



Herb Borneman

ECOLOGY: A Personal History

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ABSTRACT

FH Bormann, based on personal experience, recalls 55 years of association with the field of ecology, including the forces that led him into the field, research, development of the Hubbard Brook Ecosystem Study, and clashes between ecology, policy, and politics. He concludes with thoughts on humankind's search for the quality of life and sustainability.

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¹Editor's note: For more than four decades, FH Bormann, Oastler Professor of Ecology Emeritus, School of Forestry and Environmental Studies, Yale University, has contributed to science, education, and policy in the field of ecology. He has played a central role in the development of ecosystem science and in the Hubbard Brook Ecosystem Study which in 1995 had produced more than 850 publications. He is a past president of the Ecological Society of America, a member of the National Academy of Science and the American Academy of Arts and Science, and a recipient of the Tyler World Prize for Environmental Achievement, the International St. Francis Prize for the Environment, and the Eminent Ecologist Award from the Ecological Society of America.

THE BEGINNING

History reconstructed by looking back over three quarters of a century has its dangers. It can suffer mortal wounds from failing memory and unconscious revisionism. Nevertheless, I embark on this personal history with the hope that it will reflect some of the forces that shaped ecology in those times and my role in the field.

Although we never truly know, we all wonder how we became what we are. To me, it seems my role in ecology has been formed by five personal characteristics: a respect for nature, an ability to identify significant research problems in the field, a love of experimentation, an innate tendency to think in holistic terms, and a sensitivity to the connection between science and society.

I was born in 1922, in an apartment high above Riverside Drive in New York City. My parents had come to America in the first decade of the twentieth century, my mother from the rolling Bakony Hills northwest of Lake Balaton in Austro-Hungary, and my father from the Hartz Mountain region of Bismarckian Prussia. My father made a career as a waiter in places such as the Astor Hotel, the Waldorf Astoria, and the India Club. My youth, most of which occurred during the Great Depression, was spent in semirural Westwood, New Jersey, a suburb about 12 miles northwest of New York City.

My mother was a somewhat shy woman with a deep love of nature. She shared with me the beauty of summer skies, the glory of sunsets, and the marvel of birds and flowers. That environment of appreciation for nature, and involvement at a very early age in gardening, contributed to my lifelong love of nature. When I was about 12, a friend gave me a ten-cent pamphlet on tree identification from the Cleveland Museum. By the time I graduated from high school, I knew how to identify scores of plants, a skill I continued to expand over the years, but one that has now declined to about my eighth-grade level. Over the course of five decades, I visited scores of American ecosystems, often led about by local investigators, to get a better idea of their structure and function, and just for the fun of it. My father had little advanced education, having left Prussia at age 18 to avoid the draft. He was, however, an avid reader, and our house was filled with lectures and discussions on history, current events, and of course the Great Depression, and the numerous scoundrels then exploiting the working man. We avidly followed that period's crop of populists: Huey Long, Francis Townsend, and Father Coughlin.

Growing up in that home environment during the tumultuous 1930s made me acutely conscious of social justice. I read the muckrakers: Upton Sinclair, Frank Harris, Ida Tarbell, and Lincoln Steffens. I spent time trying to fashion theoretical utopian societies. These activities contributed to my lasting concerns about social justice and gave me practice thinking about complex systems. Our

family was too poor to consider higher education; all of my siblings went off to work immediately after high school. I too followed that track and spent about 15 months after graduation working at a variety of jobs—in the National Youth Administration (NYA), weekends at the A & P (then on its way to becoming a supermarket), shoveling snow and polishing cars—and playing an awful lot of softball.

During a marathon walk, Eby Fincher, one of my former high-school teachers, urged me to go to college and suggested it would not be expensive to attend a land-grant college in one of the western states. In September of 1941, I found myself in Moscow, Idaho, at the University of Idaho. The cost was \$80 for out-of-state tuition, \$40 for books, and \$200 plus one week's work out of every six for room and board. What a deal! This opportunity was due to the wisdom of the US Congress and the Morrill Act of 1862. At this time of Congress-bashing, we forget the many good things our government has done.

At Idaho, my real connection with professional ecology began. The general botany course, which was heavily ecological, was taught by Rexford Daubenmire, the first of three plant ecologists, trained by WS Cooper of the University of Minnesota, who contributed to my ecological education. All were heavily influenced by the writings of Frederic E Clements, the outstanding ecological theorist of the first half of the twentieth century.

On December 7, a group of us were listening to a University of California football game, when suddenly an announcer blurted, "Pearl Harbor has been attacked by the Japanese." One of our student companions was an American of Japanese descent, and the announcement was followed by an awkward silence, a kind of tension that seemed to question the patriotism of our fellow student. I have always regretted not having spoken out to affirm my belief in his loyalty.

Several months later, I volunteered for the US Navy. My request was to be assigned to the medical corps, but I ended up as a shipfitter. Pretty close by Navy standards, both jobs involved repair, one of human bodies, the other of ships. After about a year in the submarine repair base at Pearl Harbor, I spotted a small card on a bulletin board announcing a competition for appointment to the V-12 Officer Training Program. On a lark, I took the exam and about three months later found myself at Princeton University. Because of my one semester at Idaho, I was considered an irregular student, which meant that beyond a set of naval science courses, I could fashion whatever kind of academic program I wanted. Once again, unbelievable!

Although I took an array of science courses, I focused on history and political science, with a view to becoming a political science major. However, the deeper I got into these fascinating fields, the more I decided I was not suited to them. They required immense amounts of reading, a prodigious memory, and

a capacity to pull together vast numbers of unrelated facts without the aid of rigorous hypotheses.

RESEARCH

Clementsian Ecology

When World War II ended, I decided my future lay in science, in which facts are more tangible (and less long-winded), where information can be obtained by repeatable experiments, and theories such as evolution provide guidelines for assembling information. In 1946, I enrolled at Rutgers University, aided by another piece of thoughtful legislation that helped educate millions of Americans, the G.I. Bill.

In 1947, I came under the influence of Murray Buell, another Cooper student. Murray was a tremendous teacher of field ecology, and our plant ecology field trips covered the entire state of New Jersey. His unifying theory was Clementsian succession and climax, organizing hypotheses that I found attractive. For many ecologists, this time (the late 1940s and the 1950s) marked the beginning of the end for the dominance of Clementsian thinking. Clements' ideas of climax were coming under attack, his system of community classification was rigid and underestimated continuous change, and his boundaries of vegetation units were vague. His concept of the community as an organism received particularly strong criticism.

But I particularly valued Clements' regional overview, in which interactions of topography, elevation, geology, and landscape history gave rise to various environments and produced variations in the regional plant communities. Over the course of decades, I have found his approach useful in understanding regional vegetation, but what I most valued about Clements was his emphasis on processes of vegetation change, biophysical factors that produced community development. This dynamic approach to ecology was the kind of thinking that promoted the development of hypotheses and the observational and experimental means to test them; for me, this thinking moved ecology from the realm of pure description into the realm of experimentation. At Buell's suggestion, I migrated to Duke University to seek a Ph.D. under Henry Oosting, another Cooper student, who was about to publish *The Study of Plant Communities*, one of the outstanding plant ecology texts of the next two decades. Oosting, using Clementsian methods and an abundance of Piedmont farms abandoned over several decades, put together his classic study of plant succession, "An Ecological Analysis of the Plant Communities of Piedmont, North Carolina" (1). Oosting had identified a highly repeatable sequence of successional changes: horseweed → aster → broomsedge → loblolly pine → hardwood tree species.

I questioned why sweet gum, a fast-growing hardwood tree, which seemed to have many of the ecological attributes of loblolly pine, generally failed to enter the sequence except as an understory tree about a decade after the establishment of pine. For my Ph.D. dissertation, using observation and experiments, I carried out a comparative study of the survival, growth, and reproductive strategies of loblolly pine and sweet gum to explain the pattern that Oosting had observed. Differences in drought resistance during germination and early establishment and a great disparity in the availability of pine and sweet gum seed led to the pattern Oosting reported. That study, "Factors Determining the Role of Loblolly Pine and Sweetgum in Early Old-Field Succession in the Piedmont of North Carolina," (2) won the George Mercer Award of the Ecological Society of America in 1954. During the 1970s, two of my students, Peter Marks and Douglas Sprugel, also won the Mercer Award.

Individuality in Plants

Although botanists are well aware of intra- and interspecific connections between plants, such as those of symbionts or members of a clone, the relationship among trees is often thought to be one of competition between individuals. In 1952, while taking an advanced course in plant pathology at the University of Minnesota, Lake Itasca Field Station, I was introduced by Professor Clyde Christensen to a disease of oaks, called oak wilt, that is transmitted through root grafts (vascular connections that develop between two previously unconnected roots). Upon examining grafts between individual trees, I puzzled whether they could also function to alter an individual's ecology. Specifically, could grafts promote cooperation among individuals by transmitting water, minerals, and food? Could they influence ecosystem development?

Thus started about 10 years of research on transfer of materials between plants through intertwined roots and root grafts. My first approach to this problem was experimental. At Dartmouth College in 1956, I devised a technique whereby I could grow tomato plants, called donor plants, with two distinct root systems. One root system of a donor plant was planted in container A, while the other root system was planted in container B, along with the roots of an experimental tomato plant. After the plants were all well established, during a treatment period of 7 or 8 days, moisture was added to container A but withheld from container B. Experimental plants were assayed for resistance to wilting and speed of recovery from wilting. Using an experimental design in which experimental plants were planted alone or with one to three donor root systems, I demonstrated that experimental plants without donors wilted rapidly while those in contact with donor root systems obtained water in sufficient quantity to delay, or in some instances prevent, wilting of relatively large plants (3). Other experiments showed that radioisotopes could be transferred from donor

to experimental plants. Roots in these experiments were only in close contact and not grafted. These experiments demonstrated a potential mechanism for cooperation between plants. For example, a plant with a portion of its root system in moist soil and another portion in dry soil might transfer water to another plant whose roots were commingled with its own in dry soil.

My study of root grafting under field conditions, begun in 1958, focused on *Pinus strobus* L., eastern white pine. I chose white pine around Dartmouth because this species occurred in more-or-less pure stands of many ages and under a wide range of site conditions. The research involved anatomical studies to determine how grafts were formed in the first place and to ascertain that true vascular connections existed between roots of different plants, and excavations to expose root networks and intraspecific grafts. Dozens of tons of earth were removed to expose roots. Finally, we used tracers, dye radioisotopes and herbicides, to detect unions and to demonstrate their functionality.

Root grafting was found to be common among trees of all ages and on all sites, ranging from bogs to dry sites; often sizable clumps of trees are organically united by an extensive complex of grafts. We demonstrated that grafts are functional, through the transfer of dyes and radioisotopes and of food from living trees to living stumps; we found that roots of dominant trees often "adopt" roots of smaller trees with tops that are no longer functional. This process gives rise to multigenotypic unions which are common in many white-pine communities.

This research (4) indicated that the role of intraspecific competition in determining which trees in a developing stand will survive was overrated. In many stands, many trees are linked into organic unions. The relationship among members of a union is not strictly one of intraspecific competition among plants occupying the same area and dependent on the same site factors. Participants in the union have lost at least part of their individuality and are subject to the biochemical and biophysical influence of their grafted companions and to transfer of disease organisms. A tree's ability to preempt materials being translocated within the union may determine whether it will persist as a visible member of the stand or succumb to more vigorous companions, leaving a part of its root system to participate in the living union.

Literature review, coupled with many personal observations, indicated that intraspecific root grafting is a common phenomenon in many species (5) and needs to be considered in working out strategies of ecosystem development or in designing silvicultural management plans.

Origin of the Small Watershed Idea

At Duke, our emphasis had been on the abstract plant community, built from the bottom up. We used quadrat analysis, soil pit information, and data from

chronosequences (older stands in the sequence were assumed to represent young stands in a more developed state) to describe the composition, structure, and development of abstract plant communities. Estimates of stand processes such as primary production, transpiration, and photosynthesis were relatively hard to come by, and often these were estimated from small subsamples blown up to represent stand level. Although these approaches, particularly dimension analyses, were very useful, I felt ill at ease with estimates such as stand photosynthesis that were based on a single-leaf measure increased to stand proportions. These estimates were too static, and they poorly represented the seasonal range of environmental conditions experienced by the stand. The opportunity for error seemed enormous.

Because of the close relationship between ecologists and foresters at Duke, ecology students were exposed to both theoretical botany and applied botany, in this case forestry. To this day, I feel strongly that ecologists need exposure to both applied and theoretical aspects of ecology in order to be prepared to link ecological science to the solution of human problems. Unfortunately, in many biology departments, young instructors and graduate students learn that applied studies and linking science with social problems are not a sure road to tenure. Through the botany-forestry relationship, I was introduced by Professor Ted Coile to forest hydrology studies at the US Forest Service (USFS) installation in Coweeta, North Carolina. There, with the use of monitored small watersheds, research was under way on the hydrologic input and output of whole, i.e. intact or experimentally manipulated, forests.

Coweeta was, for me, like discovering the Rosetta stone. Watershed studies allowed quantitative measurements of precipitation entering a defined forest, runoff leaving the forest, estimates of evapotranspiration, estimates of erosion, and experimental measurements of the effects of landscape manipulations on these parameters. This was truly a top-down way of studying whole stands. When combined with the well-established bottom-up methods already described, this approach seemed to offer an order of magnitude increase in the power of ecological research. Although I did not appreciate the potential at the time, watershed measurements provided both topographical and physiological boundaries for scientists attempting to model stands from the bottom up. For someone trying to model whole-stand transpiration from measurements of individual leaves and estimates of whole-stand leaf area, watershed measurements of evapotranspiration provided a realistic checkpoint, estimated evapotranspiration in liters hectare (ha)-yr⁻¹. One of the surprising aspects of that time was the lack of communication between the fields of plant ecology and forestry as seen through the eyes of such notable forest scientists as M Hoover, J Kittridge, and EA Colman.

When I became a teacher at Emory University in 1952, forest influences and hydrological studies were a central piece in my plant ecology course, allowing me to connect bottom-up and top-down approaches to ecology and to join the science of ecology to problems of human welfare, particularly problems related to forestry, agriculture, and urban sprawl. For example, we could discuss the impact of forest cutting on ecological parameters, such as transpiration, runoff, and erosion and social parameters, such as availability of water for human use. Every fall, my class and I trekked north through the Georgia foothills studying vegetation zonation and changes in soils, and at Coweeta examining the work in progress by scientists of the USFS.

Shortly after my arrival at Emory, Gene Odum's book, *Fundamentals of Ecology*, was published. This book presented an approach to ecology quite different from the one I learned at Duke. The ecosystem was the central focus, and in a global way the book emphasized nutrient cycles. Although my major research at that time concerned photosynthesis and its relationship to ontogenetic leaf changes in southern pines (6), Odum's book started me thinking about nutrient cycling.

In 1956, I joined the Botany Department at Dartmouth College in Hanover, New Hampshire, where the cloudy, cool summer of 1956 brought more than a bit of longing for some of that Georgia heat. I soon began the intensive research on plant individuality and the intra- and interspecific transfer of materials between individual plants previously discussed. In my teaching I continued to emphasize ecology and forest influences. In fact, I designed a course that was wholly about forest influences, called Forest Science, irreverently named by the students "Twigs 12." Through continuing interest in forestry and new contacts made in New England, I met Victor Jensen of the USFS, who in 1957 introduced me to the newly established hydrologic studies at the Hubbard Brook Experimental Forest (HBEF). About 1959, under the initial guidance of Dr. Dick Sartz of the USFS, I began field trips to Hubbard Brook with classes from Dartmouth. These trips continued with the cooperation of Dr. Robert Pierce, the new USFS program director for Hubbard Brook.

By 1960, Forest Service scientists Bob Pierce, Richard Sartz, George Trimble, and Howard Lull had a vigorous hydrology research program in place. Hubbard Brook was judged to be almost ideal for hydrology research because of its well-defined watersheds and its relatively impermeable bedrock overlain by fairly impermeable glacial deposits, which guaranteed input-output measurements not clouded by excessive deep seepage losses.

In 1953, I had the good fortune to spend the summer north of the Arctic Circle in Alaska with a Boston University research group, under the scientific direction of John Cantlon, studying the relationship between tundra plant communities and permafrost. Professor John Tedrow, a soil scientist from Rutgers and a

member of the team, made the major discovery of a new great soil group which he termed Arctic Brown. Somewhere in the mid-1950s, the thought occurred to me that Alpine tundra on top of Mount Washington, New Hampshire, might be underlain by a montane analog of Arctic Brown. After a visit to the site, Tedrow concluded it was worth a soils study. He enlisted a Ph.D. candidate, Henrich Harries, and we were able to arrange for him to work out of Dartmouth, capitalizing on its proximity to Mount Washington.

One summer day, Henrich and I were ascending Mount Moosilauke, about eight miles west of the HBEF, vigorously debating the recently aired European hypothesis about nutrient flushing: namely, that water entering high elevation soils would leach nutrients downslope, thereby enriching lower slopes and contributing to enhanced productivity. As we walked beside Gorge Brook arguing the possibilities of nutrient flushing, it occurred to me that nutrient-rich water moving downslope would not deposit all of its nutrient load in lower-slope soils, but would pass some on into drainage streams. At that moment, my years of thinking and teaching about the hydrology of small monitored forested watersheds came together. If streams were draining the watershed of nutrients, by measuring streamwater nutrient concentrations and flow rates, it would be possible to estimate nutrient losses for a whole watershed. Inputs could be estimated by multiplying nutrient concentrations in rain and snow by quantities of water entering the watershed. By measuring the chemistry of input and output water, it would be possible to construct nutrient budgets for whole ecosystems. Not only could top-down hydrologic budgets be obtained for whole ecosystems, but, it seemed reasonable to think, the same could be true for biogeochemical budgets. Thus was born what has come to be known as the small watershed technique for the study of biogeochemistry.

In November of 1960, I wrote to Robert Pierce, USFS program director for the HBEF.

Dear Bob,

The other day while discussing the problem of mineral cycling through ecosystems, the thought came to me that your installation at Hubbard Brook represents a veritable research gold mine in regard to fundamental studies on mineral cycling.

One of your small watersheds with a weir at the outlet represents a perfect area for controlled research. If one were to select one to several minerals, such as K^+ , it would be possible, by taking weekly water samples and analyzing them, to determine quantitatively the amount of K^+ leaving the system. Weekly estimates of K^+ per liter multiplied by the liters of water leaving the watershed would give the quantitative figure. Since the watershed is theoretically tight all the water falling on the shed appears at the weir (excepting evaporation which would not remove any minerals), [thus,] the quantitative figure would represent total loss of K^+ from the watershed (excepting leaf litter blown out, or presumably this would be counterbalanced by leaf litter blown in. The argument goes for other losses and additions due to animals, etc.).

Some minerals may be added by rain or snowfall, therefore both rain and snow would have to be analyzed for the mineral(s) in question. These analyses multiplied by the amount of rain or snow would give the total amount of mineral(s) added to the system.

By subtracting the total amount added from the total amount lost, it would be possible to estimate steady-state losses from the system. Theoretically the only place these minerals could come from is the underlying parent material and bedrock. Thus, the loss represents the rate at which bedrock is wasting away in terms of the mineral(s) under consideration. By knowing the chemical composition of the bedrock, it would be possible to determine the rate at which it is breaking down.

This figure would seem to be of considerable consequence because it would quantify the rate of erosion, it would shed considerable light on the rate of soil formation, and it would tell something about the rate at which minerals useful to plant growth are added and lost from the system. The latter might lead into further studies of how treatments affect mineral cycling patterns.

I consider the small watershed technique outlined in the letter to be the product of many forces: (a) a kind of system-thinking that emerged from teen-aged efforts to devise utopian social systems and which I generally apply to all problems; (b) a respect for the dynamic and interactive nature of plant communities as espoused by the Clementsian ecologists Murray Buell and Heinie Oosting; (c) training in the philosophy and use of the experimental method by Paul Kramer; (d) many discussions about the nature of ecology with George Woodwell and Philippe Bourdeau; and (e) the inestimable contribution of forest scientists such as EA Colman and J Kittredge.

The Birth of the Hubbard Brook Ecosystem Study

Robert Pierce encouraged me to go ahead with the implementation of the small watershed technique, but it was not an easy path. Biogeochemistry was an entirely new field to me, and I was not particularly well prepared in chemistry or geology. To proceed I needed partners with other skills. I explored several avenues with negative results. I tried without success to interest a young stream biologist with some expertise in chemistry. I approached one junior and two senior geologists at Dartmouth, all well-respected scientists. After a day at Hubbard Brook when I explained the potential of small watershed biogeochemistry for the study of weathering, they were encouraging, but had little time to commit. One of them, Robert Reynolds, who was on his way to becoming a renowned clay mineralogist, oversaw the first biogeochemical study, a study of iodine cycling in 1962. Some 30 years later, Bob told me that his decision not to join the Hubbard Brook study was a major misjudgment.

In 1961, the idea of a small watershed study of biogeochemistry rested in a drawer in a filing cabinet, perhaps on its way to the dead letter department, when one of those small miracles happened. In the autumn of 1961, the Department of Zoology at Dartmouth hired a temporary instructor, a young Ph.D. candidate

from the University of Wisconsin, a student of Arthur Hasler. That person was Gene Likens. Likens and I found much common ground, we enjoyed each other's company, we found time for numerous explorations of field ecology, and we exchanged many ideas from our very different positions within the field of ecology: plant ecology and limnology. He worked with whole systems, the circulation of small lakes, and he came to Dartmouth with a sense that aquatic and terrestrial systems should be fused into a single landscape unit. His studies of the chemical stratification of lakes were a form of biogeochemistry. It was apparent to me that his training and point of view were a complement to my own training and experience with terrestrial ecosystems and my years of theorizing and attempting to understand the nature of ecosystems.

I proposed that we join forces on the small watershed approach to the study of biogeochemistry. He readily accepted, despite the considerable chance that studying the biogeochemistry of forests might not be a stepping stone in a career in limnology. Gene soon rectified that problem by launching studies of forest-stream ecology and of land-water interactions using the hydro-chemical output data from forests as inputs to 35-acre Mirror Lake, embedded in the Hubbard Brook forest. In 1961, Likens and I began planning a grant proposal to the National Science Foundation (NSF). Likens did something we both have done repeatedly during our 34-year association. Seeing a need for expertise, he brought a bright young geochemist, Noye Johnson, newly arrived at Dartmouth, into our planning. Likens returned to Dartmouth in 1963 as a faculty member, and in that year, Bormann, Likens, and Johnson received the first of 32 years of continuous NSF grants to Hubbard Brook.

These days there is much talk about interdisciplinary research and stretching research dollars. These objectives were built into the Hubbard Brook Ecosystem Study (HBES) from the very start. The initial cooperators included a hydrologist/soil scientist, limnologist, geochemist, forester, and plant ecologist; two institutions, the USFS and Dartmouth College; and funding by the NSF and the USFS.

The USFS owned and operated the HBEF; designed, built, and operated the weirs and meteorological stations; stored the hydrologic data; and maintained a staff of professional scientists and research assistants. Without their science and cooperation, the HBES would likely never have been created. Mounting the chemical side of the HBES was a formidable task. We were faced with designing and executing an extensive sampling program that included not only the pleasant New England summers (excepting the millions of black flies), but also extremely severe, cold, and relentless winters. Then there were the problems of chemical analyses: potentially thousands of sample preparations, wet chemical analyses, and sample storage. Once again, luck came into play.

Faced with these problems, I called John Tedrow to inquire about the possibility of farming out samples to Rutgers. John advised us that their chief analytical chemist, "Tiny" Grant, had just moved to the University of New Hampshire and that we should consult him. So one hot summer day, Gene and I drove across the state and met with Tiny, who was anything but tiny. He was helpful, but somewhat discouraging. However, shortly before our departure, he recommended that we get in touch with the Perkin-Elmer Corporation. As a consequence, the HBES acquired the fifteenth Perkin-Elmer Atomic Absorption Spectrophotometer sold in the United States. Our luck in acquiring the latest analytical tool probably increased our analytical capacity one or two orders of magnitude and reduced the possibility that analytical fatigue would have ended the HBES after a few years.

Due to the 60-mile distance between Dartmouth College and Hubbard Brook and our need to have closer laboratory and living facilities, we rented Pleasant View Farm, a 200-year-old farmhouse, from Mrs. Henrietta Towers. Pleasant View played a central role in the science and character of the HBES.

The farmhouse was large, with eight bedrooms, a big kitchen and dining room, two so-called bathrooms, a boulder-walled cellar, and an attached barn. To meet our needs for preliminary processing of water, soil, vegetation, and faunal samples, we cleaned an attached shed, as best one can clean a 200-year-old structure, built some laboratory benches out of plywood, and installed some rudimentary equipment: drying ovens, a deionizer, balances, pH meters, and various other devices. To achieve reasonably moderated temperatures, Likens constructed a lab in the cellar affectionately called the pit. Later in our first decade, when the HBES had achieved some notoriety and visitors were coming from throughout the country and the world, we would give them a standard tour of the watersheds ending up in our "laboratory facilities." Wide-eyed, they would gingerly descend into the pit and poke around the shed, and one could see the unstated question written across their faces: "This is it?"

Pleasant View played an equally important role in promoting interdisciplinary thinking. For entire summers, Pleasant View was home for up to 18 persons: scientists, Ph.D. candidates, and senior technicians. They came from all over the country and the world, represented both sexes, many cultures, and various scientific disciplines. They cooked for each other and ate together, they played together, they laughed together, and some even loved together. The atmosphere was one of almost constant interchange of thoughts about everything, including science. New scientific projects involving cooperation between totally different disciplines emerged. For a time, both Likens and I lived at Pleasant View, but we were not up to parenting and soon learned that we were inhibiting factors. We moved out to let the ferment proceed.

Many group activities were organized, and there was a strong interest in sports including an annual deep-sea fishing expedition. For years I considered myself the sports impresario organizing softball, volleyball, and bocci games. One of my great satisfactions is my perception that those young scientists who worked and played at Hubbard Brook and matured at Pleasant View Farm had a truly exciting and marvelous experience, one they will remember all their lives.

Everyone knows that integrated research requires management skills. From the start, we were acutely aware of this and the potential for discord to disrupt research efforts. To forestall problems of this nature, a scientific advisory committee (SAC) composed of Robert Pierce, Likens, and myself was formed. Early on, we formulated four management principles: All researchers should be supportive of the ecosystem concept, all researchers should have as much intellectual freedom as possible within the frame of integrated research, all must respect the Forest Service rules governing the site, and all must be open with other researchers and share data with the understanding that researchers had first use of data they accumulated. To ensure that these goals were met, research proposals were reviewed, scientists were engaged in conversation, rules of engagement and data sharing were discussed, and, within the boundaries of ecosystem study, individual research initiative was encouraged. Our position on individual initiative was influenced by a strongly negative response to the management strategy of one of the International Biological Programs (IBPs) in which research was designed by a directorate and assigned to research associates for execution.

In the early years, the HBES grew by a process of reaching out, seeking new researchers and new ideas. The development of the HBES was under almost constant review by the SAC. Discussions covered work proceeding in other ecosystem studies that might be appropriate at Hubbard Brook and research needs perceived from an analysis of our own situation. From this process, we compiled a great array of research ideas appropriate for Hubbard Brook; far more than we could ever hope to implement. To partially fill our needs, we and Forest Service scientists initiated studies, and graduate students from Dartmouth, and later Yale and Cornell, carried out dissertation and thesis work. We also contacted well-established scientists working in appropriate areas and invited them to join the HBES. In this way, Robert Whittaker, Donald Fisher, Arlo Gambell, Dick Holmes, Frank Sturges, Margaret Davis, Tom Ledig, Tom Winter, William Smith, Charles Driscoll, Philip Johnson, and Garth Voigt, to name a few, joined us. After a time, we began to receive unsolicited requests to do research at the Brook. By 1983, after two decades, several dozen senior investigators and scores of graduate students worked at Hubbard Brook, approximately a half-dozen governmental agencies and private foundations supported

research, and 373 papers, 169 abstracts, 36 Ph.D. dissertations, 28 Masters and honors theses, two audio tapes, and two books were published (7). Research at HBES received a considerable airing in the press and on national television and through numerous lectures by senior investigators and appearances before congressional committees.

The power of the small watershed technique for the study of biogeochemistry was seen by its adoption on almost every continent of the world. According to Frank Golley in his book, *A History of the Ecosystem Concept in Ecology* (8, p. 151), "The Hubbard Brook project served as a model ecosystem project for many investigators, and it continues this role today, not only as an integrated ecosystem project, but also as a part of the long-term ecosystem research program of the NSF. This means that the more conventionally scientific approach at Hubbard Brook has become the norm of ecosystem projects rather than the exception."

On some occasions, Bormann, Likens, and Pierce played a more aggressive role in promoting scientific projects deemed central to the HBES. In 1969, Lawrence Forcier and FH Bormann presented a paper called "An intraspecific energy flow model for tree populations in mature forests" (9), based on Hubbard Brook data. Thomas Siccama, starting from this model, built a forest growth simulator using population data from our reference watershed. His simulator predicted how the forest would grow over time. About this time, there was great emphasis among IBP groups on modeling whole-ecosystem behavior. Likens and I decided that the HBES needed a modeling component, but we felt that it should be more mathematical than the Siccama model. After discussions with Siccama, we approached Dan Botkin, a young assistant professor at Yale. At the same time, Likens had been sought out by an IBM research group in Yorktown Heights, New York, interested in working with natural systems. A meeting was arranged at Yale, and Tom Siccama and Dan Botkin collaborated with IBM scientists JF Janik and JR Wallis in a project to create a forest growth simulator. Siccama soon dropped out, and Janik, Botkin, and Wallis went on to create the now world-famous JABOWA forest growth simulator (10).

Another example of the role we played comes from the North American Cloud Water study. Tom Siccama placed a cloud water sampler (developed at the University of Vermont) atop Mount Lafayette, a 5100-ft peak near Hubbard Brook. Cloud water deposited on the collector was significantly richer in ions than rain water. Cloud water clearly had the potential to be a major nutrient input to the Hubbard Brook watersheds. Gene and I soon initiated a comparative cloud water study. A new and more efficient cloud water collector was developed by Kathie Weathers and Bruce Daube, a student at Worcester Polytechnic Institute, and cooperators were sought throughout North America and the Caribbean

area. The study was funded by the AW Mellon Foundation. Our data (11) indicated that, although cloud water directly impinging on vegetation was relatively unimportant at Hubbard Brook, it was very significant at higher elevations and in particular regions of North America, and in fact was an important component of the acid rain problem. Our North American Cloud Water study was one model upon which an important national cloud water study was later based.

Although it is not the purpose of this autobiographical sketch to detail the science of the HBES, (see 8 and 12 for historical accounts), a discussion of some of the ways the HBES changed ecological thinking is appropriate.

In our first paper in *Science* in 1967 (13), we proposed the use of small watershed ecosystems for the study of nutrient cycling, and we set forth our nutrient flux and cycling model. This model not only proposed four compartments within the ecosystem and enumerated flux rates between them, but it emphasized that inputs to and outputs from the ecosystem were part of larger biogeochemical cycles. The underlying theme was that the structure and function of ecosystems could be influenced by naturally occurring and man-made inputs of advertent and inadvertent origin, and that advertent or inadvertent activities within the ecosystem could influence interconnected systems through outputs. In a useful way, the model linked the small ecosystem with the greater biogeochemical cycles of the earth and made clear that air pollution originating in Ohio, for example, could affect forests in New Hampshire, or that forest cutting in New Hampshire could affect water supplies and water quality in Massachusetts. The model also featured emphasis on processes like weathering, erosion, and secondary mineral formation, processes usually neglected in other ecosystem models. Our focus on the flux of chemical nutrients and water created intellectual links between ecology, geochemistry, forestry, soil science, hydrology, and atmospheric science; a great expansion of the then-typical ecological focus on biological processes alone within the ecosystem.

One of the nettling problems for modern ecosystem science is that of boundaries. How does one study the boreal forest ecosystem, the North Sea ecosystem, or an ecosystem defined by the range of a grizzly bear? Where are the boundaries? How do we tie data to unit surface of the earth? How do we tie the system under consideration to the rest of the biosphere?

Not only did our use of small watersheds settle the boundary issue for us by using a natural unit of landscape, but it simultaneously provided a means for measuring the relationship of that unit to the rest of the landscape, as well as providing scientists working at subecosystem levels within the watershed-ecosystem with previously unavailable information. As I mentioned earlier, physiologists working at the single-leaf level were provided with whole-system transpiration rates, as were whole-system hydrologic modelers, ecologists working with

intrasystem nutrient cycling, and soil scientists concerned with soil development. Geologists concerned with weathering were given quantitative chemical input, output, and budget data. Animal ecologists could evaluate animal impacts on nutrient cycling, and so forth.

In 1977, with the publication of *The Biogeochemistry of a Northern Hardwood Forest Ecosystem* (14), Likens et al summarized 11 years of research, presented an expanded nutrient flux and cycling model, quantified both hydrologic and nutrient input-output budgets, examined the representativeness of Hubbard Brook hydrology for New England as a whole, presented flux and cycling data for the calcium and sulfur cycles, put forth a new way of calculating weathering rate that included nutrients stored in accumulating living and dead biomass, demonstrated that ecosystems can act as filters on biogeochemical flows, and estimated the relative importance of chemical (dissolved substance) and physical (particulate matter) erosion in lowering the ecosystem in place. Hubbard Brook was positioned in a world spectrum of nutrient budgets by an intensive literature review. . *The Biogeochemistry of a Northern Hardwood Forest Ecosystem*, now in its second edition, illustrated that ecosystem control over biogeochemical functions is highly predictable and, in intact systems, relatively repeatable from year to year. The book is widely used as a text for courses in biogeochemistry.

In 1965, we designed the first of our whole-watershed ecosystem experiments. Using our nutrient flux and cycling model for guidance, we decided to block a major flux pathway—nutrient uptake by green plants—and to determine the fate of nutrients that previously would have followed that path. To block the uptake pathway, we clear-cut the forest on Watershed-2 (W-2), but left all fallen trees in place. We did not want to introduce harvesting disturbance into the experimental design. Later, herbicides were applied to destroy remaining growth. W-2 was held bare for three years and then allowed to regrow.

We had expected major increases in hydrologic runoff, but the great flush of nutrients leaving the ecosystem was a total surprise. As it turned out, our experimental treatment affected the microbiology of the system and turned on the microbial process of nitrification with a greatly accelerated loss of nitrate and cations. Cations, such as Ca^{++} and Mg^{++} , were displaced from exchange sites by hydrogen ions generated in the nitrification process. This finding immediately stimulated nationwide criticism of clear-cutting discussed below.

The W-2 experiment yielded a wealth of information on how biogeochemical processes are affected by disturbance, clear-cutting, and revegetation. This information, coupled with ecological studies on nearby chronosequences of cut over northern hardwood sites, and projections by the JABOWA forest growth simulator, allowed us to develop a theoretical growth model.

The Hubbard Brook Biomass Accumulation Model was presented in our second book, *Pattern and Process in a Forest Ecosystem*, and became the centerpiece of the HBES (15). The model integrated, over time, biomass accumulation, biogeochemistry, energy flow, succession, species behavior, and the role of forest gaps. It presented a new version of steady state, the shifting mosaic steady state. The steady state was not a rigid Clementsian climax, but rather a collection of varying-age patches resulting from random local disturbances, mostly tree falls. Although spatially heterogeneous, the steady state as a whole would exhibit some forms of equilibrium: Over time, biomass would remain roughly the same, photosynthesis would approximately equal respiration, and hydrologic and nutrient inputs and outputs would remain roughly the same.

Although the Biomass Accumulation Model was influenced by Gene Odum's 1969 article, "The Strategy of Ecosystem Development" (16), our model (15) contradicted some of his basic conclusions: Ecosystem development is not well characterized by an asymptotic curve. Rather, biomass accumulation reaches a maximum about the midpoint in ecosystem development and then declines in maturity. During two phases in development, ecosystem respiration exceeds ecosystem photosynthesis, and the system operates at an energy deficit, but makes up the difference by drawing on energy reserves stored in nonliving biomass. Although drawing conclusions about species diversity is always hard, because we never have information on the full species complement of an ecosystem, our limited species richness data questioned Odum's conclusion that species diversity increases as ecosystem development proceeds.

A final and major point about the model: We proposed that biogeochemical responses to disturbance, accelerated losses of water and nutrients, and loss of temperature control are also components of a complex biophysical feedback mechanism that allows the ecosystem to recover rapidly from disturbance. We proposed that exploitive species, like pin cherry and trembling aspen, had evolved reproductive and growth mechanisms to take advantage of these temporarily enriched conditions.

The Sandbox Experiment

The small watershed technique had other applications. Tim Wood and I used it to evaluate the effect of simulated acid rain on experimental ecosystems made up of small pots containing soil of known chemistry and seedlings of northern hardwood species. We evaluated the effect of various strength acid rains on nutrient loss from the ecosystem, leaching from foliage, growth rates, and productivity (17).

In a nitrogen budget constructed for an intact forested watershed ecosystem at Hubbard Brook (18), we identified an unknown input to the system which

we believed to be symbiotic nitrogen fixation. Repeated measurements utilizing acetylene reduction techniques failed to identify a nitrogen input sufficient to fill the projected input. In frustration, I decided to examine the capacity of experimentally constructed mesocosm-watershed ecosystems to accumulate nitrogen. Over the course of several months, a group of professors and graduate students including Garth Voigt, Bob Pierce, Dave Smith, Deane Wang, Breck Bowden, Steve Hamburg, and Tim Wood struggled to design an input-output mesocosm-ecosystem capable of detecting significant inputs of nitrogen. We finally settled on small ($2.5 \times 2.5 \times 1.5$ m, or $7 \times 7 \times 1.5$ m) lined pits filled with sand of known low-nitrogen content and planted with seedlings of local origin with and without biological nitrogen-fixing capacity. These pits became known as the sandboxes. Estimates of nitrogen entering the sandboxes in rain and snow were taken from the Hubbard Brook precipitation monitoring system, and outputs were measured using drainage water from several boxes. By knowing the starting quantities of nitrogen in the boxes, and inputs and outputs, after several years it was possible, by dismantling and analyzing the ecosystem, to determine if nitrogen content had changed significantly and whether or not that change could be accounted for by measured inputs and outputs.

After only four to five years, we found significant nitrogen gains that could not be accounted for by measured inputs (19). In the case of black alder and black locust, known symbiotic nitrogen fixers, we attributed those gains to symbiotic nitrogen fixation. To our surprise, we found high levels of productivity and nitrogen additions, about $50 \text{ kg ha-year}^{-1}$, in red and pitch pine ecosystems. Pines are not known to be symbiotic fixers. With the aid of acetylene reduction analyses, we concluded that those additions were due to associative nitrogen fixation, a process generally associated with grasses but not previously associated with *Pinus*. The finding that *Pinus*, the world's principal plantation genus, could not only be productive but also might simultaneously build the ecosystem's nitrogen reserves, was exciting. In this study, I had the pleasure of working closely with my son, Bernard, who joined the study in its later phases and played a major role in the final analyses and writing.

From the inception of the Hubbard Brook studies, I have considered weathering a paramount process, and in the HBES we devoted considerable effort to quantifying it. In 1993, we began a study using Sandbox ecosystems to estimate weathering in a more precise way. Knowing starting conditions and primary mineral mass and nutrient inputs and outputs, we believe we can estimate quantitatively the rate at which primary minerals weather into available nutrients and other weathering products. Preliminary results suggest that weathering occurs at rates much faster than previously estimated.

POLICY, POLITICS, AND A WIDENING CIRCLE

Many experiences shaped my current view of the relationship between science, politics, and policy. When I first entered science, I viewed the field as a bastion of objectivity and the scientist as a knight in shining armor, a totally dedicated seeker of truth. Today, science is still characterized by those virtues, but now I recognize that science is not some satellite floating in incorruptible space, but rather an ideal buffeted by the foibles of men.

In the late 1960s, a titanic debate was raging in the western states pitting the Forest Service and the forest industry against an increasingly strong environmental movement. The topic was clear-cutting of national forests. From 1968 through 1970, the HBES published three papers (20–22) documenting and explaining the heavy loss of nutrients from our experimentally clear-cut forest and the fact that water draining the ecosystem might not be fit to drink. The nutrients were coming from the soil, and the prospect that future forest productivity might be endangered had to be considered. Others quickly entered our results into the debate, transferred them to western forests, and used them to support the argument against clear-cutting. In the eyes of clear-cutting advocates, we were responsible for these arguments, even though our papers were limited to Hubbard Brook and our advice was that potential nutrient losses in drainage water should be taken into account when designing forest management practices.

We received vitriolic letters demanding that we come to the defense of clear-cutting. Rumors reached us about deep unhappiness within the USFS; a program officer at the NSF told us that we would not be allowed to study forest cutting anymore. Apparently we were embarrassing another federal agency! We fought that dictate all the way to the NSF Director, had it reversed, and, in the process, helped to strengthen the principle that the NSF should stick to science (23).

In the long run, our studies had important implications for forest management policy: The best management is achieved through an ecosystem approach and nutrient cycling must receive adequate attention in preparing environmental impact statements. In the early 1970s, we stumbled into another controversy, acid rain. We found that rain and snow at Hubbard Brook were quite acid, and Likens found the same was true for New York State. In 1974, in *Science*, Likens and I published “Acid Rain: A Serious Regional Environmental Problem” (24). A week later, the results were described on the front page of *The New York Times*. For years afterward, our telephones rang constantly as reporters from everywhere looked for a story. We became particularly wary, for along with the reputable journalists there were more than a few looking for a sensational tidbit, a slip of the tongue, or some statement to misconstrue in order to sell papers or jazz up the evening news hour.

Today acid rain is recognized as an international problem. Tens of thousands of research articles have been published, and new policies for its control have emerged. We think the HBES has played an important role in that process. Our contribution began with the discovery of acid rain as a widespread problem in northeast North America and continued with the many studies done by Hubbard Brook scientists, but the major contribution was the long-term HBES record of precipitation chemistry. That record was used not only to document the problem, but also to demonstrate the effectiveness of regulatory controls put in place by the US Congress and to aid in the writing of a new Clean Air Act. These Hubbard Brook events reaffirmed that science does not exist in a vacuum, but earlier experiences had already prepared me for difficulties in dealing with bureaucracies, scientists with narrow vision, and the power of economic forces.

In 1957, my greenhouse experiments at Dartmouth, on mineral transfer between roots, were ruined when the control plants were contaminated with radioactivity. A random-grab sample of leaves from our garden indicated that all plants outside were radioactive. We called the New York Office of the Atomic Energy Commission, and they said not to worry, but immediately sent their second highest officer to Hanover. It turned out we had detected fallout from the 1957 atomic bomb tests in the Pacific. Those were days of great secrecy, and not a word on fallout appeared in the media. The great bulk of Americans had no idea what was drifting down from the sky. The officer told Paul Schaffer, Dave Mulcahy, and me the only problem was that a Geiger counter could fall into the hands of some damn fool. This gentleman was a guest in our house, so my pregnant wife made a point of serving him an especially large garden salad. Because of the paucity of information on fallout, a growing public concern about nuclear war, and the potential health effects of radioactive strontium in milk, we felt our discovery could be important. We expanded our fallout survey. Elm leaves, which are good dust collectors, were collected throughout New England, and an isoclinal map was prepared showing mixed-fission fallout everywhere and a large hot spot in southern Maine.

During this time, I gained unending respect for John Dickey, president of Dartmouth College. The fall term was about to begin, and a public announcement that the Dartmouth campus was blanketed with radioactive fallout had the potential to be very disruptive. We asked guidance from President Dickey. His approximate reply was: Gentlemen, if you are sure of your facts, be guided by your conscience.

We wrote a paper, "Fallout on the Vegetation of New England During the 1957 Atom Bomb Test Series," (25) and submitted it to *Science*. Despite its timeliness, we had no reply for months, and then it was rejected for what seemed to us the weakest of reasons. I suspected that rejection came because

the potential pool of reviewers was dominated by Atomic Energy Commission scientists and our “uncontrolled report” was thought too dangerous for the public to handle. We should have fought that decision, but not yet knowing the ropes, we published more than a year later in *Ecology*, where our study went largely unnoticed. I thought that *Science*, for whatever reason, had abetted the secrecy goals of the government. That experience has made me wonder how often special interests have diminished the “objectivity” of science, particularly when corporate and government bureaucracies have huge stables of scientists and lawyers to present their point of view.

Lucy Braun, in her classic 1950 book, *The Eastern Deciduous Forest*, described an extraordinary but small patch of “climax” sugar maple forest in Gifford Woods State Park in central Vermont. At this time, I had my first taste of how science is weighted in natural resource decision-making. In the mid-1950s, Murray Buell and I carried out an intensive study to document the ecology of this rare forest (26). Having learned that the state of Vermont was planning to construct a pond beside the forest, cut an adjacent secondary forest, and admit full sunlight into the interior of the old age forest, a deadly blow to the forest’s integrity, I wrote to the appropriate state official explaining the forest’s rare and unique nature and the devastation that would result from constructing the pond. He wrote back suggesting a meeting at Gifford Woods. I went there expecting to debate our scientific evidence. Upon arrival, I was surprised by dozens of cars and people milling about. Most were motel owners and businessmen from the local region. Our debate was brief, an early version of jobs against environment. The pond was built. Never again would I be so naive as to think that natural resource decisions are based only on good scientific evidence. Social and economic factors will override science, no matter how much research has been done, if the public is not educated to understand the relationship of that science to their own long-term welfare.

From 1974 to 1977, I sat on the Executive Committee of the National Academy of Science’s Assembly of Life Sciences. The Committee dealt with a wide array of both science and policy questions regarding subjects such as health, agriculture, and nuclear energy. The Committee performed a most important function and performed it well. However, one aspect of our work deeply troubled me. The Committee formed numerous subcommittees to investigate and report on a wide array of issues, many of which concerned the public. Invariably, members of a subcommittee were sought from three of what we might now call special interest groups: universities, government, and industry. Rarely were members sought among nongovernmental organizations: I had the sense that such persons were regarded as unqualified. More than once I came away with the question “Who speaks for the people?”

My concept of ecology in the late 1950s and early 1960s was greatly enlarged by growing public concern about potential environmental impacts of nuclear and chemical technologies. Potential effects of nuclear war, recovery from nuclear attack, and simply living in a nuclear world were wholly new drivers of ecological research. Many studies of radiation effects and cycling of radioactive nuclides were conducted, and major ecological programs, such as those of Robert Platt of Emory University, Stanley Auerbach at Oak Ridge, and George Woodwell at Brookhaven National Laboratory, were put in place. I closely followed these developments and spent a sabbatical year, 1963–1964, in Woodwell’s laboratory at Brookhaven. Woodwell and I spent many hours trying to think through the ecological implications of technological changes. Somewhat puffed up, we made a missionary trip around the country spreading the message to ecologists that “ecology is more powerful than ecologists” and that ecologists needed to emerge from their cocoon. We gained few converts, but many enemies.

At Dartmouth College, in 1962, robins were seen on the campus exhibiting traumatized behavior. I and my students avidly followed a study by Charles Wurster linking that behavior to ingestion of DDT. Wurster went on to become, along with George Woodwell, a founder of the Environmental Defense Fund. At that same time, Eby Fincher, my old high-school teacher, called my attention to an article in *The New Yorker* magazine by Rachel Carson, excerpted from her soon-to-be published world classic, *Silent Spring*.

Not only did I find many of these studies on radionuclides and pesticides to be fundamental ecological research, but I thought them extremely important contributions to humankind’s attempt to understand its relationship to the rest of the world. I incorporated these science and social science concepts into my teaching and, through public lectures and other communications, attempted to introduce them to a wider circle.

These efforts culminated, in 1969, in a lecture series, “Issues in the Environmental Crises,” convened by myself and Garth Voigt. In weekly lectures, speakers such as Stewart Udall, Clarence Glacken, LaMont Cole, Paul Ehrlich, and Kenneth Boulding explored the emerging dimensions of a world crisis in environment. The series received astounding attention from the Yale University community. The lecture hall was always packed, and eventually the series became a best-selling book (27) for the Yale University Press. The series and the ferment surrounding it contributed to the successful leap two young theoretical physicists, Rob Socolow and John Harte, were making to new careers in environmental science. Young lawyers struggling to invent environmental law at the Yale Law School attended. As a result of contact between Paul Ehrlich and Charles Remington of Yale, the organization Zero Population Growth was established.

In 1972, in the Past President's Address of the Ecological Society of America, I utilized information from Hubbard Brook research, the series, and other sources and focused on the problem of growth and overpopulation. I titled that address "Growing, growing, gone" (28).

Through the remainder of my career at Yale, I continued to use the lecture series as a mechanism to air issues in the interaction between ecological science and society. In 1975, a series designed by William Burch and I explored the limits to growth, with participants including Paul Sears, Herman Daly, Donella Meadows, Georgescu Roegan, Evelyn Hutchinson, and Gene Odum (29). This series turned out to be a somewhat harrowing experience. Some economists at the Yale Growth Center viewed us as misguided Luddites, and at almost every lecture about two carloads of libertarians, early Lyndon Larouche supporters, arrived to disrupt the question period with "one-liners" or questions the length of the Gettysburg address. Those behind this disruption believed Burch and I were lackeys of the oil companies, using the series on limits-to-growth as a vehicle to disparage growth and the development of fusion power, their key to unlimited growth and social justice. Pretty arcane, I thought. My students presented me with a box of "fusion flakes" after the last lecture.

In 1989, Steven Kellert and I presented the series "Ecology, Economics, Ethics: The Broken Circle" (30). This series, whose speakers included Ed Wilson, Gene Likens, and Norman Myers, produced the greatest attendance ever recorded at the Yale School of Forestry and Environmental Studies (YF&ES). All lectures were followed by an evening public question-and-answer session. I was impressed and even humbled by the quality of the questions posed by YF&ES and other Yale students. Their insightfulness and concern made me feel more optimistic about the future.

Looking back in time, it was just after graduation from Duke that I began special efforts to learn about important regional environmental problems and to see them for myself. On my honeymoon, my new wife and I spent a day inspecting the environmental damage around the great nickel smelter near Sudbury, Ontario. Later, while on a visit to Montana, I was introduced to problems associated with clear-cutting by Arnold Bolle and Larry Forcier. I saw the impacts of open-pit copper mining in Utah guided by Chuck Wullstein, and observed the effects of wildfire on the forests of central Ontario under the guidance of George Marek.

In 1971, Paul Miller introduced me to his elegant field experiments which demonstrated that the pollutant, tropospheric ozone, was the causal agent for the widespread sickness of ponderosa pine in the San Bernardino Mountains of California. Fifteen years later, Deane Wang, DF Karnosky, and I (31), using open-top chamber technology, found that ambient tropospheric ozone in southeastern New York State reduced tree sapling growth by 16% without

any visible symptoms of damage. In surveys of ozone damage on vegetation, estimates are usually based on visible symptoms. Clearly such estimates are underestimates.

More or less coincidental with the beginning of the HBES there was an outpouring of concern about the health of tropical forests. In the Amazon valley, rapidly expanding lumbering, followed by fire, extensive cattle ranching, and subsistence agriculture were thought to disrupt the forest's biogeochemistry, impede its capacity to recover from disturbance, and sharply reduce species diversity. I felt a need to incorporate this important topic into ecological teaching and research at Yale.

Through my work with the Ecological Society of America, I met the Mexican scientist Arturo Gomez-Pompa. We exchanged visits, and Arturo took me on a field trip to southern Mexico, where we witnessed extensive conversions of wet tropical forest to cattle pastures, and he showed me his many projects concerned with ecosystem restoration. Arturo and I developed an informal cooperative association which led to more than a decade of research, an extensive exchange of students and scientists, and several Ph.D. dissertations.

To give Yale students experience in a tropical environment, I designed a course on tropical ecosystems centered around ecologic and social conditions in Puerto Rico, gained funding from the Rockefeller Foundation, and established contacts with the Puerto Rican Department of Natural Resources and with Frank Wadsworth, director of the USFS Institute of Tropical Forestry. Every spring vacation, my class and I would take the red-eye flight to San Juan and begin two weeks of intensive ecological and social field work and meetings with scientists and private citizens that culminated at Señor Levi's coffee plantation high in the central cordillera. There, professors and students alike regularly disgraced themselves with uncontrolled consumption of piña coladas! Ralph Schmidt, who became head of the Puerto Rican Forest Service, was a student in my course.

Doctoral dissertations were done in Costa Rica, Puerto Rico, and Brazil, and nutrient cycling studies were completed in Puerto Rico in cooperation with Ariel Lugo. Another study, on the use of fuelwood in underdeveloped countries, begun at Princeton's Center for Energy and Environmental Studies, was completed at the East-West Center in Honolulu. With Kirk Smith and Bernard Bormann, I designed a microcomputer model for quantifying the energy efficiency, environmental impact, and social aspects of various fuelwood systems (32). This model was designed so third world planners, with minimal computing equipment, could evaluate energy, environmental, and economic impacts of competing fuelwood systems.

Eventually, with Garth Voigt and Francois Mergen, a teaching and research program for tropical studies was designed and funded by the AW Mellon

Foundation. This program continues to function today as the Tropical Resources Institute at the School of Forestry and Environmental Studies.

Throughout my career, I have sought to bring ecological thinking and problems to the attention of the general public. I have given many lectures to citizen groups, participated in programs to write curricula for public schools from kindergarten through twelfth grade, and been a guest on perhaps a dozen television shows. What has always troubled me about these efforts was the sense of distance and lack of personal connection with my listeners; I often felt like I was preaching. For ecological principles to be incorporated into public life, they have to be debated and fitted into life-styles that include much more than ecology.

In 1990, I found a way to make this connection through a study of the lawn, one of the most American of icons. For more than three decades, I photographed the marvelous displays of wildflowers that often grace less frequently mowed roadsides. These roadsides were not only beautiful, but offered economic and environmental savings, and it was but a step to start thinking the same principles applied to our lawns. I proposed to two colleagues, Gordon Geballe, an ecologist, and Diana Balmori, a landscape architect, that we offer a course titled "The American Lawn; An Ecological Anachronism?" with the specific objective of writing a book. Eleven students joined us, helped us block out chapters, and split into teams to write draft chapters. Drafts were collectively critiqued and rewritten. After two iterations, the term ended, and we were left with the tasks of producing a third iteration, designing appropriate illustrative material, and finding a publisher.

The book, *Redesigning the American Lawn: A Search for Environmental Harmony*, completed with the help of the inexhaustible energies of Lisa Vernegaard and Jean Thomson Black, was published by the Yale University Press in 1993 (33). The book focused on America's 53 million lawn owners, and examined the history, culture, economics, and ecology that underlie the ever popular American lawn. We reported that there are many lawn types, ranging from the Industrial Lawn, heavily dependent on energy, fertilizers, pesticides, and irrigation, to the Freedom Lawn, which requires no industrial inputs. Each type has its own economic, social, and ecologic impacts, and, by their choice of lawn care management, lawn owners are expressing their concern for the health of the biosphere. We suggested that the lawn owner could consider many strategies, from simple to complex, that would simultaneously meet aesthetic and economic goals and reduce their lawn's impact on the global environment. We proposed that through their own designs they could replace eighteenth century notions of the beautiful lawn with new aesthetics that would go with the new environmental ethics.

Finally, I turn to my students. I feel blessed to have had more than four decades of interaction with students ranging from undergraduates to Ph.D.s. Often we may think of teaching as a one-way street, with the teacher stimulating students with information, ideas, and questions. With those who are more than just faces in a classroom, teaching can be a sharing of ideas, values, and the glories and disappointments of life. What other profession can boast a better mechanism for keeping current and involved in emerging issues? Each year, a new crop arrives at your doorstep spouting the viewpoints and philosophy of their moment in history. It's hard not to keep abreast of the times. More important, the teacher can contribute to the molding of the person and the professional, and that is how history is made.

CAN WE GET THERE FROM HERE?

I think the greatest question facing humankind is: Can we design truly sustainable societies in harmony with nature? We hear the word sustainable a great deal these days, but I think that we are just exploring the coastline of a new continent and that the interior will be rugged beyond our dreams. Let me explain.

Two extremely important accomplishments of twentieth century environmental science are the elaboration and quantification of the biosphere as a highly interactive ecosystem and of humankind as a geologic force capable of altering global biogeochemical processes in significant ways. We now see earth as a dynamic, self-regulating ecosystem; powered by solar energy; characterized by millions of species, including humans, intimately bound to each other and to nonliving components by biological and biogeochemical processes. We also know that humans have acquired the power to alter biospheric processes profoundly. In the past 300 years, based on social organization, technology, and science, we have produced a seemingly endless succession of technological achievements undreamt of in the eighteenth century. Marvels of human infrastructure are found everywhere in the world, health care and agriculture have made incredible advances, and we are passing from the industrial revolution to the information revolution. In material terms, the quality of life for many people is at its highest level ever. A great many others are not so lucky.

Yet with this cornucopia of human benefits has come the power to alter biospheric process in ways menacing to the human future. We might think of these as the global environmental debt (GED) or as the sum of human activities that alter earth processes in ways that threaten humanity's future just as surely as national debts (34). Examples abound! Air pollution disrupts biospheric processes, leading to global climate change and an increase in biologically

destructive ultraviolet light at the earth's surface. The quality and quantity of soil, a vital human support system, is being destroyed at rates far greater than the process of soil formation can form new soil. The use rate of global ground water far exceeds recharge processes. Species are being lost far faster than the process of evolution can create new ones. Injecting pesticides, radionuclides, and heavy metals into processes of food transfer has unknown consequences for the health of humans and the rest of nature. Harvesting pressure in excess of reproductive processes leads to serious declines in desirable fish populations. Human pressures that accelerate processes of degradation over those of aggradation result in a rapid decline of world forests.

The GED does not occur in a vacuum, but rather is linked closely to and affected by political, economic, and social stresses in human societies. Together these stresses contribute to global instability and loss of quality of life. The use and abuse of natural resources, both renewable and nonrenewable, such as fossil energy, is closely related to growth of the GED. Many express deep concern about the need to slow human population growth in the coming century. Yet rarely do I hear equal concern about the need to slow the growth in our use of natural resources, which, according to some estimates, will occur at rates many times faster than population growth.

In the business section of *The New York Times*, one rarely reads of environmental concerns entering into gigantic deals designed to accelerate use of natural resources, productivity, and profit. In fact, the goal of every government on earth and every business is, with minor nods to environmental consequences, growth at the fastest rate. This translates into massive growth in the use and abuse of natural resources and an exploding global environmental debt. Although the past 30 years have brought many changes in first world countries that are designed to bring their economies more into harmony with nature, in light of the massive economic activities generated throughout the world, and the even greater changes just over the horizon, these remediative changes seem small indeed.

The goal of a global economy dedicated to relatively unrestricted growth seems on a collision course with the goal of a sustainable world based on harmony with nature. Many have faith that science and technology will pull us through, that humans can assume full responsibility for the successful regulation of biogeochemical cycles. Although new science and technology that seek sustainability are absolutely essential, we should bear in mind that technological solutions to environmental problems, requiring still more energy and other raw materials in one part of the biosphere, often exacerbate problems in other parts. As an ecosystem scientist, I cannot believe that science and technology alone are enough.

To find our way, we need to understand better how the natural world works; how environment, politics, economics, and society interact to affect how the world works; and how humans can work more effectively with, and not against, nature. We need to question many assumptions deeply ingrained in ourselves and in our societies. These include assumptions about the sanctity of market economies, rights of individuals vs rights of societies, the distribution of wealth and environmental pain, and growth as the answer to all problems. In the end, we must consider changing the current philosophy of “man apart from nature” to a twenty-first century philosophy of “humankind a part of nature.”

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