

# The Evolving Role of Science in Wilderness to Our Understanding of Ecosystems and Landscapes

Norman L. Christensen, Jr.

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**Abstract**—Research in wilderness areas (areas with minimal human activity and of large spatial extent) formed the foundation for ecological models and theories that continue to shape our understanding how ecosystems change through time, how ecological communities are structured and how ecosystems function. By the middle of this century, large expanses of wilderness had become less common and comparative studies between undisturbed and human-dominated landscapes were more prevalent. Such studies formed the basis an evolving understanding of human impacts on ecosystem productivity and biogeochemical cycling. Research in the last third of this century has repeatedly taught us that even the most remote wilderness areas are not free of human influence. Nevertheless, extensive wilderness has been and will continue to be critical to our understanding our impacts on nonwilderness landscapes.

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No one would quibble with the assertion that research in wilderness has been important to our understanding of the functioning of ecosystems and landscapes: the majority of papers published in ecological journals over the past century were either done in or make heavy reference to ecosystems in which human impacts are minimal. As so many papers in this volume have demonstrated, our vision of wilderness over the past century (corresponding roughly to the period during which the field of ecology as a formal discipline was born and has matured) has changed considerably as a consequence of research in wilderness. Our visions of wilderness have just as surely shaped the core values and character of ecosystem and landscape science.

I have chosen to focus on the evolution of ecological sciences over the past century—1899 to 1999. Much ecological research certainly predates this time period, although it does coincide roughly with the recognition of ecology as a formal academic discipline, as evidenced by establishment of faculty chairs and peer-reviewed journals devoted to this topic. I shall dwell in particular on three areas of inquiry—1) disturbance and change, 2) the nature of the community and 3) the “physics” of the ecosystem—that were born and matured during this time period. I conclude with reflections on the evolution of ecosystem and landscape science in relation to our understanding of our relationship to wilderness.

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Norman L. Christensen is Professor of Ecology and Dean of the Nicholas School of the Environment, Box 90329, Duke University, Durham, NC 27708 U.S.A., e-mail: normc@duke.edu

## Disturbance and Change

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Henry Chandler Cowles’ (1899, 1901) studies of succession on sand dunes newly exposed by the retreat of the Lake Michigan shoreline initiated a search for a unifying theory of ecosystem change that continues to this day. Cowles’ primary interest was in understanding the relationship between the process of vegetation change and landform evolution. He did not advocate a unifying theory for successional change in this work, but his assertions on several important points were central to the evolution of such a theory. These assertions included directional change converging on a stable climax community and “biotic reaction,” that is, organismal influences on the environment as a driver of change.

## A Unifying Theory of Change

It was Frederick Clements (1904, 1916, 1932), drawing on observations from both natural and human-disturbed situations, who must be credited or blamed for asserting a comprehensive theory of succession. Building on Cowles’ notions of convergence and biotic reaction, Clements saw succession as driven by a combination of biotic reaction and competition among dominant species proceeding in relatively discrete stages along a sere that culminates in the most stable assemblage of organisms imaginable, the climax community. He argued that climate was the ultimate determinate of the character of that climax, all other environmental influences being ameliorated by biotic reaction. Competition sharpened the boundaries or ecotones between such communities in space and among seral stages in time. Although he did not use this exact phrase, Clements envisioned communities as highly coevolved entities with a similarity to organisms that is far more than metaphorical.

Clements had much to say about the character of the world’s wilderness prior to significant human influences.

Under primitive conditions, the great climax communities of the globe must have remained essentially intact, since fires from natural causes must have been both relatively infrequent and localized. Succession was far less general and was represented chiefly by priseres, especially in water and dune sand; subseres were few in number and small in extent. They became universal features only as man extended his dominion over nature through disturbance and destruction and they are permanent today in the degree to which these forces are continuous or recurrent. From the very nature of climax and succession, development is immediately resumed when the disturbing cause ceases, and in this fact lies the basic principle of all restoration and rehabilitation. Left undisturbed, every bare denuded or seral area begins its slow but

inevitable movement to the climax wherever the latter has not been destroyed over too large a territory to permit mobilization of the successive populations (Clements 1935).

This quote is from a paper entitled “Experimental Ecology in the Public Service” and certainly reflects the prevailing views at that time about the proper focus of wilderness conservation (climax communities) and the need to exclude disturbances, especially fire.

## A Theory Challenged

Today, we often depict Clementsian successional theory as the prevailing paradigm among ecologists for the first half of this century. Perhaps this is because of its simplicity and apparent explanatory power, its impact on policies and protocols for managing land resources and public agencies or its incorporation into so many ecology textbooks. But the fact that this theory has served as the starting point or straw man for so much research on ecosystem change over the past four decades has certainly reinforced that view. It is, however, important to note that views contrary to Clements’ doctrinaire approach to succession were commonplace early in this century, and those views were most often shaped by observations and data from wilderness areas. Several important themes from that work have influenced research up to the present. I refer to these as 1) the lexicon of climaxes, 2) the ugly fact of “retrogression” and 3) the inevitability of change.

**The Lexicon of Climaxes**—It was early observed that, even in seemingly undisturbed areas, rather stable communities existed that did not match up with the expectations for climax assemblages within particular climatic regimes. It appeared to many that unique hydrologic conditions (Cain and Penfound 1938; Harper 1914), geology (Billings 1950; Platt 1951), soils (Wells and Shunk 1932) or salinity (Wells 1939) were often sufficient to prevent biotic reactions from pushing change to one climatically-determined endpoint. One approach to this dilemma was a proliferation of terms such as preclimax, disclimax, edaphoclimax, aquatic climax and topoeclimax to describe this variation (for example Hansen 1921). Indeed, Whittaker (1953) counted no fewer than 36 types of climaxes named in the literature. Another approach was to abandon the notion of “climatic monoclimate” altogether in favor “polyclimax” or succession to stable endpoints determined by site-specific environmental factors such as soils, hydrology and topography (Cain 1939; Oosting 1948; Tansley 1935). This idea preserved many of the elements of Clements’ theory, but discounted the primacy of climate.

**The Ugly Fact of Retrogression**—One of the most appealing features of Clements’ world view is that succession leads inexorably to the best possible community configuration within the constraints set by the environment. This feature was highlighted in Odum’s (1969) trends to be expected during succession. Whether succession could lead to less fit community configurations was a matter of considerable debate early in this century. Clements (1916) opined that such change was not possible, whereas others cited evidence from a variety of wild areas that it was in fact common (Cowles 1901, 1919;

Cooper 1916, 1926). An oft-cited example of such retrogression is the succession of white spruce forests to black spruce “muskegs” owing to hydrologic and microclimatic changes wrought by the maturation of the white spruce canopy and associated ground-layer vegetation (Cooper 1916).

**The Inevitability of Change**—Climatic constancy over millennial time spans would seem to be a prerequisite for climatic monoclimate; however, the variability of climate over time spans relevant to successional change was appreciated early on. It is significant that Cowles (1911) recognized the importance of this fact in his characterization of succession as a “variable converging on a variable.” The idea that communities become increasingly resistant to change—intrinsically more stable—through succession was, nevertheless, widely held. Indeed, if a climax community is the most stable configuration of species possible in a particular environment, how could it be otherwise (Peters 1976)?

Very early in this century, ecologists were becoming aware that disturbance, particularly fire, was important on many landscapes. In papers that contain many of the elements of modern-day landscape ecology, Harper (1911, 1914) emphasized the importance of repeated wildfires on the structure and spatial distribution of vegetation in north Florida and in the Everglades. Garren (1943), Harper (1940) and Wells (1942) recognized the importance of fire across the entire southeastern coastal plain. Cooper (1922) cited the prevalence of fire in the Mediterranean ecosystems in California, and he described many of the adaptations of species in those ecosystems to fire. However, it was Sampson (1944) who first understood that accumulation of woody debris and biomass during succession in chaparral shrublands actually made them more susceptible to fire.

It was work by Biswell (1967) and Kilgore (1973) in the giant sequoia forests of the central Sierra Nevada which showed that the connection between successional change and the likelihood of disturbance was complex indeed. They demonstrated that, in the absence of frequent (every 8-15 years) light surface fires in giant sequoia groves, shade-tolerant species of fir and incense cedar invaded the understory. This, coupled with the accumulation of woody debris, made crown-killing fires more likely. Furthermore, in the absence of fire, seed germination and seedling establishment of the giant sequoias were very limited.

## Cyclic Change and Pulse Stability

That ecosystems displayed cybernetic behavior—that is, behavior constrained by internal feedbacks—was a matter of great interest and debate during the decades of the 1950s and 1960s (for example, Margalef 1957, 1968; Odum and others 1960). Odum (1969) saw directional change toward a stable climax as an inexorable consequence of such behavior. In his view, as succession proceeds, regulation of change becomes increasingly autogenic (that is, cybernetic). However, Loucks (1970) and Jordan (1972) pointed out that autogenic changes could also lead to cyclic behavior—pulse stability (Loucks 1970) or bounded instability (Jordan 1972). In this model, succession produces changes that make a community more susceptible to disturbance.

The disturbance, in turn, generates renewed process of succession.

The idea of cyclic succession certainly predates the coining of terms like pulse stability. Harper (1911), Wells (1942) and Garren (1943) certainly appreciated the importance of repeated fire on the southeastern landscape, although they may not have grasped the fact that fire probabilities were driven in part by autogenic changes within these ecosystems. The connection between fire probability and fuel accumulation was, however, well understood early on (for example, Biswell and others 1952; Cooper 1961; Sampson 1944). Mutch (1970) suggested that natural selection in such ecosystems may well have produced plant characteristics that increased flammability, thus ensuring regularity in fire cycles (see, however, Christensen 1985).

The so-called thaw-lake cycle on the wet tundra of the north slope of the Brooks Range of Alaska represents pulse-stability on a much longer time scale (Bliss 1988; Peterson and Billings 1978). Here, the landscape cycles among various stages of polygonized ground and shallow lakes are driven by autogenic changes in surface energy budget, location of ice wedges and the depth of the permafrost layer.

That change is constant and cyclic is actually very much a part of Watt's (1947) much celebrated model of pattern and process in the plant community (see also Bormann and Likens 1979). In Watt's view, it was primarily the spatial scale of disturbance and recovery patterns that change through time; that is., the processes of change in small-scale disturbances (a windthrow, say) in mature ecosystems in many ways replayed the larger-scale patterns of change that produced the ecosystems in the first place.

That change in wilderness areas is more complex than autogenic trajectories leading to stable climax or autogenic stable-limit cycles is obvious from the detailed descriptions in the work by individuals such as William Cooper, Homer Shantz, Roland Harper, B.W. Wells and Stanley Cain that filled the pages of ecological journals during the first four decades of this century. Nevertheless, the notion of autogenic change is implicit in the "balance of nature" concept that was (and to some extent still is) a basic assumption that scientists brought to their studies of wilderness. Furthermore, as I discuss below, both of these notions provided the justification for management policies and protocols that were consistent with our historic view of wilderness and the role of humans in it.

## Patch Mosaic Landscape Dynamics

The prevailing paradigm for understanding ecosystem change is captured in the phrase "patch mosaic" landscape dynamics. While wilderness has played a significant role in the development of this concept, it has its roots in studies of near-shore and intertidal marine ecosystems and of islands.

The focus of early discussion surrounding MacArthur and Wilson's (1967) equilibrium theory of island biogeography was on the properties of islands that rendered them more or less species rich. But it was the assertion that the species diversity of place was a consequence of an endless balance between immigration and local extinction that would revolutionize our thinking about the structure of wilderness communities. Rather than a static property of

climax ecosystems, diversity is now seen as a dynamic feature, subject to change based on factors that influence immigration and factors that put populations at risk.

Working on the communities of sedentary invertebrates and algae that "foul" virtually any solid surface, Sutherland (1972) suggested the idea of "multiple stable points"—that communities might change continuously in structure, but that certain assemblages of species were more stable than others and thus persisted for longer periods and consistently reappeared from time to time. This idea was given a "spatially explicit" context in the work of Levin and Pain (1974) and Pain and Levin (1981) in which they demonstrated that the "landscape" of the intertidal zone could be viewed as a spatial array of patches undergoing change and that the nature of that change was a consequence of within-patch autogenic process, patch size, and the relationship of the patch to other patches. This latter factor was important as it influenced opportunities for immigration to a patch. Perhaps the most important lessons from this idea are that spatial scale influences change, and it matters where a patch is relative to other patches on a landscape.

Terrestrial ecologists working in a variety of wilderness areas were quick to see the importance of this concept. Sprugel's (1976, 1984) description of the waves of disturbance and regeneration that move through the spruce-fire forest of the northeastern Appalachians captures many of the elements of Levin and Paine's model, particularly the importance of spatial relationships. Work by Romme (1981) and Romme and Knight (1981, 1982) set the stage for the widespread application of patch-mosaic thinking in the Greater Yellowstone Ecosystem following the 1988 fires (Romme and Despain 1989). Myers' (1985) work on the distribution of sand pine scrub, long-leaf pine-wiregrass and live oak ecosystems on the central Florida sand ridge provides a very compelling example of patch mosaic landscape dynamics.

Landscapes can be modeled in terms of a demography of patches, characterized by a matrix of transition probabilities describing the likelihood of a patch in a particular state changing into some other state over a particular period of time. Such probabilities are known to be affected by the autogenic processes driving change within a patch (thus capturing concepts from earlier theories of change), the size and configuration of a patch, the character of surrounding patches, and extrinsic factors that might influence disturbance. This is made more complex by the certain knowledge that such a matrix cannot be "stationary," but that these probabilities are shifting through time with changes in climate and the overall structure of the landscape mosaic (Usher 1979; White and Pickett 1985). Precisely defining such a transition matrix for any landscape would be daunting, but understanding the factors that might determine such probabilities continues to be an important focus for research in wilderness areas.

## The Community

The field of systematics is blessed with the fact that organisms, notwithstanding some messiness caused by hybridization, asexual reproduction, etc., can be classified in a natural hierarchical taxonomic system. We say

it is natural because it is a consequence of the process of evolution and its basic units, species, are created (at least in higher organisms) as a consequence of reproductive isolation. Although genetic variation within species is typically rampant, there is general acceptance of the notion of genetic discontinuity among species.

It seemed reasonable to those first studying wilderness landscapes that assemblages of organisms might be classified in some sort of similar "natural" taxonomy, based on an understanding of the underlying factors or processes that produced those assemblages. That certain assemblages do repeat themselves seemed apparent to most early students of wilderness. C.H. Merriam's (1898) "life zones" may represent the first attempt at such a natural classification. Merriam noted distinct transitions from major physiognomic types (desert, grassland, shrubland, forest, etc.) as one proceeded from low elevation to higher elevation in the mountains of the desert Southwest, which he described and named as distinct zones (Sonoran, alpine, etc.). Furthermore, he called attention to the similarity between elevation and latitudinal transitions in such zones. These life zones were recognized primarily by physiognomy and by their dominant species. Merriam assumed that climatic variation along elevational and latitudinal gradients was the factor underlying these zones, but he did not consider why such zones would be so distinct, given the gradual variation in climate.

Clements' ideas on vegetation change and the nature of the community provided the basis for a much more sophisticated hierarchical taxonomy of communities. Clements (1932) shared Merriam's conviction that climate was the overriding environmental factor affecting the distribution of plant communities, and he went on to provide a basis for why such communities would have abrupt boundaries in space and time. Clements argued that one could speak of communities in the "abstract" as idealized notions that had reality as a consequence of the biotic reactions of and competition among dominant organisms. In the same way that each of us can be grouped in the unit *Homo sapiens*, the "concrete" community was that spatially bounded entity that one encountered on the ground, and the abstract community was the taxonomic unit to which it belonged. The biotic reactions of dominant species ameliorated the environment in a way that created extensive uniform environments in their understories in the face of more gradual and continual change in climate. The boundaries or ecotones between communities were sharpened by competition among the dominant species.

The notion of the abstract community was central to Clements' organismal concept. Clements was convinced that the natural processes of autogenic change, biotic reaction, dominance and competition inevitably produced highly coevolved communities, and he posited an elaborate taxonomic hierarchy of formations, associations, sociations, communities, etc. as a natural consequence of those processes.

Although it would appear that few ecologists even in Clements' time accepted his climatic monoclimal theory (Cain 1939; Cooper 1926), the question of whether communities, abstract or concrete, really existed persisted—indeed dominated much debate among plant ecologists—well into the latter half of this century. One of the strongest latter-day proponents of the fundamental existence of abstract communities (he referred to them as "typal") was

Rexford Daubenmire (1966), working in wilderness in eastern Washington state. Like Clements, Daubenmire argued that autogenic processes (biotic reaction and competition) within communities produced extensive relatively homogeneous communities with abrupt boundaries. (I am purposefully not treating the very detailed community classification system of Braun-Blanquet (1932) still in wide use in Europe. I use the lame excuse that it was developed largely on human-impacted landscapes. Because this system is based on the fidelity of individual plants to unique environments, one could argue that it is quite "Gleasonian").

In his 1926 exposition of his "individualistic" view of the plant community H.A. Gleason describes a variety of landscapes in wilderness as diverse as Cascadia, the Andes, the desert and the complete run of the Missouri and Mississippi Rivers in which discrete communities appear to be absent.

In conclusion, it may be said that every species of plant is a law unto itself, the distribution of which in space depends upon its individual peculiarities of migration and environmental requirements. Its disseminules migrate everywhere, and grow wherever they find favorable conditions. The species disappears from areas where the environment is no longer endurable. It grows in company of any other species of similar environmental requirements, irrespective of their normal associational affiliations. The behavior of the plant offers in itself no reason at all for the segregation of definite communities. Plant associations, the most conspicuous illustration of the space relation of plants, depend solely on the coincidence of environmental selection and migration over an area of recognizable extent and usually for a time of considerable duration. A rigid definition of the scope or extent of the association is impossible, and a logical classification of associations into larger groups... has not yet been achieved.

Indeed! In his later (1939) treatise on this individualistic concept, Gleason did allow as how biotic reaction and competition might be important as part of the environment at a given location, but he argued that these processes should themselves vary in gradient fashion. This concluding paragraph from Gleason's 1926 paper on the community seems to lead almost directly to such modern concepts as the equilibrium theory of island biogeography and patch mosaic landscape dynamics.

Unquestionably, the assumptions that ecologists carried with them into the wilderness influenced the way in which they interpreted it. Indeed, those assumptions very much drove the methodologies they brought to bare on this debate. In Daubenmire's (1966) words, "it appears to me that if one selects any of several appropriate methods, one can demonstrate a continuum anywhere. The crux of the problem, as I see it, lies in the validity of methods of gathering data, or in their subsequent manipulation, if not both." Daubenmire argues that areas affected by human activities or disturbance must be avoided, and that sampling should be done only in "homogeneous vegetation."

Although methodologies for examining continuous variation were well developed in disciplines such as sociology during Gleason's time, they were discovered (in effect, reinvented) by plant ecologists in the 1950s by John Curtis and his students for Wisconsin ecosystems (Bray and Curtis 1957; Curtis 1959; Curtis and McIntosh 1951) and Robert Whittaker (1956) for the Great Smoky Mountains. These

ecologists argued that the “stratified” sampling of Daubenmire was predestined to find discontinuity on landscapes—sampling should be random. Curtis, Whittaker and their students and associates pioneered a variety of multivariate techniques for posing hypotheses and analyzing such data, and the repertoire of such techniques continues to grow.

In exhaustive reviews, Whittaker (1967, 1975) and McIntosh (1967) effectively put this issue to rest. While it is convenient to speak of community types, largely based on the importance of particular dominant species or growth forms (see, for example, Whittaker 1962), the world does indeed vary continuously. Although organisms modify their environments and competition does indeed influence species’ distributions, these facts do not inexorably lead to spatially homogeneous communities that are repeated over and over across the landscape. Nature has not provided us with the basis for a natural classification of communities or ecosystems; any such classification is arbitrary and should be based on the needs and goals of the classifier. I return to the significance of this reality to wilderness management below.

## The Ecosystem

In a wonderful paper in which he tried to “set the record straight” on many of the issues discussed above, Arthur G. Tansley (1935) offered up a new term, the ecosystem. He applied this term to mean “the whole *system* [Tansley’s emphasis], in the sense of physics, including not only the organism-complex, but also the whole complex of physical factors forming what we call the environment....” Tansley considered ecosystems to be quasi-organisms in the sense that they could be understood by the same principles of physics and chemistry that were in his time the cutting edge of organismal physiology.

Notwithstanding Lindemann’s (1942) work on trophic dynamics in lakes, it would be nearly two decades (Odum 1956, 1957; Odum and Odum 1955) before this concept began to influence actual on the ground (or in the water) research, and the brothers Odum clearly led the way. It is not my wish to rehash here the history of the development of ecosystem ecology; excellent reviews are available in Christensen and others (1996), Golley (1993), Hagen (1992) and Pomeroy and Alberts (1988). Comparatively little ecosystem research has actually been focused exclusively on wilderness areas, although comparisons between disturbed and relatively undisturbed areas (as in the paired watersheds of at Hubbard Brook and elsewhere) have been very important indeed. I do wish to assert that several important lessons from ecosystem studies over the past four decades very much condition our current understanding of wilderness and the future role of wilderness for science.

Perhaps the most daunting lesson from our studies of ecosystems is the inescapable reality of the laws of conservation of mass and energy and thermodynamics. At any scale we may choose, ecosystems are necessarily open to inputs and outflows of matter and energy. Energy transformations are such that disorder (entropy) of the universe always increases; local reversal of that trend within an ecosystem depends on the input of energy.

Given these realities, there is no agreement on exactly how an ecosystem ought to be bounded. Indeed, the operational definition of ecosystem boundaries is left to the ecosystem ecologist (Christensen and others 1996), who usually defines them based on the ease of measurement or manipulation of inputs, outputs or internal processes. Therefore, the selection of boundaries most often depends on the process of interest—the scale appropriate for processes driven by flows of water (a watershed for example) may be inappropriate for processes driven by other factors.

## Wilderness Science and Wilderness Management

Wilderness conservation or preservation, I would argue, focuses on three questions, 1) what should be preserved, 2) what size and form should preserves have and 3) by what means should preserves be managed? It is perhaps debatable whether, over the past century, our scientific understanding of wilderness has shaped or merely justified our answers to these questions. Either way, the impact of science in wilderness on wilderness is powerful.

The question of what to preserve has both a philosophical and an operational dimension. Philosophically, we might ask what should comprise the shopping list? Species? Communities? Habitats? Ecosystems? Landscapes? If we agree that our focus should be on ecosystems and landscapes, we are not likely to find agreement on the items we ought to include on the list. The list of endangered ecosystems constructed by Noss and others (1995) will serve as a starting place for those who see wilderness conservation as a high priority; others will surely see it as “too fine a filter.” That nature has not provided us with a natural system of classification of communities, ecosystems or landscapes means that any such list is driven by *a priori* goals and values.

The evolution of our understanding of ecosystem change and ecosystem processes has provided us with important information relevant to the design of wilderness preserves. Unfortunately for much of our system of wilderness parks and preserves, the information came too late. The boundaries of few preserves have been designed based on understanding of the scale of the ecosystem processes that sustain them. Lessons from the Yellowstone fires or the management of endangered species (often top carnivores) is that the areas we have set aside are poorly designed and too small. We are beginning to understand that, in the best of all worlds, we will be able to set aside a small portion of the world as wilderness, and our success in managing it will depend as much on how we manage the surrounding matrix of lands as on our protocols for management within the preserves.

Although early legislation creating wilderness parks focused on “natural and historic” *objects*, we have come to understand that wilderness management is not a simple exercise in museum curation (Christensen 1995), that we must understand and manage the processes that sustain these dynamic systems. When we viewed change as leading inexorably to stable communities, and disturbance as setting that process back, policies and protocols to exclude disturbance seemed appropriate. In a world

driven by regular cycles, our view of what to preserve became more complex, and we recognized that disturbances such as fire and the changes they set in motion needed to be incorporated into wilderness management. In the more complex and uncertain patch mosaic world we now live in, we are beginning to understand that such management can be daunting. We are not only aware that the current state of our knowledge on these dynamics is incomplete and provisional, but we are also hopeful that this is the case.

Over the past decade, "ecosystem management" has been advocated as a means of coping with each of these challenges. Such management attempts to incorporate our understanding of the dynamics of ecosystems and landscapes, the arbitrary nature of boundaries, and the "physics" of the ecosystem (Christensen and others 1996). Perhaps even more important, it seeks to include humans in management in a more realistic and sustainable way (Lackey 1998).

The human dimension of ecosystem management, whether on managed landscapes or in wilderness, has, nonetheless, been far more difficult to articulate. It is clear that we humans are an inescapable part of the biota of virtually any ecosystem on the face of the earth. While we may wish to view wilderness as that part of the earth where we have the least impact, we should not deny our historic and current impacts on even the most pristine landscapes. It is equally clear that ecosystems are open not only to flows of matter and energy, but also to the flows of human values. It is this fact that makes ecosystem management truly daunting. The spatial and temporal domains in which human values are important to an ecosystem are often far different from those relevant to matter and energy. This has been evident in the attempts of management agencies to ensure "stakeholder involvement" in management decisions. Stakeholders are not just those individuals contained within an ecosystem or those directly influenced by the behavior of that ecosystem. By their willingness to support various nongovernmental organizations (whether Wise Use or Earth First) or vote for representatives who set land-use policy, individuals who may never set foot in a wilderness area are nonetheless stakeholders. Given the evolution of global communications and the global market place, human values are transmitted at the speed of light and may have surprising consequences. For example, creation of a carbon trading system within the proposed Kyoto accords may create new and very real economic values for wilderness ecosystems. Changes in those values might well influence the values we set for commodities such as wood fiber, which in turn will likely affect much land management.

Unlike matter and energy, there is no law of conservation for human values, and for this we should be grateful. Old values and views can and do disappear to be replaced by new and hopefully better informed ones. Science in wilderness has played and should continue to play a significant role in this process.

## **Closing Thoughts: The Evolving Role of Wilderness in Science \_\_\_\_\_**

During much of this century, wilderness was that part of the world where ecologists went to understand the nature

and functioning of ecosystems free of human effects. Even where the focus of research was on ecosystem change following human activities, such as the abandonment of agricultural fields, ecologists sought to study such change as if human influences ceased at the moment of abandonment (for example Oosting 1942). The fallacy of this form of denial was obvious to some early on. While Aldo Leopold (1941) argued that wilderness provides the "base datum for normality" for a "science of land health," he also lamented, in a 1927 letter to the superintendent of Glacier National Park, that "the balance of nature in any strict sense has been upset long ago.... The only option we have is to create a new balance objectively determined in each area in accordance with the intended use of that area." (cited in Knight and Wallace 1989).

I am sure I was not the only ecology Ph.D. candidate working in the 1960s who was admonished by his advisors to avoid working in areas where human influences might confound one's understanding of the true workings of ecosystems. Confident that I had selected such a place for research on the biogeochemical impacts of fire in chaparral, I developed a completely "biocentric" explanation for seasonal pulses of soil nitrate in these shrublands (Christensen 1973). I was more than a little embarrassed when Schlesinger and Hasey (1981) demonstrated that these seasonal dynamics were driven by anthropogenic injections of inorganic nitrogen compounds into the atmosphere over 100 kilometers from my study area.

During the last half of this century, areas where human impacts have been minimal have served as "controls" against which to compare the impacts of human action or disturbance such as in the paired watershed studies at Hubbard Brook (Likens and others 1977). Given the ubiquity of human impacts (such as CO<sub>2</sub> enrichment, air quality degradation and climate change), the concept of a "control" ecosystem without human influence is the ecological equivalent of the frictionless plane in physics. This is not to deny the importance of less disturbed areas for comparative research, but it does emphasize the need to be sure that our interpretations are not confounded by interactions with the growing array of background human influences.

Some argue that, in the time span of at least the past 10 millennia, wilderness absent of human influence is a myth (Christensen 1995; Cronon 1995). We are, after all, certain that the nearly complete loss of the Native American from much of the North American landscape represents the loss of one of the most important top carnivores and agents of change in that landscape during the Holocene. We are only beginning to understand the magnitude of those influences, and we are struggling with how best to incorporate them into our understanding of and vision for the North American wilderness.

Mark Twain wryly commented on a totally unrelated matter that "the researches of many commentators have shed considerable darkness on this subject and should they continue we will surely know nothing." A cynical view of the 100-year history of our research on change might lead us to agree with Twain. We began this century with an elegant theory of change, albeit one driven more by how we would have liked the wild world to operate than one informed by real data. Today, the talk is about temporal and spatial complexity, chaos and uncertainty, and we ecologists (the

discipline accused of calling a shovel a geotome) call this progress. I personally prefer Thomas Huxley's take on this history—what he called the tragedy of science; “the slaying of a beautiful idea by a simple fact.” It is probably true that ecologists today are less certain than some of their forebears about exactly how wilderness ecosystems function, but our fundamental understanding of that function has improved by orders of magnitude. That the world is more complex than we thought should come as no surprise. That wilderness has shaped the evolution of our science should also come as no surprise. In the words of Aldo Leopold (1949), “all history consists of successive excursions from a single starting point, to which man returns again and again to organize yet another excursion for durable values. That starting point is wilderness.”

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