 1987). Tree mortality functions are, therefore, one of the weakest links in forest growth prediction and in the development of forest successional models. Inadequate knowledge of tree death is also creating difficulties in assessment of environmental problems, as noted earlier with red spruce; "normal" or "natural" rates of mortality must be known before one can identify abnormal rates of tree death!

Competitive interactions is a topic surfeit with theoretical constructs and short-term experiments. Serious long-term research is needed to determine the roles of these interactions over long periods, because it is clear that there can be significant shifts in consequences over time: e.g., relationships can shift from competitive to mutualistic. An example is the relationship between Douglas-fir and snowbrush (*Ceanothus velutinus*), a shrub associated with nitrogen-fixing bacteria. Over the short term, snowbrush may reduce growth of Douglas-fir seedlings whereas over longer times the Douglas-fir may benefit from increased soil nitrogen and more favorable soil flora and fauna associated with the occurrence of the shrub. In any case, short-term studies of competitive interactions run substantial risks that the flow of benefits may change in magnitude and kind over time. Similarly, interactions that are initially obvious may turn out to be unimportant in the long run, whereas some that are initially subtle may turn out to be important.

If long-term studies are essential to the robustness of the science of ecology, they are equally important in identifying and resolving societal issues. Ecological issues at the regional and global levels typically require long-term data sets for identification, evaluation, prediction, and decision making. Sustained productivity of forests, agricultural lands, and fisheries is a societal issue of this type. Given the importance of primary productivity, incredibly little research is currently focused on this issue. Other topics with a significant long-term component include the effects of climatic change; changes in key biogeochemical cycles (e.g., sulphur and nitrogen), photochemical oxidants, toxic trace elements, and organics; genetically engineered organisms; and losses of biological diversity at the subspecies and species level. Society will continue to be blindsided until ecologists begin to generate appropriate data bases.

Systematic Approaches to Long-Term Ecological Studies

Long-term studies are obviously essential to the health of our science and to society, and I assert that the necessary research is not likely to be accomplished with our current ad hoc approach. Given the potential importance of such work, some changes seem essential. We need a more extensive and systematic approach to long-term ecological studies. I view the following as some essential components of this approach.

Selection of Key Hypotheses

One of the first needs is selection of hypotheses or parameters for long-term study. Ecological scientists need to identify the central or elemental concepts deserving the energy and cost associated with definitive testing. Many procedures for identification of these concepts can be suggested, including a scientific polling or analysis of published literature. In any case it ought, in my opinion, to be a collective professional responsibility.

Establishment of Long-Term Studies

Long-term studies need to be established to specifically address ecological concepts which are selected as central issues. The studies should include experiments, where appropriate, to provide contrasting treatments. Nonmanipulative observation of processes in natural ecosystems will also be appropriate in many cases. Experiments should follow a KISS (keep-it-simple-stupid) principle and deemphasize sophisticated designs. The IES study (Strayer et al., 1986) noted the importance of a simple accommodating design, particularly one that can accommodate unforeseen modifications. Strayer et al. (1986) recommend experimental treatments and essential measurements that are straightforward–"unambiguously repeatable." Two examples of such "beautifully simple and accommodating" designs are cited–Rothamsted Park Grass (Lawes Agricultural Trust, 1984) and Gisburn Forest plots (Brown and Harrison, 1983). Cooperative projects with governmental agencies may improve the possibility for large-scale experiments because of their large land holdings and the security and stability that they can provide.

Developing appropriate designs for long-term research and monitoring programs is a complex endeavor. An example of one approach incorporating both biotic and abiotic components has been proposed by Wiersma and Otis (1986). Others have addressed the statistical issues (e.g., Hinds, 1984).

Comparative Studies of Processes

Comparative studies of processes must be conducted across and within biomes. There is an unfortunate tendency to view ecological processes or mechanisms as being mutually exclusive, a point of view encouraged by the emphasis on the null hypothesis. In fact, the issue in most ecosystems is the proportions or mix of various processes that are present. For example, competition, facilitation, and inhibition are commonly all operative mechanisms in ecosystem succession. Similarly, competition, predation, and the physical environment are typically all elements in the structuring of communities.

A related tendency among ecologists is to stop at the demonstration or discovery that a process is operative (often done by selecting an ecosystem where such demonstration is most likely) and an assertion of its cosmic importance and universal application. The result is sometimes extended debates of questionable value (see, e.g., Lewin, 1983a, 1983b). Rarely do ecologists attempt the logical next step—examining the importance of that process, agent, or structure spatially (in other communities or ecosystems) and temporally (in a sere). The peer review system discourages us from doing so because this is an elaboration on something that is "already known." I believe that systematic examination of how processes and structures vary in time and space is imperative if we are to develop truly general ecological theory. Ecological truth is not singular.

Studies Across Organizational Levels

It is essential that more research span organizational levels—population, community, ecosystem, and landscape. Work that links population and ecosystem processes is especially valuable, as demonstrated by the wide application of the JABOWA/FORET family of successional, forest-growth models (Shugart, 1984). Landscape issues include the structure and dynamics of edges and ecotones and processes that are operative there; and population and ecosystem responses to landscape pattern, including mobile organisms such as ungulates and anadromous fish (Swanson et al., 1988; Franklin and Forman, 1987). Many important natural resource issues, such as preservation of biological diversity, clearly require integration of knowledge at all levels, from the genetic to landscape. Preservation of the northern spotted owl (*Strix occidentalis caurina*) provides an excellent example of this type of problem (National Audubon Society, 1986).

Studies of Natural History

Studies of the natural history of organisms and ecosystems must have a major place in any program of long-term studies; indeed, such investigations need to resume their rightful place in our science. We must admit to the public, resource managers, and ourselves that such detailed and specific information is essential to making meaningful decisions about the use of biological resources as well as to the formulation of ecological hypotheses.

Procedural Considerations

Procedural considerations in a systematic program of long-term ecological study include methodology and data management. Developing some standard experimental designs and utilizing the best current technology in our measurement programs is important. This must and can be done without stifling originality and the development of new methodologies. Data quality assurance is critical. As noted in the IES study, it is essential to interpretation and use of long-term data sets that *all* data in the set be reliably accurate (Strayer et al., 1986).

Intensive data documentation and management procedures are essential (see, e.g., papers in Michener, 1986). Documentation of initial conditions and measurement programs has been a chronic problem. Data need to be assessed and archived on a systematic basis and be readily available for potential users. Collecting data and stuffing it into electronic closets is clearly inadequate; one of the most valuable practices is periodic scientific analysis and reporting of the data. Such periodic exercise will help assure that the data are appropriate and of high quality.

Exclosure studies provide an example of the importance of careful initial measurements and documentation. Exclosures are a common method of excluding vertebrates from segments of ecosystems in long-term studies. Yet, all too often the comparison to controls is made at some time period after establishment and information is absent on initial conditions (Tiedemann and Berndt, 1972; Ross et al., 1970; Hanley and Taber, 1980). The assumption is made that the treated and control areas were originally the same. This is often not the case, given the high spatial variability of vegetation. For example, in the southern Appalachians measurements of tree reproduction were made within and outside exclosures; deer browsing was found to have no effect on reproductive density (Harlow and Downing, 1970). Subsequent measurements showed that browsing actually did effect density and that the earlier and erroneous conclusions were the result of initial differences in tree density between grazed and ungrazed areas.

It is important that the ecological community lose its phobia of "monitoring." There is no clear or absolute distinction between long-term research and monitoring programs and, in any case, it is obvious to all that some monitoring activities are essential to the progress of our science. Yet we often resort to rhetorical handstands to avoid using the word "monitoring." Monitoring must be rescued from its association, in some minds, with mindless data collection and restored, where appropriate, to our vocabulary. Monitoring is a part of research.

Current Long-Term Programs in Ecological Science

Some programs for study of ecological phenomena exist that indicate the potential for a systematic approach to long-term ecological research. Within the United States we have, for example, the hydrological monitoring programs of the U.S. Geological Survey and the climatic data gathering and forecasting activities of the National Oceanographic and Atmospheric Administration. The National Atmospheric Deposition Program (NADP) is an example of a recent, multiagency, voluntary effort to provide continuing measurements of precipitation chemistry (National Atmospheric Deposition Program, 1987). This system includes standardized field methodology, a common analytic laboratory, and shared data which are subject to quality control and systematic analysis. All of these programs are, however, relatively narrow in scope and lack an ecological focus. Multimedia programs do exist, although they have not been as widely implemented (e.g., Davidson et al., 1985; Wiersma et al., 1987).

In general, scientists seem to have done much better in developing long-term programs for measurement and study of abiotic than of biotic factors. Factors contributing to greater consensus in the abiotic realm may include the obviousness of some parameters and their clear relevance to societal needs (e.g., weather), institutions that accept a monitoring responsibility as part of their mission, and greater ease in standardizing methodologies.

There are programs in other countries that provide models which include both abiotic and biotic components; the Swedish Environmental Monitoring Program is an example (National Swedish Environment Protection Board, 1980, 1985). This program collects data in both polluted and relatively unpolluted regions of Sweden. The Global Environmental Monitoring System conceived by the World Meteorological Organization has suffered from international politics but also does provide some guidance; it is also weak in the area of biological monitoring, however.

Efforts in the biological arena have been much less successful. The U.S. Department of Agriculture Forest Service (USFS) once had extensive plot-based programs for studying the population dynamics of tree species and productivity of forest and range communities. Protocols existed for the standardization of these studies nationally (see, e.g., Division of Silvics, 1935). The USFS also monitored many small watersheds, often as parts of major experiments in land use. Such long-term studies have been drastically deemphasized in the last 25 years, however, and the majority of existing studies terminated. Monitoring is required of the USFS by the National Forest Management Act, but apparently this will focus on achieving programmed management activities (e.g., road building and timber harvest) and not on ecological issues.

The National Park Service (NPS) has never had a long-term research or monitoring program even though the need was recognized over 50 years ago (Wright et al., 1933; Wright and Thompson, 1935). Such programs are obviously needed to assure preservation of the key resources and populations in essentially every park. A limited number of successful examples do exist, some resulting from NPS efforts, such as the outstanding marine resource monitoring program at Channel Islands National Park (Davis, 1983), and others primarily from outside academic efforts, such as the wolf-moose studies at Isle Royale National Park (Peterson et al., 1984; Peterson, 1987). NPS is concerned with establishing long-term monitoring programs and has developed in-service documents with proposed policies and objectives for such activities, an effort that has received emphasis from the current NPS Director (Boyd Evison, personal communication). Several parks (including Olympic and Sequoia-Kings Canyon) have also used their participation in the National Acid Precipitation Assessment Program to initiate long-term ecological research and monitoring programs; these include standardized core measurements of atmospheric inputs, meteorology, soil, vegetation, and aquatic processes (Parsons and Graber, 1985).

The Nature Conservancy (TNC) has one of the most extensive monitoring systems of any of the resource managing agencies. Monitoring programs are designed as a part of stewardship plans which have been developed for all major TNC preserves. The monitoring is strongly focused on assessing the degree to which conservation objectives are being met, such as maintenance or expansion of populations of a rare or endangered species included within a preserve. Sampling schemes which will provide a sound statistical basis for assessing trends (an important issue identified by Hinds (1984)) and design of programs which will monitor general ecosystem health are important concerns in the TNC program.

The Long Term Ecological Research (LTER) program funded by the National Science Foundation is the closest approach to systematic long-term study of key ecological processes. The basic structure of this program evolved through three major workshops (Botkin, 1977, 1978; The Institute of Ecology, 1979). The core research areas, which all funded programs are required to address, are: (1) pattern and control of primary productivity; (2) dynamics of populations of organisms selected to represent trophic structure; (3) pattern and control of organic matter accumulation in surface layers and sediments; (4) patterns of inorganic inputs and movements of nutrients through soils, groundwater, and surface waters; and (5) patterns and frequency of disturbances. Since its inception in 1980, three competitions have been held and 15 sites are now funded out of a planned total of 20. The sites are broadly representative of ecosystem types found in the conterminous United States and in Alaska (Callahan, 1984).

The LTER program does have limitations as a systematic program of long-term studies, however. Site programs are quite individualistic within the very broad limits of the five core areas. Such individualism is essential under the peer review system where each site has to compete individually. And the funded sites are widely divergent in character. Both of these factors limit the commonalities and potential for comparative study at the level of experimentation and hypothesis testing. Finally, the number of LTER sites is insufficient to adequately sample the variability within and between biomes. Hence, long-term research needs to be systematized at additional locales. The LTER sites are developing a networking program which is designed to address these difficulties and encourage standardization and comparative research.

There have been many proposals over the last 20 years for globally based ecological studies (e.g., Lundholm, 1968; Jenkins, 1971; Izrael, 1982; Ecological

Research Committee, 1970). In 1974 a Global Environmental Monitoring System (GEMS) center evolved from earlier proposals and was established by the United National Environment Programme in Kenya. Subsequently, steps have been taken to create an Integrated Global Background Monitoring Pilot Project which will (1) develop standardized site selection and monitoring procedures, (2) establish baseline levels for selected ecosystem parameters, and (3) serve as an early warning system for detecting long-range transport of pollutants and changes in ecosystem processes. An overall purpose of the project is to establish long-term studies of key ecological processes and relate these to well defined abiotic components (Wiersma et al., 1987).

Conclusions

Sustained ecological research has been recognized as a critical global need by this symposium. I believe that ecological scientists have a major responsibility in seeing that this need is fulfilled by expanding, focusing, and stabilizing long-term studies in ecology. It is also our responsibility to shape the questions, to identify the central concepts for testing, and the parameters for measurement. Oceanographers have demonstrated that such a concensus is possible in an equally complex area of inquiry (Delaney et al., 1987).

We need to develop the mechanisms to provide continuity and quality control. It may be necessary to create institutions that can design and carry on the work or adapt existing institutions to these needs. Perhaps centers for analysis and synthesis, such as atmospheric scientists have created in the National Center for Atmospheric Research, are needed. The LTER program and system might be used as a nucleus from which an extended system of collaborating sites can be developed. Almost certainly it will be essential to develop the financial resources to do the necessary work. Working through the executive and congressional branches of government is one route. However, development and use of endowments are an interesting alternative given the high level of stability which they provide. This approach is used very effectively by TNC to provide stable support of its stewardship and monitoring programs and is, of course, a major source of support for IES.

Last, but absolutely not least, we need to train and reward young scientists for their participation in long-term research. This means modifying our evaluations of vitae in proposal and tenure considerations to recognize essential service to the science. We also need to emphasize integration of disciplines and organizational levels in graduate training, reducing the destructive acrimony among subareas in ecology. IES provides an excellent example in mandating recognition and reward for multiauthored papers as a part of its charter.

Ecologists have an opportunity to play an activist role, much as has been done by other professional groups, as illustrated by the American Chemical Association's proposal on global tropospheric chemistry. If we do not take action, ecology will continue to be a fractionated and weak science, and physical scientists will continue to take charge of major environmental projects, such as the International Geosphere Biosphere Program (IGBP). IGBP is a major emerging effort to examine global environmental change and its consequences.

Long-term observations are central to almost every important ecological concept and to every environmental issue. Our approach to long-term study has been laissez-faire. This must change.

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