Archimedes 59 New Studies in the History and Philosophy of Science and Technology

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Springer

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ISSN 1385-0180 ISSN 2215-0064 (electronic)
Archimedes
ISBN 978-3-030-66932-4 ISBN 978-3-030-66933-1 (eBook)
https://doi.org/10.1007/978-3-030-66933-1

This Springer imprint is published by the registered company Springer Nature Switzerland AG
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

Chapter 2
The Origins, Early Aspects, and Development of the Long Term Ecological Research Program

Sharon E. Kingsland, Jerry F. Franklin, and Robert B. Waide

Abstract The chapter explores a series of overlapping discussions from the 1960s to the mid-1970s concerning the need to preserve natural areas for ecological research and observation, as well as the need for ecologists to be advocates for ecological science in the light of modern environmental problems. The U.S. Forest Service was also actively promoting the creation of Research Natural Areas and making them available for ecological research. A report prepared under the auspices of The Institute of Ecology in the mid-1970s drew attention to the desirability of creating a large network of experimental ecological reserves across the U.S. and its territories. This report led to three workshops during which ecologists debated what such a network of research sites might look like. The third workshop's proposals led directly to the creation of the Long Term Ecological Research (LTER) Program, which began in 1980 with funding from the National Science Foundation (NSF). The chapter follows these conversations with particular emphasis on the participation of Jerry F. Franklin, who took part in early discussions about expanding ecological research infrastructure while serving as Ecosystem Studies Program Officer at NSF during 1973–1975. He subsequently was director of the H.J. Andrews Ecosystem Research Project, which joined the LTER Program in 1980, chaired the LTER Program's Coordinating Committee from 1982 to 1995, and established and directed LTER's coordinating office from 1982 to 1996. In 1997 the Network Office shifted

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R. B. Waide, S. E. Kingsland (eds.), *The Challenges of Long Term Ecological Research: A Historical Analysis*, Archimedes 59,
https://doi.org/10.1007/978-3-030-66933-1_2

to the University of New Mexico, beginning a new phase in the coordination of the developing network.

Keywords LTER program · LTER network · LTER network office · Long-term ecological research · History of ecology · Conservation movement · Ecosystem ecology · International biological program

2.1 Introduction

This chapter explores the evolving discussions leading to the creation of the Long Term Ecological Research Program (LTER), which began in 1980 with the selection of the first group of six sites. Discussions among ecologists during the preceding two decades provide insight into how a portion of the ecological community was thinking about ways to strengthen the discipline of ecology. The intensive planning process that led directly to the Program took place over a 4-year period from 1976 to 1979. It began with a study commissioned by the National Science Foundation (NSF) in 1976, prepared under the auspices of an advisory body known as "The Institute of Ecology" (TIE), whose origins will be discussed in more detail below. TIE's report, titled "Experimental Ecological Reserves: A Proposed National Network," was completed in December 1976 and was published in June 1977. That report envisioned an extensive network of 71 sites for ecological research in 67 locations, a highly ambitious plan that was designed to expand on the existing place-based research already being conducted in the United States and its territories. Although that plan was not implemented, the report led to three NSF-sponsored workshops held in 1977, 1978, and 1979, which explored ways to support long-term ecological research and observation. The third workshop, held in June 1979 in Indianapolis, Indiana, was convened by The Institute of Ecology.

The reports from these meetings reveal concerns in the ecological community about strengthening the science in the 1970s, including programs that might be useful. The reports also show how the community worked to translate the ambitious vision outlined in TIE's report into a specific set of proposals that would be acceptable to NSF. Many in the ecological community had been critical of the International Biological Program and were not in favor of such a long-term ecological program, fearing it would draw off funds from more meritorious science. However, by 1979 long-term research had many strong and influential supporters. NSF responded to these ideas and largely adopted the recommendations of the third workshop, although the LTER program that emerged from this process was much smaller than the network concept originally envisioned in TIE's report.

However, the history of the LTER program does not begin with this planning process. We also need to understand how the ecological community arrived at this point in the mid-1970s. For this purpose, we must go back to about 1960 and seek answers in the discussions that ultimately generated the strategic planning process.

Because the decision to fund the program was based on a belief that many in the ecological community supported it, we need to probe how those members of the community were articulating the needs of their science in the 1970s. Since the vision presented in TIE's report was much more ambitious than the LTER program, it will be instructive also to explore how that vision arose from a decade and a half of intense discussion about the future of ecology. What were the imperatives to which ecologists were responding? We aim in this chapter to answer this question, not as it applies to individual sites – each of which has its own story related to its traditions of place-based research -- but to the program as a whole.

This chapter emphasizes three inter-related developments that helped create the rationale for the LTER program. One was a resurgent *preservation movement* that emerged in the postwar period and focused on the need to preserve natural areas for scientific research. The U.S. Forest Service took on a leadership role as this movement expanded in the 1970s. The second, closely related development, was the emerging *environmental movement* of the 1960s, which challenged ecologists to articulate more clearly the value of ecology to American society, and federal land management agencies to adopt an ecological perspective in their work. Ecologists' response to the environmental movement helps to explain the timing of LTER, that is, why ecologists believed that the time was ripe in the late 1970s for this kind of program, and for a systematic reorganization of ecosystem ecology. The third development was the emergence and gradual dominance of *ecosystem ecology*, which provided ecologists with a reason to seek resources for long-term ecological research at the ecosystem level (while including research at community and population levels), justified in part by its relevance to resource management.

The chapter also provides a personal account of the origins and development of the LTER program by Jerry F. Franklin, who was involved in discussions about long-term research as a program officer at the National Science Foundation during the 1970s, and went on to take important leadership roles in the program from its inception to 1995. From 1975 to 1986 he served as director of the H. J. Andrews Ecosystem Research Project. This project, at the H. J. Andrews Forest in Oregon, became part of the first group of LTER sites in 1980. From 1982 until 1995, Franklin also served as Chair of what was initially called the Steering Committee of the LTER program, and later the Coordinating Committee. While based at Oregon State University in the early 1980s, Franklin obtained a coordination grant to establish an LTER Network Office, which in 1986 moved with him to the University of Washington, Seattle. As David Coleman recalls in his history of the LTER program, "Much of the growth of the LTER Network occurred during Franklin's leadership" (Coleman 2010, p. 98). Franklin retired as Chair after 12 years, when James Gosz became the Coordinating Committee Chair. In 1997 the Network Office moved to the University of New Mexico in Albuquerque, and Robert Waide became its executive director. The chapter draws on the experiences of Franklin and Waide, providing an insider's account of the early years of the program, as seen from the perspective of those in the Network Office.

2.2 Recognizing a Need

Before we delve into the details of The Institute of Ecology's report on "Experimental Ecological Reserves," a bit of background is needed to set this report in historical context. This context will help us to understand how the creation of the LTER program was the culmination of ecology's growth and maturation as a discipline, as well as an attempt to respond to the impacts that human population growth and rapid development were having in the United States. In the 1960s ecologists were becoming aware that a sea-change was underway in postwar American society, and that they needed to recognize it, respond to it, and come up with a plan. These changes were transforming the land and were threatening to eliminate the last vestiges of what was viewed as a pristine natural environment. That sense of the finiteness of "Spaceship Earth," and the impending loss of species, habitat, and entire natural worlds, engendered profound concern, which was transformed into a call for action.

TIE's report on "Experimental Ecological Reserves," which articulated the scientific community's desire to preserve and maintain areas for ecological research, was the culmination of a preservation movement within ecology that had been building support for decades.

As Gina Rumore (2012) has noted in her study of the preservation of Glacier Bay National Monument in Alaska, this scientific preservation movement has a long history. The Ecological Society of America created a Committee for the Preservation of Natural Conditions for Ecological Study as early as 1917, an idea promoted by Victor Shelford, the Society's first president. Rumore argues that the protection of Glacier Bay in the 1920s truly fulfilled the Committee's goal of preserving land for ecological research, unlike some of its other campaigns, which were aimed more at protecting lands from development. She suggests that preserving land for research reflected ecologists' desires to do long-term, place-based research, even when funding for such long-term research was hard to obtain.

Shelford argued that research in natural areas was essential also to the work of wildlife managers. The job of management was to control populations, which meant that managers worked in highly unnatural environments. As he pointed out in 1933, their work was hampered by an absence of guiding scientific principles and accurate information (Rumore 2012). Thus, scientific research in protected natural areas could have direct practical value. The Committee was disbanded in 1945, but Rumore sees a continuous historical thread linking the earlier interests of ecologists in long-term study and the eventual creation of the LTER program. This thread can be discerned more clearly if we track how discussion about preservation of land for ecological research continued in the postwar years. That discussion led directly to the planning stages for the LTER program.

The link between scientific research on protected areas and practical problems of resource management and conservation was nowhere more evident than in forestry, a subject we will return to later in this chapter. By the early twentieth century, Americans were becoming alarmed at the devastating impact of the logging industry on eastern and Midwestern forests, where destruction of forests increased the risks of wildfires, erosion, and flooding. The early conservation movement was largely driven by concern to preserve forests.

Creation of reserved forest areas began in 1891 with a Congressional provision that allowed Presidents of the United States to establish forest reserves out of the public domain. During the period of 1892 to 1905, 63 million acres of reserved had been set aside by presidential proclamation. Theodore Roosevelt aggressively added to that total and together with his successor, William Howard Taft, left the protected forest lands at 187 million acres.

Initially administered as the Forest Reserves in the US Department of Interior, Roosevelt transferred them to the Department of Agriculture in 1905, to the jurisdiction of a newly-created Forest Service. A dynamic conservationist and close friend of Roosevelt, Gifford Pinchot, led this agency and renamed the reserves the National Forests. Passage of the Weeks Act in 1911, after a long and heated campaign, granted the federal government the right to create forest reserves through the purchase of private forest lands containing headwaters of navigable streams (Johnson and Govatski 2013). Pinchot and other conservationists argued vehemently that deforestation contributed to catastrophic floods, making the case that protection of forests had many important ecological and economic consequences beyond ensuring a supply of lumber.

The Forest Service included active research almost from its establishment, creating a research branch that reported directly to the Chief of the Forest Service. As a result, the National Forests became home to experiment stations focused on understanding all aspects of these forests, including their management. The role of forests in regulating stream flow was a part of the research program right from the start, because of the need for much more scientific information about the relationship between forest cover and streamflow and water quality – what would now be broadly categorized as “ecosystem services”. Many experimental forests and ranges were established to provide sites for long-term studies, which included experimental manipulations of watersheds.

The creation of this research infrastructure within the Forest Service ultimately had a direct bearing on the selection of sites for the Long Term Ecological Research Program. For example, in 1926 the Appalachian Forest Research Station in North Carolina started a long-term program of research on forest cover, erosion, and streamflow under the direction of forest ecologist Charles R. Hursh (Douglass and Hoover 1988). It became the Coweeta Experimental Forest in 1934, and during the 1930s benefited from Franklin Roosevelt’s New Deal programs, especially the creation of the Civilian Conservation Corps, which provided the manpower needed to expand its facilities.

With a strong research tradition that continued into the 1960s, Coweeta was selected in 1969 as one of five study areas for the Eastern Deciduous Forest Biome Project of the International Biological Program. In 1980, it became one of the first sites of the new Long Term Ecological Research Program (Swank et al. 2001). This example supports Rumore’s argument that there is a strong historical thread linking early interest in long-term research (partly in relation to specific resource management problems) and the LTER program of the 1980s. Thus, when the Coweeta LTER site was only 4 years old, it celebrated the fiftieth anniversary of research in forest hydrology and ecology at that location, and as Eugene Odum remarked, by 1984 it was already the “longest continuous environmental study on any landscape in North America” (Odum 1988).

The H. J. Andrews Experimental Forest in Oregon and Hubbard Brook Experimental Forest in New Hampshire (both discussed in Chap. 8 in this volume) provide two additional examples of important long-term research sites established by the Forest Service that ultimately became locations of LTER projects. Both were established after World War II and benefited from the pioneering watershed research at Coweeta. The extraordinary productive collaboration between Forest Service and academic scientists at Hubbard Brook demonstrated the value of such joint efforts in using long-term research, including experiments, to elucidate fundamental ecosystem principles. Research at H. J. Andrews extended such research into the massive coniferous forests of western North America. The three properties – Coweeta, Hubbard Brook, and Andrews – have sometimes been described as the “Crown Jewels” of the Forest Service’s Experimental Forests and Ranges.

Following the Second World War, the scientific preservation movement that can be traced to the early years of ecology took on new life, broadening and intensifying. The goal was to make ecology into a more predictive science, so that it could better serve the needs of resource managers, much as Shelford had argued decades earlier. This movement also involved traditional conservation groups like the Sierra Club, who saw the advantage of linking their interests in preserving wilderness to scientific needs. For example, the sixth Wilderness Conference sponsored by the Sierra Club in 1959 adopted as its theme “the meaning of wilderness to science” (Brower 1959). As was customary for these conferences, it ended with recommendations. One supported the passage of the Wilderness Bill then under discussion (but not passed into law until 1964), on the grounds that natural areas were needed for benchmark scientific studies to assess how humans had affected the planet. The second recommendation urged agencies administering public lands to undertake long-term research programs involving every branch of science and focused on natural or unaltered reserves.

By the 1960s, ecologists were also thinking harder about their responsibilities in the wake of the environmental movement, which was putting the word “ecology” into everyone’s vocabulary. As environmental problems steadily mounted in the 1950s, the tipping point was the appearance of Rachel Carson’s book, *Silent Spring*, in 1962, which galvanized the modern environmental movement with a call to curb our excessive use of pesticides. Carson’s book got the attention of the Kennedy administration, and especially of Secretary of the Interior Stewart Udall, who was also an impassioned conservationist. The growth of the environmental movement would be a challenge to ecology, a relatively small scientific discipline in the early 1950s, to enlarge its social role. In the words of plant ecologist F. Herbert Bormann, “ecology” was becoming “more powerful than ecologists” (Bormann 1996). This time of crisis and dispute, as Bormann and others realized, demanded a response from ecologists. These responses to the environmental movement, as well as revitalization of the movement to preserve natural areas for scientific research, helped lay the groundwork for the creation of the LTER program. Bormann's career at this time illustrates particularly well how these two threads were intertwined.

In the late 1950s and early 1960s Bormann was on the faculty at Dartmouth College in New Hampshire. Later he would become known for the important results that emerged from the Hubbard Brook Ecosystem Study in New Hampshire, which he co-founded in 1963 with Gene Likens, Robert S. Pierce, and N. M. Johnson, and which led to the discovery that acid precipitation was affecting the United States (Likens and Bormann 1974; Likens and Bailey 2014). Hubbard Brook Experimental Forest would join the LTER program in 1987 and is still part of that program (see Chap. 8 in this volume for further discussion). But Bormann is also important for the history of LTER because he was among the earliest to recognize that a new age of environmentalism was dawning, and ecologists needed to take note and realize what this all meant for ecology.

His own research brought this home to him. In 1957 Bormann was studying mineral transfer between roots using radioactive tracers, and found that his greenhouse experiments were ruined because the control plants, which were not supposed to be radioactive, had been contaminated with radioactivity. A sample of leaves from the garden showed that all the plants outside were radioactive, and the most likely source was the nuclear bomb test series conducted in Nevada that year. Bormann thought the results were important enough for publication in *Science*, but the manuscript was rejected. His suspicion was that reviewers from the Atomic Energy Commission did not wish this information to come to public attention. Instead he published it in *Ecology,* a journal of more limited readership and impact, where it drew no attention (Bormann et al. 1958; Bormann 1996).

These kinds of experiences, he recalled, coupled with his awareness of public concern about the environmental impact of nuclear and chemical technologies, began to enlarge his view of ecology (Bormann 1996). At the time *Silent Spring* was published in 1962, robins on the Dartmouth campus were showing traumatized behaviors. Bormann and his students followed the scientific literature linking these behaviors to ingestion of DDT. He spent a year in 1962–1963 with George Woodwell at Brookhaven National Laboratory, where studies of the ecological cycling of radioactive nuclides were being conducted. Ecology conducted at the national laboratories focused on ecosystem science, which emphasized the connection of living and non-living components of the ecosystem. Making these connections was crucial to understanding the effects of such contaminants as radioactivity and pesticides within and between ecosystems.

Bormann and Woodwell discussed these environmental issues and what they meant for ecology. Together they embarked on a "missionary trip" around the country with the message that "ecology is more powerful than ecologists" and that "ecologists needed to emerge from their cocoon" (Bormann 1996, p. 22). Bormann remembered getting few converts, but making many enemies. But ecology was starting to change under the leadership of Bormann, Woodwell, and others who recognized the relevance of these environmental problems for ecology, and for understanding our relationship to the world. After moving to Yale University in

1966, Bormann continued to spread awareness of environmental problems to students and the broader public.

In 1966, the year that he moved to Yale's School of Forestry, Bormann also spoke out about the implications of human population growth and uncontrolled development for scientific research, publishing an editorial in *BioScience* appealing to biologists to support measures to protect natural areas for research (Bormann 1966). His experiences, including 8 years in the Piedmont region of the southeast and another 10 years in the hardwood forest ecosystem of northern New England, revealed that it was getting harder and harder to locate old-age, relatively undisturbed forests. That meant that it was getting harder to find natural areas to serve as biological baselines. Bormann's colleagues were having the same difficulty, and it was affecting both their research and teaching. As he observed, "Destruction or alteration of important biological research areas is unquestionably accelerating as our population grows and as the tools for environmental manipulation become more powerful" (Bormann 1966, p. 585). Without knowing how an undisturbed ecosystem functioned, it would be difficult for a land manager to know how to implement and judge the efficacy of practices relating to such things as timber production, disease control, wildlife management, recreation, water yield, or conservation of species. Baseline studies, he argued, were needed both to understand how ecosystems functioned, but also to help people evaluate the changes they were making, and would continue to make, in their environments.

He advocated setting up a federal system of natural biological reserves in order to preserve a range of biologically important ecosystems across North America, and to use these reserves both for scientific research and teaching. At that time, he noted, there was "no thoughtfully planned national system of representative ecosystems set aside for the purpose of descriptive and experimental research" (Bormann 1966, p. 585). Instead, there was a "potpourri of biologically interesting areas set aside by thoughtful citizens both within and without the government." A more systematic approach to the study of these ecosystems, especially those that were already protected, was clearly needed.

Bormann was not alone in making this plea for a system of reserves, and pointed out that in the previous 2 years five separate groups had "called for intensified study of our natural environment" (Bormann 1966, p. 585). These groups were: the President's Science Advisory Committee (which had issued a report on "Restoring the quality of our environment" that included the idea of studying naturally occurring ecosystems as baselines); the U.S. National Committee for the International Hydrologic Decade (which similarly advocated the study of natural environments); the planning groups for the International Biological Program (IBP), which also called for the study and conservation of natural ecosystems; biologists who were concerned that plans for weather modification were being made without adequate prior study of environments that would be affected by such modifications; and finally the Ecological Society of America, which was promoting the creation of biological stations in major biomes. The American Association for the Advancement of Science had also written a report in 1963 calling for a larger and better coordinated natural area program.

Bormann also drew attention to a Senate bill introduced by Senator Gaylord Nelson of Wisconsin in 1965, called the Ecological Reserves and Surveys Bill (S.2282), which advocated creating a federal system of natural areas. Nelson is perhaps best known for leading the organization of the first Earth Day on April 22, 1970. His deep commitment to environmentalism was shared by the Secretary of the Interior, Stewart Udall. The bill would have authorized the Secretary of the Interior to conduct a program of research, study and surveys of the natural environmental systems of the United States (U.S. Senate 1966). Nelson wanted to set aside representative natural environments on federal lands for scientific study, as well as assist in setting up similar reserves on state and private lands. The purpose was in part to provide information to natural resource managers. The plan was not to encroach on other federal agencies that were supporting ecological research (amounting to about $90 million per year), but those agencies were seen as being more mission-oriented rather than focused on basic research. Nelson hoped to zero in on the study of "basic processes," of the kind that would be needed to make policy decisions about resource use.

The bill was referred to the Senate's Committee on Interior and Insular Affairs, and Nelson presided over the hearings conducted in April 1966. Although the bill never got out of committee, the hearings, which involved testimonies of many leading ecologists, including Bormann, provide a snapshot of ecological and environmental thinking at this critical time of change (U.S. Congress 1966). Bormann's statement was the basis of the editorial he subsequently published in *BioScience*. Several leading ecologists from the Ecological Society of America (ESA) similarly supported the bill, including Bostwick Ketchum (President of ESA), Stanley Auerbach (Secretary of ESA), LaMont Cole (Chairman of ESA's Public Affairs Committee), and ESA's Committee on Applied Ecology. Roger Revelle, Chairman of the National Academy of Sciences Committee for the International Biological Program, argued that the bill's provisions would help to strengthen the activities being planned in connection with the IBP, due to start the following year. Secretary Udall, speaking on behalf of the Department of the Interior, supported the Bill strongly, arguing that such a program of ecological research would help to accomplish the goals that President Lyndon Johnson had outlined in a special message to Congress on "Conservation and Restoration of Natural Beauty," delivered on February 8, 1965 (Johnson 1965).

However, both the Department of Agriculture and the National Science Foundation opposed the bill, partly on the grounds that it would duplicate work already being done. In fact, a federal regulation had just been issued in March 1966 directing the Forest Service (part of the Department of Agriculture) to designate a series of "Research Natural Areas" (RNAs) especially in areas that had unique characteristics of scientific importance. These Research Natural Areas were to be maintained as much as possible in their pristine or unmodified condition and used for scientific research. (The Forest Service had established its first research natural area as early as 1927, the Santa Catalina RNA in the Coronado National Forest in Arizona.)

There might have been legitimate cause for thinking that Nelson's bill would duplicate these and other activities within the USDA, but the idea that it would also duplicate work by NSF was less obvious. Leland Haworth, a particle physicist and director of NSF, presented the agency's brief report. He argued that the bill's wording needed clarification about which activities were to be authorized, and that a section of the bill meant to authorize grants to universities and colleges for the training of ecologists was not necessary, because there were already sufficient training grants and fellowships available in the sciences. John Cantlon, an ecologist who was serving for 1 year as program director in NSF's Division of Environmental Biology, offered a personal statement at the Senate hearings, arguing that the Department of the Interior was in fact the logical agency to conduct these kinds of preservation and research activities, because they were precisely aligned with the mission of the Department. He did, however, add that research supported by the National Science Foundation in the past and future would also be indispensable over the long range.

Despite the bill's failure, the pressure to preserve ecological diversity for scientific purposes continued to intensify in the 1960s. Ecologists realized they needed to take a more active role in educating the public about the value of ecological science and bringing about the kinds of conservation policies that would be beneficial both to science and to American society. As Udall put it, scientists had new roles as "midwives of conservation" (Miller 1968). A symposium on "The Role of the Biologist in Preservation of the Biotic Environment", published in *BioScience* in May 1968, gave ecologists an opportunity to reflect more deeply on what had been accomplished and what remained to be done. Stanley Cain, an ecologist and Assistant Secretary of the Interior for Fish and Wildlife and Parks, viewed the need to preserve natural areas as a matter of national urgency (Cain 1968). He noted that the U.S. lagged behind many other countries in establishing natural areas for research. One of the writers was Orie Loucks, Wisconsin ecologist, who wrote about Wisconsin's innovative program of natural area preservation, which went back to the 1940s and which he hoped would be a model for other states (Loucks 1968). Wisconsin had been home to the renowned conservationist Aldo Leopold, who in his posthumous book *A Sand County Almanac*, published in 1949, articulated the need for a new "land ethic", or a statement of human responsibility to preserve the land's capacity for self-renewal (Leopold 1968). Loucks later became the Science Director at The Institute of Ecology and was involved in the planning process leading to the LTER program.

The mid-to-late-1960s, therefore, were years of increasing activism on the part of ecologists, as they realized they needed to emerge from the cocoon and guide the environmental movement and also take leadership in the resurgent preservation movement. One idea was to create a coordinating institution, a National Environmental Institute, to guide and inform the newly created Environmental Protection Agency (Bowers et al. 1971). The creation of The Institute of Ecology in 1971 was another expression of this activism, and TIE's study and recommendation in 1977 to create a network of experimental ecological reserves was the culmination of discussions that had been building for a decade. If we ask why many ecologists believed that the time was ripe in the late 1970s for a long-term ecological research program, part of the answer lies in understanding the origins and *raison d'être* of The Institute of Ecology, which was so heavily involved in the planning process.

The Institute of Ecology was formed to draw attention to the importance of ecological research and of the discipline of ecology to American society (Doherty and Cooper 1990). It was a response to the realization that with the rise of the environmental movement in the 1960s, the federal government was ignoring or overlooking the expertise of ecologists. Ecologists began to see that having a concrete plan for expanding resources for ecological science was important in representing the interests of the ecological profession to Congress. The Institute of Ecology originated in a strategic plan developed within the Ecological Society of America, but it was incorporated as an independent body in 1971. TIE was actually a consortium that involved many institutions, and it operated much like a think tank or general advisory body. But instead of having a small group of thinkers who operated from a central institute, on the model of other think-tanks, it was dispersed across a wide range of academic institutions and included people outside of academia. Until it ended in 1984, it served as a vehicle to foster cooperative relations between ecologists and people having other skills and perspectives (The Institute of Ecology 1977). Each of its projects included several institutions and disciplines, and any given project might involve 200 or more people. Working through numerous advisory and study groups, TIE obtained grants from agencies such as the NSF, which it used to sponsor symposia and workshops, and from these generated reports on a broad range of topics. In selecting TIE to prepare an advisory report to NSF on the future directions of ecological science in 1976, it was a foregone conclusion that TIE would strongly back the expansion of resources for the support of ecology, and its report of 1977 did exactly that.

Yet another source of support for the idea of creating and preserving natural reserves was the Atomic Energy Commission, especially through the work of the national laboratories established under its control after the Second World War. The national laboratories were surrounded by large reservations, some created during the Manhattan Project to act as buffer zones for the secret work being done on the atomic bomb. When the national laboratories were created, these buffers became ecological reserves, and in 1971 were officially designated as "National Environmental Research Parks" (NERPs). By the late 1970s (by which time the Atomic Energy Commission had been dissolved and replaced by the Department of Energy) several NERPs had been designated at Savannah River (South Carolina), Hanford (Washington), Idaho, and Los Alamos Scientific Laboratory (New Mexico). Others were under discussion at Oak Ridge National Laboratory and the Nevada Test site. These locations were highly disturbed sites, and one of the goals of ecological research was to measure the effects of disturbance on ecosystems (Hinds 1979).

Among these many initiatives, discussions, and calls for the preservation of reserves that were important for ecological research, the Forest Service stood out in the 1970s as it began to greatly expand its network of Research Natural Areas. In the next section we will consider in more detail the role of ecologists within the Forest

Service, focusing especially on the career of Jerry F. Franklin, who was on the planning committee for TIE's 1977 study and was a strong voice in the discussions about the need for long-term ecological research in the 1970s.

2.3 Expanding Ecology's Role

In 1968 Jerry Forest Franklin, then Research Forester with the Forest Service's Northwest Research Station in Portland, Oregon, explained the logic behind the Research Natural Areas in an article co-authored with James Trappe. They argued that the forestry profession should continue its past leadership role in building a system of natural areas (Franklin and Trappe 1968). While many research problems that they named were closely tied to specific practical issues, such as evaluating pollution, they also recommended research on more basic ecosystem processes and pointed out that natural areas could serve as gene reservoirs for species. They urged foresters not to think of natural areas as "museum pieces," but as "outdoor laboratories for applied and basic research" (Franklin and Trappe 1968, p. 461).

Although research natural areas were an old concept within the Forest Service dating to 1927, the designation of such areas had remained fairly flat through to the 1960s. That changed after the passage of the National Environmental Policy Act (NEPA), which went into effect in January 1970. NEPA contained a provision for the preservation of important historic, cultural, and natural aspects of the American heritage. As Franklin explained in 1972, the Federal government was involved in an "intensive program to greatly expand the number of Research Natural Areas in the next several years, in order to have at least a minimal system incorporating all major ecosystem types" (Franklin et al. 1972, p. 135). From 1968 to 1972 the number of natural preserves increased from 336 to over 500. Franklin, along with Robert E. Jenkins (ecology advisor to the Nature Conservancy, Arlington, Virginia) and Robert M. Romancier (also from the Pacific Northwest Research Station in Portland) appealed to ecologists to make more use of these Research Natural Areas, which, being permanently protected, were ideal for obtaining baseline information of natural systems and for doing integrated ecological studies (Franklin et al. 1972). Yet they reported that there were few natural areas being used for these kinds of integrated studies.

This period of rapid expansion of Research Natural Areas coincided with the expansion of ecosystem ecology and with the biome studies conducted in the U.S. in connection with the International Biological Program. Eugene P. Odum, along with his brother Howard T. Odum, had been working to make the ecosystem concept into a central organizing concept for ecology since the 1950s, and Eugene Odum became the chair of the Ecological Society of America's Committee on IBP. Some American biologists had reservations and even aggressively opposed IBP as it was first conceived. However, as Golley (1993) has discussed, by 1968 many were more strongly in favor of the program and eager to start the biome studies, which ran from 1968 to 1974. The IBP biome projects analyzed five biomes: grasslands, tundra, coniferous and deciduous forests, and deserts (a tropical forest study was planned but did not materialize). These studies helped promote ecosystem ecology because of their relatively large scale, their focus on the study of biogeochemical cycles, and their emphasis on ecological modeling.

As Michigan ecologist Frederick E. Smith explained in 1968, the term "ecosystem" was understood to mean "the total system of populations together with all nonliving components that interact in a defined region of space and time" (Smith 1968). Despite uncertainty about whether it was possible to analyze whole ecosystems and compare them meaningfully, Smith saw potential in what the IBP might bring to this area of ecology. He had just switched from an interest in population ecology to ecosystem ecology around the time of the IBP, and served as Director of the IBP Analysis of Ecosystems Program. Until about 1968, Smith explained, ecosystems had been "regarded primarily as backgrounds against which studies of components are executed" (Smith 1968, p. 7). As the IBP got underway, he noted that ecologists were feeling optimistic that it would be possible to analyze something as complex as an ecosystem, and that there was a growing conviction "that ecosystem analysis should be at the forefront of ecology, rather than serving as the background" (Smith 1968, p. 7).

Smith's discussion sheds light on how a contemporary ecologist saw in the IBP some hope for the future of his profession, a future that meant change both in intellectual directions and in the culture of science. In Smith's appeal to his colleagues to recognize and embrace the potential of the IBP, we can discern both a sense of frustration at the slow pace of ecology's development until that time, and a hopeful sense that this snail's pace of development was about to speed up.

Smith admitted that "despite some courageous attempts, we have no way to interrelate different aspects of ecosystems, such as energy flow, species diversity, and vegetational structure," and that "we have only the haziest of ideas on why some particular set of results is found." Those continuing uncertainties meant that "ecosystem analysis is a discipline that is just being born" (Smith 1968, p. 7). But in 1968 he looked forward to the way the biome projects of the IBP would stimulate new concepts and principles in ecosystem ecology that would, he hoped, add up to "a whole new level of ecology" (Smith 1968, p. 10).

It is noteworthy that Smith's vision and his hopes for ecology included a strong element of human ecology as a counterweight to ecologists' tendency to study natural or undisturbed systems. Because the IBP included a program focused on human adaptability (mostly aimed at the disciplines of anthropology and physiology), he expected that human ecology would also be incorporated into ecosystem-level analysis, and that ecologists might be nudged to adopt a point of view that accepted the idea that humans were part of ecology. He observed that a shift toward human problems was already underway, "perhaps in response to congressional interest in ecology," and commented that there was strong interest among graduate students in the applications of ecology and in "man as a part of ecology" (Smith 1968, p. 11). He also imagined that the IBP would help the discipline of ecology become more unified or integrated, but that change would require cultural shifts: "There is even hope that we can be induced to accept such radical concepts as team research and data sharing. If this is accomplished, the eventual effect of the IBP will be establishment of an integrated profession based on regional centers of study, dedicated both to the development of basic science and to its applications to human welfare" (Smith 1968, p. 11). Although the IBP projects did not all live up to the grand expectations that were envisioned, we wish to highlight how the IBP stimulated ecologists like Smith to see it as a vehicle to strengthen and advance their discipline. In many respects the LTER program, which entered its planning stages a decade later, was part of the same conversation about expanding the role of ecology.

The potential of the IBP biome projects for advancing ecology scientifically was exemplified in the Coniferous Forest Biome project. This was the last of the biome studies to be funded and almost lost out because of the inability of interested parties at the University of Washington and Oregon State University to reach agreement on a collaborative program. Ultimately, under threat that there would be no funding if an accommodation was not forthcoming, the principals agreed to a program divided between the two institutions. Dr. Stanley P. Gessell became the Director of the Coniferous Forest Biome study, Jerry Franklin his Deputy Director, and Richard Waring and Dale Cole the respective leaders of the Oregon and the Washington efforts. The University of Washington utilized several research sites near Seattle, Washington and the group at Oregon State University, which included both university and Forest Service scientists, focused primarily on the old-growth coniferous forest in an experimental watershed in the H. J. Andrews Experimental Forest.

The primary goal at H. J. Andrews was to build carbon, nutrient, and hydrologic models of a pristine old-growth Douglas fir-western hemlock forest ecosystem. The project was a collaboration of Forest Service scientists at the Corvallis, Oregon Forest Sciences Laboratory and academic scientists, primarily at Oregon State University. Generic objectives at both Biome locations were to study the characteristics of ecosystems, the processes causing the transfers of matter and energy within the systems, how the systems responded to natural and human-induced stresses, and to understand the land-water interactions characteristic of each biome. The results of these and earlier studies would be synthesized into predictive models that would aid in resource management in each biome (U.S. National Committee for the International Biological Program 1971).

Franklin viewed this kind of integrated biome study as urgently needed to respond to what he called the "demand explosion," i.e., the tremendous need policy makers had for information as resource management problems became more complex (Franklin 1972). The relatively straightforward questions traditionally asked by forest managers were being replaced by more complicated questions involving the broad ecological impacts of such activities as clear-cutting forests or applying herbicides. Effects of management on all aspects of ecosystems were being considered, and information was needed on how the different parts of ecosystems were linked, how the natural and social sciences were linked, and how quickly ecosystems recovered from disturbances. All of this was driven by requirements of federal environmental legislation passed during the 1960s and 1970s, including the Endangered Species Act and the National Forest Management

Act of 1976 (see below). These laws made it imperative that policies would have to develop and adopt ecosystem-level approaches to management of federal forest lands (Skillen 2015).

The "whole systems" or ecosystem viewpoint was not traditional in either resource management or ecological science, and ecologists were challenged to develop interdisciplinary programs that would provide the relevant science. The biome projects were an initial attempt to confront these big problems directly, each struggling to learn to deal with a complicated new worldview and its unprecedented demand for information. The Coniferous Forest Biome project, Franklin argued, was critically important to provide the information needed to design and forecast the effects of ecosystem-based management practices as well as other stresses on the composition, stability, and short and long-term productivity of the forest. "At least as important as the increased predictive capabilities," he added, "are the insights into processes and the identification of additional research needs such as in the areas of below-ground processes, decomposition and extrapolation of predictive capabilities in time and space" (Franklin and Waring 1974, p. 232).

The environmental legislation of the 1960s and 1970s profoundly affected federal land agencies, as discussed by Skillen (2015) in his historical analysis of federal ecosystem management. The Forest Service had faced intense criticism and litigation over its management practices, which favored timber production as the most important forest value. The merit of its multiple-use management approach was debated throughout the early 1970s and its practices found to be illegal under existing law, which led to the passage of the National Forest Management Act of 1976. This Act required the Forest Service to prepare comprehensive forest plans for all resources on each national forest every 15 years. The planning process had to follow the procedures laid out in the National Environmental Policy Act, which included public review of and comment on proposed plans. All of these required more scientific information, more attention to environmental concerns; the agency had to hire "ologists" of many different specialties, including ecologists to meet these requirements. The Act also required the Forest Service to protect habitat in order to preserve biological diversity, and this mandate, as Skillen argued, "forced the agency to look at national forests through an ecological lens, because it had to look at forests through the perceived needs of various species" (Skillen 2015, p. 78). The research conducted as part of the IBP Biome projects was exactly the sort that the Forest Service needed to develop its forest plans to achieve ecological goals, not just traditional timber production goals. But this expanded vision of the Forest Service also carried over into the discipline of ecology more broadly.

By advocating an expanded role for ecology, especially in relation to forest management, Franklin and his associates were also arguing for an expanded research capability, so that ecological advances could be made simultaneously on different fronts. The availability of biological reserves, such as the Research Natural Areas (which by this time involved all federal land management agencies, not just the Forest Service), were providing new opportunities for ecological research, with an emphasis on collecting baseline data from relatively pristine, or undisturbed, systems. Similarly, several of the Forest Service Experimental Forests and Ranges were proving critical for manipulative types of research activities. Conservation efforts were underway in various branches of government, in the private sector, and internationally in connection with the International Biological Program. The Biome studies were a beginning in the effort to develop a more useful and predictive ecology, in an emerging era where managers were required to understand and address management of federal lands as ecosystems.

Franklin was also involved in the follow-up to the IBP, UNESCO's Man and the Biosphere program, where he chaired the U.S. Committee on Project 8, which was about Biosphere Reserves. This international program was meant to safeguard genetic diversity of species as well as provide areas for ecological research, education, and training. The U.S. program focused mainly on preserving natural areas representing major biomes or biotic divisions and pairing them up with areas that could be experimentally manipulated, such as the experimental forests and ranges. The list of eligible sites included many of the locations that would later become part of the LTER network, chosen because they were already experimental areas with histories of ecological research and monitoring. The U.S. Committee created working groups to encourage collaborative programs linking reserves of different types, with the goal of stimulating research and monitoring programs (Franklin 1977). However, resources for such expansion were still sparse; these discussions reflected what scientists aspired toward rather than what was achieved.

All these developments were converging in the mid-1970s, setting the stage for the creation of the Long Term Ecological Research program. Jerry Franklin was a "rotator" or temporary program officer at NSF's Ecosystem Studies Program from 1973 to June 1975. Tom Callahan, who would later be the program officer for the LTER program, was his assistant. Franklin recalls the climate at NSF at that time[1]:

> I arrived at NSF in September 1973 to take a rotator position as Program Officer for the Ecosystem Studies Program and continued in that capacity until June 1975. My boss was Dr. Eloise "Betsy" Clark (who was then a divisional director within the Biological and Medical Sciences Division, and from 1976–1983 was assistant director of the new Directorate for Biological, Behavioral, and Social Sciences created after a major reorganization of NSF). Permanent NSF staffer Tom Callahan was my assistant. My initial duties included reassuring folks in the Biome programs that their funding was not going to terminate abruptly with the end of the International Biological Program. My understanding was that this was because the funding for IBP that Congress had added to the NSF budget was going to be rolled over to support an expanded Ecosystem Studies Program in FY 1974. Other immediate activities included working with Hubbard Brook folks on continuation of their funding and dealing with complex policy issues related to NSF funding research activities at Oak Ridge National Laboratory. The job often got interesting around and during panel reviews as evaluations of ecosystem research proposals were being done by the regular ecology panel, which was composed predominantly of traditional scientists, many of whom were very unenthusiastic about ecosystem science!

Franklin remembers many discussions with other program officers in the division concerning long-term ecological research:

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[1] All quotations of Franklin, except where otherwise noted, are from an essay written for inclusion in this chapter.

> I remember many sessions Tom [Callahan] and I had over lunch with Bill Sievers, the program director for Biological Research Resources (BRR Program) in the nearby General Services Administration cafeteria. There were also larger, more formal brain-storming sessions occurring within the staff of the Division of Environmental Biology. There were many converging concerns in NSF with developing mechanisms to provide for longer-term support for ecological research. This was because many questions in ecological science – particularly ecosystem science -- needed to be approached using long-term experiments and installations at field locations. This research also typically needed to be conducted by interdisciplinary teams. It was clear to NSF staff that research of this sort simply was not possible using the typical 2–3-year NSF grants that provided support for individual academic scientists with one or two graduate students. There also had long been a concern over the need to sustain field stations and other important field research facilities.

The Hubbard Brook long-term ecosystem studies in New Hampshire had provided a model of interdisciplinary research and had strong policy implications, especially on the problem of acid rain in the U.S., one of the key findings of this study (see also Chap. 8, this volume). As Franklin notes, it was “an early and outstanding example of problem-relevant science.” The IBP had also “generated strong field-based interdisciplinary programs and there was a desire to provide continuing support for meritorious groups that had emerged from that and other initiatives.” Finally, as Franklin recalled, “there was in NSF at this time an increased concern over support for more problem-oriented (‘applied’) research, as well as broader pressures within the federal government for a significant expansion in ecological research and education,” which the Council on Environmental Quality, an advisory group to the President, had recommended in 1974 (Council on Environmental Quality 1974). These discussions occurring within NSF prompted the exploratory initiatives that constituted the immediate planning process leading to the formation of the LTER program. Franklin had a key role in all these discussions. We turn in the next section to these various reports and workshops.

2.4 Planning for LTER: The Institute of Ecology’s Report and NSF Workshops

The Institute of Ecology’s planning committee for its project on “Experimental Ecological Reserves” was the first stage directly leading to the LTER program. Franklin participated in this committee. The key recommendation of the report, which distilled over a decade of thought within the scientific community of ecologists, was to establish a “network” of field research facilities, or experimental ecological reserves, for long-term research. The report provides insight into how the “network” was conceived by the scientific community, and the level of consultation that went into its recommendations. Discussion of the study involved more than 40 groups before it was submitted to the National Science Foundation, and more than 300 scientists contributed to the project. The planning committee of 11 scientists represented a variety of government, academic, and private groups, including members from the Forest Service, the Nature Conservancy, the Organization of Biological Field Stations, and several universities (The Institute of Ecology 1977).

In this report, we see the fuller expression of the rationale behind creation of a network of ecological experimental reserves, as articulated by the community of ecologists. There are three outstanding differences between this report and the later LTER program. One was a difference of scale: the original network was imagined to be far larger and more comprehensive than the LTER program, a vision that was unrealistic given the budget constraints. The LTER Program in contrast was not originally designed as or expected to be a network (as will be discussed later in this chapter as well as in Chap. 12).The second was the synergistic relationship that was being promoted between basic and applied ecology, or between basic research and resource management, which was originally envisioned by the TIE planning group as central, but which was not emphasized in the later NSF program announcement. The third was the management strategy or administrative structure that would maintain the network. The report recommended creating a consortium of Federal agencies, universities, and private institutions, which would "implement and maintain" the reserve system "in the nation's best interest" (The Institute of Ecology 1977, p. 28). The eventual LTER program would abandon this concept of creating and managing reserves altogether, and also the idea of a consortium, which was modeled on TIE's structure, but could not have been adapted to the research-oriented LTER program.

The goal of the scientists writing this report was to raise the level of long-term place-based research across the discipline of ecology and across the United States and its territories. They envisioned a very large network of 71 existing field sites in 67 locations across the U. S. (including Alaska, Puerto Rico, and the marine environments of the Virgin Islands). These were selected to ensure that a wide variety of ecosystems would be included. About three-quarters of those sites had some research facilities and ongoing programs already, but not all had facilities to support long-term experimental research, so capital improvements were included in the plan.

Funding of the network was considered to be relatively modest given the scale of the enterprise, but was still substantial. Estimates (based on 1975 dollars) were that it would require over $73 million to make the capital improvements that would make all sites sophisticated, year-round experimental reserves, with annual operating costs over $15 million at that level. The lowest estimate, which envisioned all sites having minimal facilities for long-term research, was $270,000 for capital improvements and just over $390,000 for annual operating costs. The highest estimate would have made this program into a Big Science project for ecology, although at about $300 million over a 15 year period, it was less expensive than other Big Science projects being contemplated around that time, such as the Human Genome Project (an international project on which discussion began in the mid-1980s and which cost $2.7 billion in 1991 dollars over a 13 year period). In TIE's report we see a pattern of creating very ambitious plans that were too expensive to be executed, and that were significantly scaled back when implemented. (Much later, a larger and more expensive program did ultimately emerge, the National Ecological Observatory Network.)

The report advocated adopting a systems-level approach, one that aimed at understanding how ecosystems functioned and how they were affected by human activities. "As ecology moves from a descriptive toward a predictive discipline," it noted, "scientists need access to sites at which to test their hypotheses" (The Institute of Ecology 1977, p. 6). Being "predictive" therefore meant performing experiments and testing hypotheses. Those tests would involve experimental manipulations, perhaps perturbing a site to assess its response. Long-term studies were essential because experiments might involve changing the site environment, and ecological effects might take time to emerge.

The report strongly emphasized creation of a "network" for various reasons, but these fell short of fully articulating the meaning of the term "network," or providing a worked-out vision of a new kind of ecological enterprise that was functioning as a network. One justification was to help provide resources to sites, while eliminating redundancy. Another was to create a database that was long-term and that could coordinate or combine the results of independent environmental monitoring projects. The report also envisioned more collaborative and integrated research efforts, including large-scale field experiments made possible by having an enhanced database. However, mindful that ecologists would worry about loss of autonomy through greater centralization, it also recommended that proposals would be selected through a competitive peer-review process, and that funding would come from additional sources, not from existing research funds. These arguments, which rely on wishful thinking rather than realistic assessments of what was possible, suggest that the participants in the planning process were still struggling to develop a new vision of ecological science, did not want to alienate people in the process of planning an expanded scale of research, and could not yet foresee the kind of cultural shift that would be needed to develop a network of sites.

One of the most interesting aspects of this report was the way it advocated a network of ecological reserves specifically in order to link basic and applied ecology. As the report noted, "a new interface is emerging as the need for similar data at the ecosystem level brings basic and applied ecologists into cooperative interdisciplinary studies, which result in both the development of new scientific knowledge and improvement of resource management" (The Institute of Ecology 1977, p. 9). The value of this kind of extensive research network was related to the needs of policy makers, who were tasked with evaluating the impacts of new technologies, new products, and new management strategies on the nation's ecosystems.

The type of work envisioned for this network of sites included very specific practical questions. For example, experiments could be designed to assess the impact of toxic substances on the environment, as a supplement to laboratory tests of toxicity, and therefore guide regulatory agencies who had to set standards for the use of such substances. Another example was to assess the impact of coal and oil extraction on different ecosystems. The reason that a "network" of many sites was needed was because it was important to be able to assess environmental impacts across a range of ecosystem types. The report made a clear link between basic research and resource management or environmental assessment needs, and anticipated that one outcome would be better communication between scientists and resource managers and users.

The management or governance structure of this large-scale network was envisioned quite differently from the eventual LTER program. Since Eloise Clark, the Assistant Director of NSF's Biological, Behavioral, and Social Science Directorate, had not envisioned LTER to be a network or managed by NSF as a network, the LTER Program started with an informal governance structure, although a more formal one evolved later as part of the strategic planning process in the early 2000s. NSF always retained the power to select or to withdraw sites, and it determined the core research areas that all sites had to embrace. In contrast, TIE's report recommended the creation of a new consortium that included Federal agencies, universities, and private institutions, to coordinate research within the network. The rationale was that these institutions held the land containing the sites. But non-land-holding agencies such as the National Science Foundation and the Council on Environmental Quality (an advisory body to the U.S. President) were included, and it was thought that representatives of state and local governments, state and private universities, and private owners of ecological reserves would also be involved.

This vision of governance was extraordinarily inclusive, although it was potentially unwieldy for the same reason, and the report did not appear to recognize that a structure like this might militate against the pursuit of hypothesis-driven science. The consortium concept was similar to that adopted by TIE itself, and seemed intended to provide for a broad or democratic form of governance, one that allowed for input from any agency with a stake in ecological research at these sites, and a stake in how that ecological research would be applied to solve environmental problems. The details of how this consortium would operate were not spelled out, but the proposal included a core staff of people who would handle communications, travel, and coordination of the network.

Given that this was a Big Ecology proposal in terms of its budget, and that with so many sites identified it was also aiming to raise the level of ecosystem ecology across the board, it would have required substantial buy-in from the entire ecological community to pursue this agenda, and substantial lobbying efforts to ensure that funding for this kind of enterprise would flow from different agencies and institutions. But TIE's report was prepared for NSF's Division of Environmental Biology, which could not undertake such a broad and expensive project. Instead the Division responded by encouraging continued discussion, in the form of three workshops held in 1977, 1978, and 1979, which allowed ecologists to present the case for long-term research, and to identify some of the sites where such research would be feasible.

The first workshop did not try to duplicate the full ambitions of TIE's report, but instead adopted a more modest program based on measurement and monitoring. The workshop was chaired by Daniel B. Botkin, who was then at the Ecosystem Center of the Marine Biological Laboratory at Woods Hole, Massachusetts. It was entitled "Long-Term Ecological Measurements," and stressed the need for *measurement* instead of ecological *experiments*, which had been included as one facet of TIE's report (National Science Foundation 1977). NSF was to provide support through its Biological Research Resources Program, which had been established in 1973 and was mainly funding systematic biology, that is, the work of museums and botanical gardens (Appel 2000).

Experimental work was not excluded, but the report of the workshop placed the goal of long-term measurement front and center, and this emphasis made the proposal look like a monitoring program and little more. The repetition of the words "measurement" and "monitoring" in this workshop's report is striking. As Franklin noted, "A comprehensive list of measurements important for terrestrial, freshwater, and marine ecosystems was provided in the report, which was viewed by some critics as a typical ecologist's laundry list of data sets that they would find interesting."

The second workshop in 1978, also chaired by Botkin and convened at Woods Hole, was called "Pilot Program for Long-Term Observation and Study of Ecosystems in the United States," which again outlined "monitoring strategies" across a range of ecosystems (National Science Foundation 1978). One central goal was to distinguish between cyclical changes and unidirectional changes and to distinguish human-caused changes from natural ones. This report also presented a plan for organizing, developing and administering a long-term program, and recommended specific study sites. A minimum set of monitoring sites was proposed that would provide "a representative cross-section of the major ecosystems in the United States" with NSF funding through the Biological Research Resources Program.

NSF was not keen to support "monitoring", however, and the first two workshops did not gain traction because they did not appear to promise enough research. The third workshop, convened at the headquarters of The Institute of Ecology in Indianapolis, Indiana, in 1979, and chaired by Orie Loucks, Science Director for TIE, adopted a different approach. It was titled "Long-term Ecological Research: Concept Statement and Measurement Needs," and it opened with the statement that "the proposed program would have to answer significant ecological research questions if it were to be considered for support by the National Science Foundation" (National Science Foundation 1979).

The language of this report returned to the more forceful research-oriented language of the initial TIE report. It argued that "a strong program of long-term ecological research must encompass individuals, who provide creativity, and institutions, which maintain longevity and continuity." The database generated by such a program would be used to "generate new hypotheses in the future and to formulate and test hypotheses from long-term measurements in the present." The proposal advocated ensuring that these criteria would be met by proposing a "core research program" at a network of sites, as well as an "investigator-specific program" which could be done either at the primary sites or at other locations outside the core network. Data generated would be used to answer local research questions or to make broad comparisons across sites. In this report, scientific creativity, hypothesis testing, and the importance of placing ecological research in temporal and spatial context were emphasized. This vision found favor at NSF, and the LTER program that was subsequently approved adopted much of the same rhetoric and the same goals that the third workshop outlined.

In placing ecological research squarely at the center of the proposal, this workshop sought a balance between common or “network” activities and individual curiosity-driven research. There would be a common core of research areas pursued at all sites, an idea that appeared for the first time in this workshop report, but individuals were given freedom to pursue other questions related to their study sites. The report referred to the common core as “research questions,” but they were really not specific questions, rather they were broad areas of investigation, and there was no requirement that LTER would provide comparable measurements of such phenomena of any kind. These dealt with five topics:

1. Dynamic patterns and control of primary production over time, and in relation to natural and induced stresses or disturbances;
2. Dynamics of selected populations of seed plants, saprophytic organisms, invertebrates, fish, birds, and mammals in relation to time as well as natural and induced stresses or disturbances;
3. Patterns and control of organic accumulation (biomass) in surface layers and substrate (or sediment), in relation to time or natural and induced stresses or disturbances;
4. Patterns of inorganic contributions (atmospheric or hydrological) and movement through soils, groundwater, streams and lakes in relation to time and natural or induced stresses or disturbances;
5. Patterns and frequency of apparent site interventions (disturbances) over space and time (drought, fire, windthrow, insects or other perturbations) that may be a product of, or induce, long-term trends.

In addition to these core areas, the report recognized that investigators would also wish to pursue long-term studies of other problems. Some would be site-specific and others might involve multiple sites. The report noted that the allowance for individual and exploratory research, over and above the five core areas, might in time prove to be significant nationally and therefore might even become part of the core research program in the future.

Recommendations for interagency cooperation, coordination and management of the program, and criteria for grant determination and funding periods also were part of the third workshop report, as was a second major section on the measurements recommended for core research at LTER sites. The report recommended that a network coordination office be created, and that overall policy and review would reside in an “LTER Council”, which was envisioned as including members from land ownership and management agencies (such as the Forest Service or National Park Service), the Department of Energy, universities, foundations, and other agencies with research interests (such as the NSF). This idea harkened back to the inclusive approach of TIE’s report, but was not adopted by the LTER program. The report also emphasized that the data generated by such a program could be used in many different ways, ranging from state and federal resource management and regulatory agencies, the academic research community, and private industry. In this way the report partly captured some of the breadth of vision and the inclusion of applied ecology in the TIE report, in which Loucks had also been involved.

These wide-ranging discussions form the backdrop to the creation of the LTER program. Franklin recalled that there was much internal debate at NSF before it decided to proceed, although he was not privy to these discussions. He did have a conversation in August 1979 with Frank Golley, ecosystem ecologist at the University of Georgia, who was about to become director of NSF's Division of Environmental Biology. Golley asked for his perspective on the LTER proposal because he was doubtful about its merit and unsure whether he could support it. Franklin was given to understand that the request for proposals that NSF finally issued for the program was approved by Eloise Clark (Assistant Director of NSF's Directorate of Biological, Behavioral, and Social Sciences), on the grounds that projects were to be selected upon their individual scientific merits and not as elements in a network. This concern about the intellectual merit of the proposals may have been in Golley's mind as well.

In 1979 Clark approved the release of a Request for Proposals for the initial set of sites, with the proviso that they would be selected on their individual scientific merit. This stipulation was followed in the selection of the sites, although not all staff in NSF's directorate agreed with it. The new program absorbed many of the ideas of the third workshop, including the requirement that sites address a set of five core areas. Unlike the vision set out in the workshop, however, the five core areas did not change in the entire history of the LTER program, despite later efforts from the community to introduce other themes. Nor did it require even a minimal set of comparable measurements that were to be made at all sites, as had been proposed in the TIE report. Otherwise, the vision Loucks and his colleagues presented in the third workshop report became the LTER program.

Unlike the large scope of the project originally imagined in TIE's report, LTER, when created in 1980, was not designed to raise the level of the whole of ecology, but was a smaller scale pilot program that began with six sites in 1980, to which five more were added in 1981 (Callahan 1984). The maximum funding for each site was set at $300,000 per year. Four of the original sites were connected to the biome projects of IBP, including to Franklin's Western Coniferous Forest Biome study.

A Coordinating Committee was created shortly after the program started. It was originally called a "steering" committee. The initial plan was that the chairmanship of this group would rotate yearly among the Principal Investigators (PIs) of the LTER sites. Dr. Richard Marzolf (PI of the Konza Prairie LTER in Kansas) was the chair of the committee for 1981 and was awarded a small coordination grant by NSF. Dr. Richard Waring (from the H. J. Andrews Experimental Forest LTER in Oregon) was scheduled to be the chair the next year, but he asked Franklin to do it in his place and Franklin agreed to do so. At the 1982 meeting at North Inlet, South Carolina, a decision was made on the need for a network office and that this could not very well move each year. Consequently, Franklin was asked to continue to chair the LTER Coordinating Committee for an indeterminate period, to which he agreed. He continued as chair through 1995, which was a period during which the network office and coordination activities made immense growth. At the time of his resignation, the LTER program had grown to 18 sites.

In the next section, Jerry Franklin offers his recollections of the early years of the LTER program, during the time that he chaired the Coordinating Committee. His comments also address the relationship between the individual sites and NSF, and the problem of fostering cross-site comparative research, or of behaving like a "network" as opposed to a collection of independent sites.

2.5 To Be or Not to Be a Network

Jerry Franklin recounts here his experiences and perceptions of the LTER program during his tenure as chair of the Coordinating Committee. His account starts with the Denver 1986 meeting of the Coordinating Committee. John Brooks, Director of the Division of Environmental Biology at the National Science Foundation, took the unusual step of attending the meeting and informing the participants that they had to pay more attention to cross-site comparative work.[2] He was concerned that LTER did not have specific products or results to showcase at a time when there was growing interest in long-term ecological research in Britain and Europe. Since the program was approaching a 10-year review, which was completed in 1993, Brooks hoped that the program would be able to demonstrate unique results that could not have been obtained in another way. In his view, the program needed to incorporate greater time spans, be geared up to include larger spatial dimensions, and develop an approach to comparative ecosystem analysis. As Franklin recollects, Brooks made explicit that if LTER did not begin to function as a network, it would not have a future! His remarks surprised and were taken seriously by all present.

2.5.1 Jerry Franklin on the Early Years of the LTER Program

The LTER sites viewed themselves as independent but with some common interests for the first several years of the program. The Coordinating Committee did agree, as a collective, to adopt the principle that the LTER sites would not compete against each other for advantage in funding. If NSF wanted to provide additional funding to supplement work at LTER sites, it would have to be at all sites. As new LTERs were added in the second and third Requests for Proposals they were appraised of this understanding. Efforts at integration or cross-site coordination were minimal, however, until John Brooks appeared at the Coordinating Committee

[2] Minutes of the LTER Coordinating Committee Meeting, November 8–9, 1986, Denver, Colorado. In the Center for Limnology Document Archive, Steenbock Library, University of Wisconsin Archives and Record Management, Madison, Wisconsin.

meeting in Denver in 1986 and directed, "that we would be a network or LTER would not continue!"

As a group the PIs of the LTER sites were not happy with the NSF direction that we were to become and function as a network. Gradually the sites began to reluctantly step up – on the basis that NSF was going to have to pay for any standardization and additional work, such as in cross-site analysis. However, a breakthrough occurred at the LTER Coordinating Committee Meeting at Kellogg Biological Station in November 1988. With encouragement from many data management personnel, representatives of all the LTERs endorsed adoption of common network hardware and software, which was called the minimum standard installation (MSI), and asked NSF to provide supplemental funding to each LTER for its acquisition. NSF readily agreed to do so.

Major "micromanagement" of the LTER network and supplemental funding, including of the LTER Network Office, really began at this point. Some NSF staff have asserted that they did not conduct such management direction. The majority of the initiatives undertaken by the LTER Network were, in fact, the result of direct requests by NSF. My job, as network office director and chair of the Coordinating Committee, was often functioning as the "rubber bumper" between NSF and the network. An NSF program manager would call me up and say, "Well, we think it is time for there to be an LTER newsletter" (or a book series or a new round of cross-site comparisons or a long-term plan for the network). In most cases I would communicate this to the members of the coordinating committee (a committee composed of one representative from each of the LTERs) to inform them and then develop a supplemental request for additional funding to pay for the proposed activity. In one case, the NSF Deputy Director Mary Clutter contacted me directly with her proposal that we establish an International LTER Committee, which we did.

Rarely was said agreement to such additional activities immediately forthcoming from the LTER sites, particularly when it might involve commitment of their time or funds. Chairing LTER Coordinating Committee meetings was always a personal challenge, particularly because the LTERs were generally led by well-known scientists who had their own strong opinions about things. Several of the LTER PIs clearly were not at all interested in being part of a network; they had their grant and they wanted to get on with their activities without the time and expense of having to be part of a network. There were several individuals with particularly strong egos and voices that generally could be expected to be naysayers regarding new initiatives and I had to learn how to contain them and insure that some of the LTERs represented by individuals with quieter voices also were heard from during discussions and the ultimate votes.

The idea for the All-Scientists meetings did emerge from the LTER group itself. Support for it was a roller-coaster with several ups-and-downs within the NSF hierarchy. I was one of the individuals that conceived and strongly supported it, believing that such meetings were critical if we were really going to have collaboration among the sites. It represented a major expense, however, and at least twice decisions were made in NSF that there wouldn't be any more All-Scientists meetings. These decisions were obviously later reversed.

My participation abruptly ended in 1995. We were preparing a grant proposal for a large new multi-year grant to the coordinating office. After extensive discussions with NSF staff, we had prepared and submitted a final proposal for continued funding of the LTER Network office in Seattle. Late on a Friday afternoon I received a call from Mike Allen, who was the responsible program officer at that time, saying that NSF was not going to fund this proposal. Dr. Mary Clutter, the Assistant Director in the Directorate for Biological Sciences, had decided that the next network office proposal needed to be competed. I burned all remaining bridges with Mary Clutter and NSF in challenging their decision, which would have resulted in an unacceptable gap in funding for network activities had it been implemented as NSF originally proposed. Sufficient interim funds were provided to bridge the time between ending of the current network office grant and awarding of the competed grant at the new location for the LTER office in Albuquerque. Dr. Clutter never forgave me for the Cain that the Vice Provost for Research at the University of Washington and I raised over their abrupt decision to decline the proposal which we had prepared – and following their direction!

2.5.2 Changing of the Guard and the Next 20 Years

James Gosz of the University of New Mexico succeeded Franklin as chair of the coordinating committee, while Franklin continued as director of the Network Office in Seattle. NSF announced an open competition for the LTER Network Office in 1995, and the University of Washington and the University of New Mexico submitted a joint proposal with Gosz and Franklin as lead investigators. The University of California-San Diego and the Santa Fe Institute were collaborators on the proposal. Although the open competition was designed to attract a broad range of applicants to host the Network Office, only two proposals were received. NSF decided to accept the proposal from New Mexico, but again declined to fund the Washington component. As a result, Jim Gosz led the transition of the Network Office from Seattle to Albuquerque in March 1997.

The second seismic shift in the LTER program occurred when Scott Collins replaced Tom Callahan as LTER program officer. The cause of this change is not known with certainty. Callahan himself, as David Coleman recalled, offered no explanation beyond the comment “I was a bad boy,” but Coleman added that the shift might have reflected Mary Clutter’s belief that permanent staff positions should rotate every decade or so (Coleman 2010, p. 103). Although Collins did an excellent job in the position, many people were disappointed that Callahan, who had shepherded the program through its formative period, was removed unceremoniously. Collins continued in the position until 2000, when he became program officer for the National Ecological Observatory Network (NEON). Henry Gholz then assumed responsibilities for the LTER program.

With the addition of two urban sites in 1997, the LTER Network comprised 20 sites when the Network Office moved to Albuquerque. Two of the original sites

(Coweeta and North Temperate Lakes) received augmentations to their budgets for regional-level and interdisciplinary research. The International LTER Network was growing, and Gosz was elected chair of its Network Committee. NSF initiated a competition for new land-margin LTER sites. LTER scientists increased their focus on cross-site research through two funding programs. NSF held a 1995 special competition that supported 13 awards for cross-site comparisons and synthesis at LTER and non-LTER sites. In the same year, seven LTER scientists received funding from the NSF/DOE/NASA/USDA Joint Program, Terrestrial Ecology and Global Change (TECO). The transition of the Network Office from Seattle thus took place in a very positive environment for the LTER program.

The award establishing the LTER Network Office (LNO) in Albuquerque had two important differences from previous awards for the Seattle Office. The new award established the position of Executive Director, whose function was to coordinate the Network Program, oversee the awards and supplements that supported LTER Network activities, and guide the Network Office activities and staff. This new position allowed the Chair of the Coordinating Committee to focus on long-term planning and development of site and cross-site research, education, and collaborations. Because the Coordinating Committee had instituted a policy of periodically rotating the Chair, assigning responsibility for the LNO to an Executive Director increased the stability of the Network Office and the continuity of its programs.

The LNO was supported by NSF through a Cooperative Agreement rather than a research grant, as had been the case in Seattle. Cooperative Agreements are designed to ensure coordination between the Foundation and the awardee and involve frequent communication with and oversight of the awardee. Theoretically, activities and funding under a Cooperative Agreement are re-negotiated annually. The use of this kind of award instrument provided NSF with the capacity to influence the direction of activities in the LTER Network. This hands-on approach resulted in confusion regarding the reporting structure for the LNO since up to then the Coordinating Committee had guided its activities. Under the new award structure, the LNO was viewed by NSF as being somewhat independent from the LTER Network. For example, NSF required a strategic plan for the LNO that was separate from the planning activities undertaken by the Network. As mentioned above, the LNO was set up as a kind of buffer between NSF and the LTER Network, which was a continuing source of tension on both sides.

Bob Waide was hired as Executive Director after a national search for the position. Waide was one of the founding principal investigators of the Luquillo LTER site which he led before moving to the LNO in October 1997. John Vande Castle, Associate Director for Technology, had moved from the Seattle office to Albuquerque, and provided continuity in the transition as well as critical institutional memory. James Brunt, Associate Director for Information Management, had been the data manager for the Sevilleta LTER site and was a trusted member of the LTER information management community. These three individuals continued in their positions until the LNO was moved to Santa Barbara in 2015. The staff of the LNO, which at one point reached 18 individuals, had a broad range of expertise directed at three major activities: development of the Network Information System, coordination and support of Network research programs, and communication and outreach. The staff was augmented by two NSF program officers who were temporarily assigned to the LNO. Christine French, from the Office of International Programs, was posted to the LNO from 1997–2000 and played an important role in the development of the International LTER Network. Sonia Ortega, who managed multiple education programs for NSF, joined the LNO from 2001–2004 and worked with the LTER Education Committee on strategic planning.

In 2000, the LTER Network defined the central, organizing intellectual aim of the LTER program as an effort "to understand long-term patterns and processes of ecological systems at multiple spatial scales."[3] Over the following 20 years, the LTER Network endeavored to achieve that aim through six interrelated themes. We summarize the results for each of these themes, noting chapters in this book that bear on each theme. For a more detailed description of the accomplishments of the LTER Network over the last decade, consult the Long Term Ecological Research Network Decadal Review Self Study.[4]

Understanding: Gaining Ecological Understanding The foundation of the LTER Network is place-based, long-term research conducted at each LTER site. Although sites share common research themes, each site creates a research program that addresses their particular environment. Overall, LTER sites have been extremely effective at developing in-depth understanding of a diverse range of ecosystems and extending that understanding to broader geographic scales. Rates of scientific publication have increased fourfold since the beginning of the LTER program, and LTER sites are considered as unique resources for scientists from many disciplines. Three examples of how LTER sites gain ecological understanding are presented in Chaps. 5, 6 and 7.

Synthesis: Create General Ecological Knowledge Comparative research took hold early in the development of the LTER program. The breadth and depth of these comparative studies increased over time until LTER became a closely integrated research network (Chap. 12). As a network, LTER developed a conceptual model integrating social and ecological sciences that has been adopted by many scientists outside the LTER community (Chap. 14). LTER science has contributed to the advancement of our understanding of many fundamental ecological principles.

Provide a Legacy for Future Scientists The LTER Network has provided two kinds of legacies for the benefit of future scientists. The first is the existence of well-designed and documented long-term observations, experiments, and samples that provide a baseline for detection of future change. Many of these observations and

[3] LTER 2000–2010: A Decade of Synthesis. https://lternet.edu/wp-content/uploads/2010/12/lter_2010.pdf. Accessed 31 May 2020.

[4] Long Term Ecological Research Network Decadal Review Self Study. https://lternet.edu/wp-content/uploads/2019/10/LTER_Self_Study_2019-10-04.pdf. Accessed 27 July 2020.

experiments are designed to continue under the leadership of generations of future scientists.

Information: Create Accessible Databases The second legacy is the archive of well-documented data from LTER sites representing many of the biomes of the United States (Chap. 13). In addition to the data itself, the LTER Network created a language for documenting ecological metadata that has been adopted by research networks around the world. The LTER Network Information System is the forerunner of the Environmental Data Initiative, which allows the broader scientific community to benefit from LTER's four decades of experience in managing ecological data. The influence of LTER information management has been felt in other ways. For example, William Michener of the LNO was the first Principal Investigator and Director of DataONE, a national facility that provides access to Earth and environmental data across multiple member repositories.

Education: Promote Training, Teaching and Learning Because most LTER sites are located at or managed by academic institutions, they provide excellent opportunities for undergraduate and graduate study and research. The LTER Network has also been very successful in attracting students from outside the host institutions to work at LTER sites. Many of these students have continued to work at LTER sites as technicians or researchers once they have received their degrees, and LTER leadership is sprinkled with scientists who began their academic careers at LTER sites. Equally impressive is the development of a Network-wide Schoolyard LTER program serving K-12 students. Despite minimal funding, Schoolyard LTER has become one of the best-known accomplishments of LTER.

Outreach: Provide Knowledge to Address Complex Environmental Challenges The dissemination and application of knowledge based on LTER research is a key goal of the program. Sites in the LTER Network, especially those sites associated with Federal agencies like the Forest Service, are well positioned to inform resource managers and policy makers and have had many notable successes with such collaborations (Chaps. 8 and 10). Informing the general public is a challenging task that LTER sites have approached in a variety of innovative ways, including framing the scientific message in ways that are more friendly to the user (Chap. 9) and utilizing non-traditional modes of communication (Chap. 11). Dissemination of information to the broader ecological community is done traditionally through publications (approaching 20,000 for the Network as a whole) and presentations at scientific meetings. The influence of the LTER community is felt by ecologists in other ways. For example, the valuable experience LTER has accumulated in managing ecological data is shared through training, publications, and access to the Environmental Data Initiative services. LTER scientists have often played important roles in developing other national (e.g., Critical Zone Observatories) or international networks (e.g., Global Lake Ecological Observatory Network, International LTER Network: Chaps. 15 and 16). The National Ecological Observatory Network (NEON) is a continental-scale observation facility that was designed to collect long-term ecological data. LTER scientists provided expertise in designing NEON, and two LTER scientists (Bruce Hayden of the Virginia Coast Reserve and William Michener of the LNO) led the first effort to formalize that design. These are just a few of the ways that the LTER Network has contributed to better understanding and management of key national ecosystems.

2.6 Conclusion

The LTER program both reflected and responded to ecologists' ideas, especially those articulated in the third NSF-funded workshop, but it also differed in crucial ways from the ecological community's original ambitions, as presented in TIE's report. That vision could not be realized in the funding climate of that time because of its scope and cost, but it was also in crucial ways different from LTER in conception. It was broader, more inclusive, and had a clear emphasis on applied problems. As an experiment and a pilot project, LTER was far less ambitious in its scope and budget. LTER did not adopt the somewhat unwieldy idea of a multi-institutional consortium to manage the network and did not place the goal of linking basic and applied science at its center in the way both the TIE report and the third workshop report sought to do. It did include the idea of partnering with other agencies (such as the Forest Service) and it adopted many of the recommendations of the third workshop, including the emphasis on research and the identification of five core research areas that would be common to all sites. LTER also did not adopt even a minimal set of common measurements, as the TIE report had proposed; had it done so, developing inter-site comparisons probably would have been much easier.

But even here there was an interesting difference between what NSF did, and what the ecological community thought would occur. The LTER program that was created was smaller and perhaps also less responsive to community input than the planning documents envisioned. But LTER did evolve over four decades, and in some respects it evolved more in the direction of the original vision proposed in TIE's report of 1977, but not without experiencing growing pains. This evolutionary process will be examined further in subsequent chapters. One of the central themes in the evolution of the LTER program, one that emerged especially strongly in the mid-1980s, was whether this group of independent sites could evolve into a network by promoting cross-site research and collaborations. It is clear from Franklin's comments in this chapter that this pressure to become a network largely came from NSF, and represented a shift toward greater control of the LTER program by NSF. But Johns Brooks' presentation at the 1986 meeting in Denver suggested that NSF was trying to protect the program by making demands for more cross-site research, as well as for research at expanded temporal and spatial scales; it probably also represented his original vision for LTER, which differed from his boss at the time, Eloise Clark.

The solution to this problem of how to function like a network had to be devised by the community of researchers at the sites, with help from the LTER Network

Office. The problem of how to behave as a "network", although implied in the early discussions of LTER, had not been thought through in a deliberate or systematic way from the start. Several of the chapters in this book – notably those by Magnuson and Waide (Chap. 12), Stafford (Chap. 13), Collins (Chap. 14), and Zimmerman and Groffman (Chap. 15) – explicitly address the question of what it meant to operate as a "network", and related to that, what new ideas, resources, technological developments, cross-disciplinary connections, governance structures, and cultural changes were needed to support this new notion of "network science."

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