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In an initial research proposal of December 1969, the scientists of the Coniferous Forest Biome (CFB), an ecosystem study centered in the Pacific Northwest and part of the larger International Biological Programme (IBP), expressed optimism that computer simulations and systems modeling could transform empirical knowledge of the carbon, water, and nutrient flows turned into viable forest management practice. The CFB's strategy aimed to use projections of the computer simulations and data from field study to constantly check and direct each other, resulting in a flexible, refined, and accurate understanding of forest ecosystems, as well as a reliable guide to forest management. To what extent did the CFB's research program, centered on a total system model, complete its cycle of field study, modeling, and validation? Despite the innovative strategies of the CFB modelers, ecosystem modeling lost its preeminent status among the goals of the CFB, due to different interpretations of the purpose and philosophy of ecosystem modeling and the practical limitations of administering a large research

program. Instead, small field-based studies during the CFB yielded a number of ground-breaking discoveries.

Although they diverged from the modeling objectives, these areas of fieldwork emerged from questions the forest's functions and cycling processes that the modeling efforts of the CFB required. Focusing on the work of CFB participants from Oregon State University and the USDA Forest Service in the H. J. Andrews Experimental Forest, this thesis addresses the relationship between the marginalization of the modeling objectives and the rising centrality of field-based forest studies in the CFB from 1969 to 1980. Given the ongoing legacy of CFB research at the Andrews Long-Term Ecological Research (LTER) site and the later implications of CFB findings in debates over forest policy and management, this thesis also seeks to evaluate the Coniferous Forest Biome as a whole and discuss the role of modeling and field work within large ecological research endeavors more generally.

The Forest and the Mainframe:
The Dynamics of Modeling and Field Study in the Coniferous Forest Biome, 1969-1980

by

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Chair of the Department of History

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I understand that my thesis will become part of the permanent collection of Oregon State University libraries. My signature below authorizes release of my thesis to any reader upon request.

Tulley A. Long, Author

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Several years ago, when I was trying to decide on the right field for my graduate studies, my grandfather, Horton K. Andrews, declared across the dinner table that "the History of Science sounds real interesting." Indeed, it has proven to be just that, and, for the nudge Grandpa Horton unknowingly gave me in this direction, I dedicate my thesis to him.

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INTRODUCTION

The work of large research endeavors can be difficult to evaluate because of the breadth of topics studied, the sheer size of the effort, and the multiplicity of research objectives. The Coniferous Forest Biome, a large ecosystem study of the Western forests of the United States in the 1970s and a joint venture with scientists at the USDA Forest Service's Pacific Northwest Research Station in Corvallis, Oregon State University and the University of Washington, provides a local story in which to address the problems of evaluating such complex studies. As part of the U.S. portion of the much broader studies of the International Biological Program (IBP), the Coniferous Forest Biome (CFB) shared the national objective of building computer models for the major ecosystems in the United States. Between the time that CFB operations began in 1970 and IBP funding ended in 1976, however, the CFB marginalized the original modeling objectives. In their place, a highly productive research program in interdisciplinary, field-based forest ecology at the H.J. Andrews Experimental Forest in the western Cascade Range grew to be the CFB's most notable outcome. Thus, the motivating questions for this study were: Why did the centrality of modeling in the CFB research scheme erode over the seven years of IBP-funded research? What field-based research goals took priority over modeling during the course of the CFB, and how did these new emphases arise out of the initial research program?

Framed in terms of their later implications in forest policy debates and the broader cultural understanding of forests' economic, aesthetic, and ecological value, the productive field studies of the CFB have been of great interest to writers and scholars over the last decade. Several popular books and sociohistorical studies have focused on the eventual role and effectiveness of the Andrews group in shaping forest policy and the process by which the modern concept of "old-growth" arose from the studies at the Andrews since the 1970s.[1] These studies tend to project the meanings and values later attached to the Andrews findings onto the IBP-era research and, in doing so, often misinterpret the motivations of the research, idolize the scientists, and romanticize the setting in which the work was done. Accounts of the IBP's inception, the organization of the U.S. Biome projects, and the state of ecology in the 1960s and 1970s provide excellent insights into issues surrounding the CFB's research, but they do little to flesh out the CFB's history and research objectives.

At the international and national levels, E.B. Worthington and Frank Golley provide thorough explanations of the IBP's initial aims. Golley also

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[1] Alexios R. Antypas, *Translating Ecosystem Science into Ecosystem Management and Policy: A Case Study of Network Formation* (University of Washington, Ph.D. dissertation, 1998); Posy E. Busby, *Preserving "Old Growth:" Efforts to Salvage Tress and Terminology in the Pacific Northwest* (Harvard University, Undergraduate Honors Thesis, 2002); Alston Chase, *In A Dark Wood: The Fight over Forests and the Rising Tyranny of Ecology* (Boston: Houghton-Mifflin Company, 1995); William Dietrich, *The Final Forest: The Battle for the Last Great Trees of the Pacific Northwest* (New York: Simon and Schuster, 1992); Jon R. Luoma, *The Hidden Forest: The Biography of an Ecosystem* (New York: Henry Holt and Company, 1999).

contributes a general critique of the Biomes, using the Grassland Biome as a case study of the program's strengths and failures.[2] In a parallel study to this thesis, Chunglin Kwa also discusses the Grassland Biome in order to explore the appeal and effectiveness of the ecosystem concept and the use of systems analysis techniques as a means of understanding and controlling natural systems.[3] The shifting objectives in the IBP-era research at the Andrews forest reflect the broad, changing currents within the discipline of ecology. Sharon Kingsland addresses the tensions between the practical use of quantitative, predictive computer modeling in ecology and the theoretical aims of qualitative mathematical modeling that reached a peak around 1970.[4] Joel Hagen traces the gradual decline in the 1970s of the ecosystem concept's dominance over the American ecological community, a trend within the discipline spurred by the challenges of a young cadre of evolutionary ecologists.[5] Given the wealth of historical, sociological, and popular treatments of the issues and events associated with the CFB, a critical historical analysis of the CFB contributes toward understanding the ecological science conducted at the H. J. Andrews forest and the context from which the ground-breaking work emerged.

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[2] E.B. Worthington, ed., *The Evolution of IBP*, IBP Series 1 (Cambridge: Cambridge University Press, 1975); Frank B. Golley, *A History of the Ecosystem Concept in Ecology: More than the Sum of its Parts* (New Haven: Yale University Press, 1993).
[3] Chunglin Kwa, *Mimicking Nature: The Development of Systems Ecology in the United States, 1950-1975* (University of Amsterdam, Ph.D. dissertation, 1989).
[4] Sharon Kingsland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: The University of Chicago Press, 1985).
[5] Joel B. Hagen, *An Entangled Bank: the Origins of Ecosystem Ecology* (New Brunswick, N.J.: Rutgers University Press, 1992).

Although the CFB represented the efforts of scientists throughout the Pacific Northwest of the United States, the work that researchers from Oregon State University and the Forest Service's Pacific Northwest Research Station, both located in Corvallis, Oregon, provides a particularly useful focus for evaluation the marginalization of modeling and the rise of field-based studies within the CFB. The modeling efforts of the CFB relied heavily on the research at the Andrews Forest and the uniform set of data on vegetation, litter, water, carbon, and other nutrients compiled from common collection sites on a single watershed within the forest. Numerous research sites, reliable techniques, and regular sampling intervals lent quality and uniformity to the Andrews data sets. These factors provided the CFB's modelers with prime material to create consistent nutrient, water, and carbon budgets for the coniferous forest ecosystem, the conceptual models that served as templates for the computer models. In addition, Andrews researchers produced a number of more qualitative, observational findings during the IBP years, many of which proved to be groundbreaking and seminal to the program of research that developed at the Andrews in subsequent decades. This thesis, therefore, seeks to examine the complex dynamics between the modeler's techniques of systems analysis and the qualitative field studies in the IBP-era ecosystem investigations at the H. J. Andrews Experimental Forest. It will also explain how and why the promising tool of total system modeling lost its original importance in favor of the field ecology conducted on the ground and in the canopy of the forest.

To this end, the study will focus on the shift in research approaches and goals in the CFB from its planning stages in 1968 and 1969 to the end of IBP-funded research in 1976. In order to place this change within the CFB in its proper context, the thesis will be structured as follows. Chapter 1 provides the background of the IBP and the Biome programs in the United States and a discussion of the dynamic state of ecology in the late 1960s and early 1970s. The chapter also examines the general optimism about the ecosystem concept as a theory and systems analysis as a tool, as well as the significance of the CFB and its initial objectives within this broader context. Chapter 2 elaborates on the central and integrative role of modeling in the CFB and analyzes how changes in Biome personnel, development in modeling philosophy, and technical and interpersonal difficulties affected the marginalization of the modeling objectives for the Biome. Chapter 3 traces the growth of a strong network of field ecologists and their findings within the model-centered structure of the CFB. In particular, the chapter focuses on how the questions about ecosystem function that were necessary to develop the model motivated breakthroughs in understanding of the complex structure of the forest. Finally, a conclusion summarizes the conditions and motivations behind the shift away from total system modeling and provides a forum to evaluate of the science of the Coniferous Forest Biome as a whole.

CHAPTER 1: Ecosystem Ecology and the US/IBP: Conceptual Foundations and Practical Applications

Before examining the dynamics at work within the Coniferous Forest Biome, a discussion of several aspects the Biome's scientific, political, and cultural context is necessary. Long before the CFB initiated research planning in 1968, the International Biological Program struggled to establish a common theme for the worldwide research endeavor, eventually agreeing to focus investigations on biological productivity as it pertained to human welfare. During the 1960s campaign for a US/IBP program, similar debates over the best interpretation of the broad IBP theme ensued. Eager to strengthen its tenuous state within the American scientific community, the discipline of ecology persistently promoted an ecosystem approach to studies of human well-being and biological productivity, aided by the perceived potential of the ecosystem idea to address contemporary environmental anxieties. A brief history of ecosystem ecology, its concepts and techniques, disciplinary tensions, and practical applications shows how the U.S. proponents of the IBP shaped the objectives around ecosystem ecology and adopted a strong emphasis on system analysis. The arguments and concerns surrounding ecosystem ecology came together during the Congressional Hearings on the IBP in the summer of 1967, which clenched governmental support for the IBP in the United States. This chapter will trace the factors that influenced the development of the US/IBP as a predominantly ecosystem-oriented project.

The Origins and Objectives of the US/IBP

Although the International Biological Program eventually became a major vehicle for the promotion and development of ecosystem ecology in the United States, the project began life in 1959 as the idea of a British biochemist. Sir Rudolph Peters, president of the International Council of Scientific Unions (ICSU), envisioned a biological equivalent of the highly successful International Geophysical Year (IGY) of 1957-58, a multinational program of coordinated studies on global geophysical phenomena recently sponsored by his organization.[6] The idea of a similar endeavor in biology sparked immediate interest. Still, five years of planning passed before active research began under the aegis of the International Biological Program. In that period, the program's objectives underwent several reorientations, first internationally and then within the individual national programs. In the United States, advocates of the IBP promoted the growing field of ecosystem ecology as a focus for the national effort. Such an interpretation of the general IBP goals was by no means inevitable, however, as many research topics fell within the objectives of the program.

Peters' suggestion of an international investigation of nucleic acids only represented a fraction of the biological sciences. A range of biological specialties demanded broader consideration. A 1960 meeting of the ICSU executive committee in Lisbon accepted the overarching theme of "The Biological Basis of Man's Welfare," and a preparatory committee convened several times in the

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[6] E.B. Worthington, *The Ecological Century: a Personal Appraisal* (Oxford: Clarendon Press, 1983) 160.

following year to hone the large objective down to a reasonable number of project areas. Prominent practitioners in nature conservation, plant genetics, and human heredity proposed the IBP should focus on their own fields of interest. Other committee members, including C.H. Waddington, a Cambridge geneticist and newly-elected President of the International Union of Biological Sciences, insisted that none of these subject areas addressed the IBP's theme adequately. Waddington insisted that the program should focus on a few topics that were "indubitably of major social and economic importance for mankind."[7] But what central scientific topic met social needs as diverse as food production, human health, and resource management? Waddington, despite his professional investment in genetics, suggested the general area of biological productivity, investigations into "the way in which solar energy is processed by the biological world into the formation of complex molecules which man can use, as food or otherwise."[8] By 1963, the opposing views on the IBP's research focus agreed on a structure of research subcommittees. A section was devoted to "Human Adaptability" to preserve the initial interest in physiological and genetic studies, while multiple sections organized around different aspects of biological

[7] C.H. Waddington, "The Origin," in *The Evolution of IBP*, E.B. Worthington, editor (Cambridge: Cambridge University Press, 1975) 4-5; Strong support for this view came from two Soviet scientists, Vladimir Engelhardt and Andrei Kursanov, who believed the application of fundamental biology "towards the betterment of mankind" should be the IBP's organizing principle.

[8] Waddington 5.

productivity, ranging from the metabolic bases of productivity to the productivity of marine, freshwater, and terrestrial communities.[9]

Additionally, Waddington insisted that ecosystem ecology might be an especially useful framework in which to study how the living world transmits and stores energy. Still, the United States was the only national project to base its efforts around the ecosystem concept. In its simplest form, the concept of the "ecosystem" described a complex of the organisms and the physical environment which, together, forms a functional unit with its own emergent properties; thus, ecosystem ecology was the study of the interconnections between the biological and non-biological components of the unit. In the years after Sir Arthur Tansley initially coined the term "ecosystem" in 1935, the discipline of ecosystem ecology coalesced, drawing on the provocative insights from fields such as thermodynamics and information theory. The field tackled a number of fundamental questions: What factors influence the primary productivity, the storage of energy as sugar through photosynthesis? What functional relationships enable the storage and cycling of energy, water, and nutrients within an ecological network? How do animals, plants, and bacteria influence these processes and the non-biological environment?

By 1953, when the ecologist Eugene Odum first published a full explanation of the ecosystem concept in his textbook, *Fundamentals of Ecology*, these questions had resonance within the social sphere, as well. The recognition

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[9] Waddington 7.

that humans as integral parts and manipulators of these complex natural systems emphasized the need for a better understanding of the ways human actions impact the other components of the system.[10] In order to manage the natural world for long-term human benefit, especially the biological productivity upon which all life depends, the structure and function of ecosystems had to be understood.[11] The holistic elegance and direct utility of the ecosystem held the ability to inspire a collective effort of ecologists in the US/IBP toward this goal, a phenomenon that can be understood in the context of the maturation of ecosystem ecology's concepts and techniques in the 1950s and 1960s.

The Development of Modern Ecosystem Ecology

In the early 1960s, as the IBP was in its initial stages of planning, ecosystem science was just beginning to emerge as a formal field of study, transforming what had been a traditionally descriptive science into an experimental one, with unexpected allies in funding, as well as new methods and tools.[12] Historians and scientists alike commonly trace the birth of modern ecosystem ecology to the work of the young ecologist, Raymond Lindeman. Lindeman's 1942 paper, "The Trophic-Dynamic Aspect of Ecology," built upon the concept of food chains in ecological relationships and transcended the older idea by reducing explanation of ecosystem dynamics to the common denominators of energy and energy flow. As the historian of science Joel Hagen argued, food

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[10] Golley 66.
[11] George Van Dyne, "Preface," *The Ecosystem Concept in Natural Resource Management*, edited by George Van Dyne (New York: Academic Press, 1969) vii.
[12] Worthington 164.

chain studies were firmly rooted in natural history, with its emphasis on description and with biological species as the basic unit of study. Lindeman's work lent itself well to empirical measurement of ecosystem function and opened the field of ecology to insights from the physical sciences.[13] In 1954, a pair of brother ecologists, Eugene and H.T. Odum, successfully put Lindeman's ideas into action by measuring the metabolism of a whole ecosystem, a coral reef at Eniwetok Atoll in the South Pacific's Marshall Islands.[14]

The landmark Eniwetok study, which established the reputations of the Odums and proved the power of Lindeman's conceptual framework, also drew upon another important factor in the growth of ecosystem ecology. The military development of nuclear devices during the Second World War and the avid testing of nuclear weapons that followed in subsequent years provided ecologists with new techniques and funding opportunities. Radionuclides, radioactive isotopes of common elements, could be easily traced as they traveled through an environment, providing an innovative method to track nutrient cycling and offering a new way of elucidating the function of the ecosystem. Ecologists soon began using the new technique, and centers of this new "radiation ecology" sprang up around the nuclear research facilities in the United States, such as Oak Ridge National Laboratory in Tennessee and the Savannah River nuclear reservation in Georgia. The Atomic Energy Commission (AEC) also became the main funding agency for the nascent field of ecosystem research. In addition to the fortuitous connection

[13] Hagen 98-99.
[14] Hagen 101-105.

through the techniques of radiation ecology, the science's potential as an aid to regulation and mitigation of possible environmental impacts of atomic power caused the agency to commit to the ecosystem concept.[15] Indeed, the relationship between ecosystem ecology and nuclear proliferation in the wake of the Second World War has been described as both a symbiosis and a "double-edged sword." The fact that the very technologies capable of inflicting severe damage on the natural world were also those that provided ecologists with powerful tools and insights to assist human and environmental well-being is one of the great ironies in the history of ecosystem ecology.[16]

In addition to the advent of radiation ecology, the AEC's sponsorship of ecosystem studies enabled the development of techniques in systems theory and analysis, including computer simulation and mathematical modeling, in ecological research. H.T. Odum, who developed a productive conceptual model that analogized energy flow in an ecosystem to an electrical circuit, pioneered this branch of ecosystem ecology, which came to be known as systems ecology. With the technical and financial support available from the AEC, Oak Ridge National Laboratory's Environmental Program became a hub of systems ecology, attracting the funds to purchase expensive computers and the talent of three first-rate ecosystem modelers. Jerry Olson, Bernard Patten, and George Van Dyne viewed the systems models as crucial to the study of Eugene Odum's conceptualization of complex ecosystems. Such models were valuable tools to hypothesize and test

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[15] Hagen 119.
[16] Hagen 100-101.

suppositions on large natural systems that, otherwise, might not lend themselves easily to experimentation.[17]

The Oak Ridge systems ecologists also placed emphasis on the ability of ecosystem modeling to inform practical applications, like natural resource management, with the insights of ecology. Systems ecologists viewed ecosystem modeling as an efficient way to weigh the many variables of a natural system, ascertaining more quickly the most promising management strategies for balancing social needs and ecological functions.[18] Apart from the use as tools in the practice of management, Van Dyne and others also envisioned ecosystem models as vehicles to educate new natural resource scientists about the properties of whole ecosystems and instill ecological perspective in the future generations of resource managers.[19]

Moreover, there was hope among systems ecologists that models could strengthen ecological theory, a unifying feature that was noticeably lacking in ecology in the 1960s. American scientists worried that the IBP lacked the solid theoretical structure that had made the IGY so productive, instead having "much

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[17] Golley 95-97.
[18] Charles F. Cooper, "Ecosystem Models in Watershed Management," *The Ecosystem Concept in Natural Resource Management*, edited by George Van Dyne (New York: Academic Press, 1969) 322-323.
[19] George Van Dyne, "Implementing the Ecosystem Concept in Training in the Natural Resource Sciences," *The Ecosystem Concept in Natural Resource Management*, edited by George Van Dyne (New York: Academic Press, 1969) 328.

breadth but little depth."[20] Critics maintained that ecosystem management could only be effective if the underlying ecological concepts were sound. As the US/IBP formulated its research strategies around ecosystem ecology in the 1960s, IBP promoters asserted that ecosystem theory, as validated by systems ecology, would provide an overall explanatory framework for the ecological phenomena of energy flow and nutrient cycling, a solid basis for ecological management, and a reputation for the discipline itself. In some ways, the intense focus of the US/IBP on the large-scale ecosystem studies of the Biome projects represented a test of ecosystem theory as an effective conceptual foundation.[21] As exciting and elegant as the ecosystem concept was in the 1960s, the stakes were still high.

The Ecosystem Concept's Centrality in the US/IBP

The focus on productivity studies, while not explicitly an outline for a research program in ecosystem ecology, provided an opportunity for such a program to arise. Indeed, the IBP effort in the United States took advantage of the occasion and coalesced around the ecosystem concept. Nevertheless, the apparent enthusiasm for ecosystem ecology during the US/IBP, as well as the generous funding accorded to it, was a somewhat unlikely phenomenon. Waddington made the observation that the most vigorous objections to the IBP's proposal occured in

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[20] W. Frank Blair, *Big Biology: The US/IBP*, US/IBP Synthesis Series, No. 7 (Stroudsburg, PA: Dowden, Hutchinson, and Ross, Inc., 1977) 14.
[21] For further discussion of the ecosystem as a unifying ecological theory and the challenges that it received from the rising theoretical vigor of evolutionary population ecology in the late 1960s and early 1970s, see Hagen's chapter 8, "Evolutionary Heresies," as well as Sharon Kingsland's chapter 8, "The Eclipse of History," in *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: The University of Chicago Press, 1985).

the United States, where geneticists, microbiologists, and molecular biologists represented the majority of the biological establishment and had "much less hesitation in asserting, in the hearing of government or the academy, that any organism bigger than *E. coli* served only to confuse the issue."[22]

Apart from conceptual differences, American biologists expressed concerns centering on the potential difficulties of administration and practical issues of implementing a large-scale research project. An *ad hoc* committee of the National Academy of Sciences formed to evaluate support of American IBP involvement. In a questionnaire circulated by this committe, a number of concerns surfaced. Many of the respondents doubted that the biological community would significantly change their research aims and methods while participating in the IBP. If participants failed to align their research to the IBP objectives, the value and effectiveness of the program would be compromised. Furthermore, whereas the IGY set simple goals for its field investigations and the data gathered "contributed *conceptually* to geophysics," critics complained that the proposed IBP did not have the same value, stating that "there is little, if any, conceptual framework" in which to synthesize findings and develop theory.[23] Biologists also worried that American involvement in the IBP might drain manpower from important scientific work into committee meetings and other bureaucratic

[22] Waddington 8-9; Waddington does not see the British and American biological communities as identical and, rather, differentiate the two schools of biology in several ways. One of these revolves around the fact that there are a few American field biologists and ecologists who have gained more clout than their peers in Britain and felt that "productivity is not an American problem."
[23] Blair14.

functions, resulting in a loss of research time and a decrease in output. "If more of these outstanding intellects were freed from conference obligations," one respondent noted, "is it not possible that our quote (sic) of Darwins and Einsteins might be increased."[24]

This last comment, as with many of the criticisms, seems to imply that biologists and ecologists in the United States saw a dearth of scientific leaders and breakthroughs in their field and keenly sought to establish and maintain their reputations as valid disciplines. Some biologists expressed concern over the potential for failure of such an enormous research effort and the resulting impact on the reputation of their fields. The state of professional ecology in the United States was more tenuous still, as ecologists were relatively weak in numbers and institutional support compared to other biological disciplines.[25] These ecologists voiced deep concerns that their field was not better represented in the NAS, the body that ultimately oversaw the US/IBP.[26] The appointment of Roger Revelle, an oceanographer and geophysicist, as the chairman of the U.S. National Committee on the IBP in 1965 seemed to prove the NAS's "arrogant disregard" for the IBP's central ecological objectives, angering ecologists despite Revelle's strong credentials in international science.[27] Revelle eventually stepped down in 1968, and the ecologically-inclined zoologist W. Frank Blair assumed the chairmanship of the National Committee. Likewise, leading ecologists eventually comprised a

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[24] Blair 11-13.
[25] Waddington 7-9.
[26] Blair 8.
[27] Blair 21.

large portion of the National Committee and held prominent roles within the subcommittees. The freshwater ecologist Arthur Hasler from the University of Wisconsin, the marine biologist Bostwick Ketchum of Woods Hole Oceanographic Institution, and the ecosystem ecologist Eugene Odum from the University of Georgia chaired the Freshwater, Marine, and Terrestrial Productivity groups, respectively.

Odum's terrestrial group was particularly influential on the course of the IBP in the United States, drafting a national program statement that suggested a completely different style of ecological research with a new set of tools. The terrestrial productivity team proposed that research be conducted within "landscapes," with consideration of both freshwater and terrestrial environments and the functional connections between them. Another crucial attribute of the Odum group's report was the idea of using the techniques of systems analysis, such as mathematical modeling and computer simulation, as a tool to integrate the findings of such a large study.[28] These two features reflected the major advances in ecosystem research since 1950. Inspired by the promise of total system modeling and interested in the general IBP goals, a group of scientists gathered for an organizational meeting in Williamstown, Massachusetts, in the fall of 1966.

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[28] George Van Dyne, one of the systems ecologists from ORNL, was a member of the Terrestrial Productivity group and was, no doubt, a strong voice in favor of the inclusion of systems analysis of ecosystem in the program statement. After he left ORNL for Colorado State University, Van Dyne later received the directorship of the Grasslands Biome at Eugene Odum's recommendation. See pages 116-117 in Frank B. Golley's history of ecosystem ecology, and Chunglin Kwa, "Modeling the Grasslands," *Historical Studies in the Physical and Biological Science*, 24, no. 1 (1993): 129-130.

This body enthusiastically endorsed the US/IBP terrestrial group's report. The conference produced a list of objectives aimed at examining the structure and function of whole systems, including goals for organizing data, collaboration, and development of systems analysis techniques, such as total system modeling and computer simulation. Attendees of the Williamstown meeting also suggested research sites should be established within each of six distinct ecosystems in the United States, an idea that would become the Biome projects of the US/IBP.[29]

With the ecosystem concept reaching new heights of rigor and centrality within the IBP, the American public in the mid-1960s also stood primed for the ecosystem view of nature as an interconnected whole and a fragile balance. The 1962 publication of Rachel Carson's *Silent Spring* emphasized that chemical hazards saw no barrier between the natural and human environments and, likewise, threatened the health of the public just as much as the bird populations. Not only were humans imbedded in intricate and interrelated living systems, their position high on the food chain placed them at severe risk of accumulating toxic substances. Carson's prose brought home the simplified message of the ecosystem and motivated an awakening of grassroots environmental activism.[30]

The growing environmental movement of the 1960s "adapted and adopted" concepts of ecology, but there were, in fact, major differences between the scientific and popular conceptualizations of the ecosystem. On the one hand, the

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[29] Golley 110-119.
[30] Robert Gottlieb, *Forcing the Spring: The Transformation of the American Environmental Movement* (Washington, D.C.: Island Press, 1993) 84-86.

general public's perception of the ecosystem was a fragile "web of life" that, when disturbed or manipulated, would result in serious local and global implications for the welfare of humans. On the other hand, the IBP ecologists considered the ecosystem concept as a framework in which to solidify theory about complex, long-term dynamics of the biological world and the physical environment and build their discipline through the training of new ecologists. Nevertheless, a strong base of ecological scientists and theory would, in time, serve as a platform for management and mitigation of environmental issues. Some ecological thinkers, like Eugene Odum, maintained that IBP could bridge the two conceptions of the ecosystem and play the important role of "catalyzing new ideas and techniques which will make it possible to evaluate whole landscapes within the framework of man's dual role as manipulator of, and a functional component in, ecosystems." Further, for Odum, the IBP's ecosystem studies could provide a scientific basis for understanding and managing of the "whole landscapes" that society resides within and uses.[31]

The scientists also used the popular derivatives of ecological notions to the advantage of the IBP in the United States. As the sociologist of science, Chunglin Kwa, has pointed out, the fact that the planners of the US/IBP promoted ecosystem ecology "as the kind of basic science needed to provide background knowledge for the solution of environmental problems like pollution, proved to be

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[31] Golley 116-117.

of utmost importance in finding funds for the IBP."[32] Certainly, the hearings before the Congressional Subcommittee on Science, Research and Development in the summer of 1967 required the American proponents of the IBP to justify their ecosystem-oriented approach in light of rising concerns about disappearing species, environmental degradation, and the wellbeing of human society. An examination of the hearing proceedings demonstrates the way in which the IBP's defenders advanced the discipline of ecology, even while framing the ecosystem concept within the IBP's social objectives.

The Congressional Hearings

Sent to the Subcommittee on Science, Research, and Development of the U.S. House of Representatives in May of 1967, House Concurrent Resolution 273 endorsed the U.S. involvement in the International Biological Program as a "unique and effective means of meeting the urgent need for increased study and research related to biological productivity and human welfare in a changing world environment."[33] Chaired by Connecticut representative Emilio Daddario, the subcommittee was familiar with the potential of ecology as the scientific basis for environmental management and mitigation after a series of 1966 hearings regarding the "adequacy of technology for pollution abatement." The witnesses, many of whom were active in the IBP, advocated the IBP's ecological focus as a

[32] Chunglin Kwa, "Representations of Nature Mediating Between Ecology and Science Policy: The Case of the International Biological Programme," in *Social Studies of Science*, Vol. 17 (1987), 421.
[33] IBP Hearings before the Subcommittee on Science, Research, and Development, 90th Congress, 1st Session, on House concurrent Resolution 273, May, June, July, and August 1967 (Washington, D.C.: Committee Print, 1967) 1.

promising route to addressing a number of such pressing ecological issues, ranging from food and water shortages and population control to issues of environmental quality. The scientists and the Congressmen found common ground in the urgency and gravity of the matter. Daddario commented gravely that the world had watched "some of the great animal species of this earth just disappear. They continue to disappear at an ever-increasing rate. If this can happen to them, it can happen to us." Roger Revelle, still chairman of the US/IBP National Committee during the initial hearings, was equally somber in his response: "That is right. It undoubtedly will sometime."[34]

The proponents of the IBP echoed the international goal of "human welfare" in the IBP and the currency of environmental concerns among the public. In the words of S. Dillon Ripley, Secretary of the Smithsonian Institution:

> There is an implicit weight of public opinion in this country which could easily express itself in some way in favor of this program. I do not quite know that the position has been explained to them, (but) it is my conviction that this problem is central to the welfare of the American people, and that over a period of time this will become evident. Environmental problems are so central to the welfare of every one of us that there should be no basic difficulty of the Congress to relate to the citizenry and to explain the circumstances of concern which are manifest here.[35]

Ripley implied that the goals of the IBP were so vital to the well-being of society that the program would undoubtedly attract public support and, therefore, governmental support.

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[34] IBP Hearings 34.
[35] IBP Hearings 64.

Before any application of ecology could be effectively made to improve human welfare, however, the base of fundamental ecological knowledge had to be drastically increased. Citing the way human technologies threatened to alter and destroy the fragile "web of life," Revelle observed that the same technology "has outpaced our understanding, our cleverness has grown faster than our wisdom."[36] Ivan Bennett of President Lyndon B. Johnson's Office of Science and Technology gave a similar endorsement of the IBP's goal of accumulating basic knowledge, describing Americans as "space age men in a physical world and stone age men in a biological world."[37] Revelle felt that the notable lack of such basic understanding arose from the fact that ecology

> inevitably tended to lag behind the laboratory biological sciences because it was necessary to get basic information at the molecular level and the cellular level and the organ level before it was possible to understand the whole organisms and their relationships to each other.[38]

Moreover, the witnesses believed that federal funding was not equally distributed among the various branches of science in the United States. S. Dillon Ripley criticized Congress for the lack of governmental support for ecology, despite the discipline's promise in addressing environmental problems that were "central to the welfare of the American people."[39]

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[36] IBP Hearings 2.
[37] IBP Hearings 31.
[38] IBP Hearings 11.
[39] IBP Hearings 60-64; In response, the members of the Congressional subcommittee made it quite clear that the science of ecology was in competition for funding with the war in Vietnam and that, perhaps, the discipline could apply its knowledge to be more helpful toward "security needs" by learning more about

The proponents of a US/IBP also touted the program to Congress as a major vehicle for training a new generation of scientists in ecosystem science. As environmental awareness grows, the witnesses argued, more ecologists would be needed to address the concerns and could be educated through the IBP's efforts. The IBP proponents repeatedly claimed that bright young biologists preferred to go into established fields, like molecular biology, where they could be more assured of their fiscal and professional success, rather than "pioneer areas" that lacked disciplinary prestige and financial incentive.[40] Based on new and alarming environmental problems and the critical and urgent need for basic biological knowledge and scientists to address them, Revelle asserted confidently that "the time has come for ecology," as well as for the congressional support of the discipline.[41]

During the hearings, the scientists endorsed research projects called "watershed studies" that involved "a large number of scientists working on a watershed in its entirety, using the whole ecosystem approach."[42] These studies were the direct outgrowth of Eugene Odum's terrestrial productivity group and its innovative landscape approach and embodied the type of coordinated effort for gaining that basic ecological understanding that had been so acutely lacking. To achieve this whole watershed approach, the terrestrial and freshwater productivity

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tropical vegetation and diseases in Southeast Asia so American troops would not have to learn by experience.

[40] IBP Hearings 33, 55-60.

[41] IBP Hearings 34.

[42] IBP Hearings 73.

groups combined into a single Analysis of Ecosystems (AOE) program in February of 1967, headed by Frederick Smith of the University of Michigan, another systems ecologist.[43] W. Frank Blair spoke excitedly about the AOE program as "the first real ecosystem study," tracking the "movement of minerals, nutrients, pesticides, everything else in the watershed." For Daddario, such watershed studies fell "within the recommendations this committee made when it completed its pollution reports last year: That we needed to in fact do just this kind of thing in order to be able to meet some of the challenges which face us."[44] The rigorous, scientific ecosystem concept appeared to directly address the Congressman's environmental concerns. Blair believed that the AOE, if funded under the IBP, would be of mutual benefit for the nation and the science of ecology. The AOE, he declared, represented an opportunity that ecologists had long dreamed of: an integrated, multidisciplinary attempt to understand the structure and function of ecosystems in their totality.

The disciplinary debates over the lack of a unifying ecological theory also surfaced in the hearings. David Gates, a botanist at Washington University in St. Louis with a strong background in the physical sciences, reported with alarm that "less than a half dozen" American ecologists concerned themselves with theoretical ecology, a field with a "strong fundamental work in biology, but in addition a good training in physics and good training in mathematics and the use

[43] Blair 24-25.
[44] IBP Hearings 73-75.

of computers." Most likely referring to the work of systems ecologists, Gates continued:

> By theoretical ecologists, I mean those people who can take the data that is accumulated in the field from observation and bring it onto the desk and organize it and derive from it causes and effects and out of this theory. Hypothesis and theories really pull all the threads together into a coherent fabric which will give ecology a real body.

Although ecology had the appropriate technologies to build such theory, Gates felt "it is not being done…; without it, we are only in the descriptive stage of this science."[45] Gates' testimony seemed to reinforce the earlier concerns that the reputation of ecology suffered from its tenuous and fragmented theoretical basis. In Gates' opinion, a commitment within the US/IBP to the development of ecological theory by systems ecologists was the only way to allow for further advances in ecology and avoid disciplinary embarrassment.

Generally, the witnesses exuded a genuine excitement for a collective effort to amass basic biological knowledge and for the opportunity for ecology to prove its ecosystem ideas and its utility. For the members of the House subcommittee, as well, the science of ecology seemed poised to aid mitigation of potential environmental catastrophes, a conviction gleaned from the scientists' enthusiasm more than it was based in solid theory and skillful presentation. As evident in S. Dillon Ripley's overt criticism of the distribution of scientific funding, many of the scientists promoting the IBP were "openly contemptuous of

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[45] IBP Hearings 156-163.

the political process on which they so depended for financial support."[46] Just as C.H. Waddington pressed vigorously for the focus of ecology and biological productivity for the IBP at the international level, the successful promotion of ecosystem ecology in the United States benefited from the dynamic and forthright personalities of the Odums, Blair, Revelle, and the Oak Ridge systems ecologists within the US effort of the IBP. The determination of these scientists was sorely tested while they were "selling the IBP in Washington." Three years passed before President Richard Nixon signed the law granting the IBP secure funding in October of 1970. Throughout the hearings, there was evidence that the politicians and ecologists agreed on many aspects of the IBP's merit in theory, as Chunglin Kwa has argued. Still, the struggle to achieve federal backing suggested that the ecosystem ecology may not have resonated in Washington in practice as much as Kwa's interpretation intimated.[47]

The U.S. component of the International Biological Program was the last group to launch its research operations; by many accounts, the American approach of large "landscape" ecosystem studies, the Biomes, was also the most ambitious. Each of the five Biome sites involved massive groups of field scientists, administrators, computer programmers and modelers that aimed their collective efforts toward modeling complex, large-scale systems. In addition to the physical

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[46] Hagen 174; Hagen also mentions quite pertinently the fact that Revelle was the only witness who was not a "political neophyte;" indeed, on examination of the hearing transcripts, the incoherence of many of the statements and answers is remarkable, leading Hagen to note that IBP proponents "sometimes seemed to be their own worst enemies."

[47] Hagen 173-174.

size of the endeavor, the Biomes attempted to coalesce around a concept that still lacked a solid theoretical base and universal acceptance in the American ecological community.

In retrospect, then, it should be no surprise that the US/IBP largely failed to achieve its goals of building the ecological understanding from which to design ecologically-informed resource management and environmental. Theoretical challenges from a determined group of young evolutionary ecologists also deflated the ecosystem's promise as the overarching framework of ecology. Worthington's reflection that the program's "greatest success was in the way that IBP led to recognition of the importance of ecology" in the ensuing "environmental revolution" implies a causal relationship when the actual connection of post-IBP ecological science and popular environmentalism seems more complicated and indirect.[48] Still, the dialogues that began during the founding and funding of the IBP in the United States enabled an attempt at a new type of ecological research, the training of a new kind of ecologist, and the forging of a new relationship between ecology and government. The outcomes of these developments in American ecology ultimately proved applicable to issues of human welfare and important in the realm of natural resource management, though the realization of the IBP's relevance in this respect occurred long after the program ended.

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[48] Worthington 172-173.

CHAPTER 2: The Modeling Efforts of the Coniferous Forest Biome

The enthusiasm of systems ecologists ran high after congressional recommendation of the US/IBP in 1967, and the IBP Biome projects seemed like an opportunity to finally bring ecology to fruition. With the additional incentive of substantial NSF funding, ecologists and biologists in the Pacific Northwest rallied together to write and submit proposals for a Biome project. The NSF funded Oregon State University and the University of Washington as the two primary institutions for the Coniferous Forest Biome, the last Biome project to launch its operations, in September of 1970. The Biome also drew many key participants from the USDA Forest Service Pacific Northwest Research Station in Corvallis. The scientists of the CFB had been at work several years in advance of full funding, planning of their strategies for Biome research, structure, and collaboration. All aspects of the CFB seemed centered on the construction of a total system model, yet the interpretations of and commitments to this goal varied greatly among Biome participants. This chapter will explore the basis of the CFB's modeling philosophy, the different understandings of the modeling objectives, and how these divergent opinions on the proper roles and potential of ecosystem modeling effort in a large, synthetic research project led to the disintegration of the original goals it hoped to achieve.

Establishing and Planning the Coniferous Forest Biome

Infused with the same optimism evident in the defense of the IBP goals in the Congressional Subcommittee the previous summer, fifty-six scientists convened at the Pack Research Forest outside Seattle in February of 1968 in order to formulate a tentative plan for the research of the Coniferous Forest Biome.[49] In December of 1968, the newly formed executive council appointed Stanley P. Gessel as Biome director and quickly submitted an administrative proposal to the NSF that received partial funding in June of 1969. Upon revision and review of research proposal drafts, OSU and UW jointly submitted the final proposal to the NSF in December, 1969, to be fully funded starting in September of the following year.[50] Alhough the Biome office would be at the University of Washington, the researchers decided to have two intensive research sites, one in Washington's Cedar River Watershed to be associated with the UW scientists and one at the H.J. Andrews Experimental Forest on the western slope of the Central Cascade Range to be associated with scientists from Oregon State University and the US Forest Service Station in Corvallis. The Biome justified the two distant sites by the fact that

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[49] In our conversation on 07 July 2004, Dick Waring recalled he and his OSU colleagues were quite excited on the car trip from Corvallis, "talking and driving without seatbelts all the way up and all the way back!" Many of the scientists at the Pack Forest planning meeting traveled much farther, coming from institutions in California, Utah, Colorado, Idaho, and Alaska, in addition to the attendees from UW and Corvallis.

[50] Coniferous Forest Biome, 1970 Proposal (Seattle, Washington: University of Washington, Coniferous Forest Biome Central Office, 1969) Preface, 6.

> it would be difficult, or a least inordinately expensive, to attain our objectives (of a comprehensive study) at either location alone. By combining the resources and integrating the research, a satisfactory intensive study of coastal conifer forest ecosystems is possible.[51]

Indeed, the sites complemented each other. The site in the Cedar River Watershed included Finley Lake and Lake Washington for limnological studies. The Cedar River site also featured abundant young, second-growth Douglas-fir forests that lent themselves well to quantitative studies of tree physiology or studies of elemental and water cycling within individual trees. In contrast, the geography of the H.J. Andrews occupied a series of steep watersheds that were ideal for stream studies and investigation of total system nutrient and water cycling. The forests of the Andrews were significantly older, as well, dominated by 450 year old Douglas fir and Western hemlock. "Research on these stable old-growth stands," the Biome claimed, "is especially timely in view of their rapid disappearance at man's hand and importance in ecological theory."[52] The objectives of better management

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[51] CFB 1970 Proposal 23; The founding and funding the CFB were not without its conflicts between UW and OSU. Jerry Franklin, then of the Forest Service and an ardent supporter of old-growth study, has told interviewers that his efforts to establish the Andrews as the main CFB site for exclusive study of natural, old-growth forests were frustrated by UW's bid for the NSF funds. See Jon Luoma, *The Hidden Forest* (New York: Henry Holt, 1999) and Posy Busby, *Preserving "Old Growth"* (Undergraduate thesis, Harvard University, 2002). While Franklin's irritation with the UW competition is no doubt accurate, Richard Waring's version of the story (interview of 07 July 2004) was much more congenial and in line with the proposal, with UW extending an invitation to OSU to join the Biome. However, with Franklin's account in mind, the allocation of an intensive study site to each of the rival forestry schools could be read as a compromise in a dispute over ultimate jurisdiction in the Coniferous Forest Biome.

[52] CFB 1970 Proposal 23.

through ecosystem ecology were not far from the minds of the founding Coniferous Biome scientists.

Throughout the early proposals of the CFB, the scientists echoed the international enthusiasm for the ecosystem concept by expressing their optimism that investigation of the forest as an ecosystem was both highly practical and immediately necessary in an era of growing environmental awareness and increasing demand on limited forest resources. Understanding the intricacies of forest ecosystems, they asserted, would "provide a basis for more intelligent management and use of these lands and thereby have important social consequences."[53] Furthermore, the CFB researchers were confident that the basic, theoretical knowledge of the carbon, water, and nutrient flows connecting components of the forest ecosystem could be turned into viable management practice through computer simulations and systems modeling.

The Coniferous Biome proposals also emphasized that these models could serve a number of critical functions in the Biome. In addition to representing the main synthetic product of the Biome's research, the models were to act as tools to integrate divergent research interests and encourage interdisciplinary cooperation. They would illuminate the structure and function of the complex ecosystem. The models were, however, also integral to the structure and function of the research and the administration of the Biome as a complex organization. As crucial cogs in the wheel of research development, the CFB's strategy consisted of "process

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[53]CFB 1970 Proposal 1.

study, computer modeling, and validation in that sequence, the procedure being, however, cyclic."[54] Projections of the computer simulations and data from field study were to constantly check and direct each other, resulting in a flexible, refined, and accurate understanding of forest ecosystems. This structure was also designed to facilitate communication between the widely varying interests that came together under the framework of the Biome. The models would bring field and lab, terrestrial and aquatic, and theory and validation into closer contact and promote a more unified view of the ecosystem in all its dimensions.

Alongside this optimism, the limitations of ecosystem modeling were becoming increasingly clear in the early 1970s. As the last Biome project to receive funding from the National Science Foundation, the leadership of the Coniferous Forest Biome took note of the challenges and outcomes of the other Biomes' modeling efforts and shaped their own modeling strategies accordingly. In a progress statement in November of 1973, Biome researchers noted that some of the other Biomes "abandoned the project as unrealistic," while "others pursued the goal with little success or created models too large and complex to be of general use." The authors, Phil Sollins, Dick Waring, and Dale Cole, pointed specifically to the IBP Grassland Biome's model named ELM as an example of an overly complicated model.[55] Indeed, Chunglin Kwa's analysis of the Grassland

[54] CFB 1970 Proposal 11.
[55] P. Sollins, R.H. Waring, and D.W. Cole, "A Systematic Framework for Modeling and Studying the Physiology of a Coniferous Forest Ecosystem," in *Integrated Research in the Coniferous Forest Biome*, Coniferous Forest Biome Bulletin 5, R.H. Waring and R.L. Edmonds, editors (Seattle, Washington:

Biome's model indicated serious problems within the ranks of personnel and in the ambitious modeling strategy. Although ELM was the largest ecosystem model of its time, its size made it an unwieldy and impractical managerial tool that Kwa compared to the extinct Irish elk, "a beast viable in and of itself but too big and clumsy to survive in the long run."[56]

With the example of the Grassland Biome's modeling project before them, the modelers of the Coniferous Forest Biome began work in earnest by 1969, "determined not to produce a white elephant" like the ill-fated ELM. As with the Grassland Biome's model, the Coniferous Biome aimed to mathematically represent the ecosystem as differential equations. In other words, the model simulated the ecosystem's basic functions in terms of flows of energy and nutrients between various storage sites or compartments. The Coniferous Biome scientists aimed to execute this basic plan differently. The Grassland model, they declared, was "difficult to comprehend or modify because of the lack of any consistent notational scheme."[57] Additionally, the Coniferous Biome modelers recognized the strategy of the Grassland model caused it to quickly escalate in complexity, making the model inefficient to run and unfeasible to use.

W. Scott Overton, the "conceptual brain" of the CFB modeling and analysis group who many Biome participants considered advanced in his thinking on systems modeling, strove keep the Coniferous model simple and easily

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University of Washington, Coniferous Forest Biome Central Office, September 1974) 8.

[56] Kwa, "Modeling the grasslands" 154-155.

[57] Sollins, et al., "A Systematic Framework" 8.

accessible to modelers and biologists alike. To standardize notation, Overton developed a modeling paradigm in which a strict set of symbols assigned to specific variables allowed for easy tracking of components of the system.[58] Overton also restricted the number of variables and organizing the whole system into a series of coupled subsystems. Where the ELM model had 120 state variables and more than 1000 parameters requiring many millions of computer runs, the subsystem structure of the Coniferous Biome model allowed for greater efficiency by breaking such calculations into parts.[59] The entire ecosystem could then be modeled by linking the subsystem models of major components, such as water, carbon, and various nutrients. As the Coniferous Biome launched into its seven years of IBP-funded operations, this unique modeling strategy and the direction of Overton promised to overcome the handicaps of the earlier attempts at total system models.

W. Scott Overton

When the Coniferous Biome researchers began to sketch out the project's design and modeling strategy in 1968, Scott Overton was a relative newcomer to systems theory and modeling. Overton's interest in natural history and ecology grew from a childhood spent hunting and exploring in the forests of Virginia, a love that he enriched with independent reading of many field guides and books on

[58] Conversation with Phil Sollins, 22 November 2004. Many thanks to David McIntire for his apt description of Scott Overton in our discussion on 13 December 2004.

[59] W. Scott Overton, "The Ecosystem Modeling Approach in the Coniferous Forest Biome," *Systems Analysis and Simulation in Ecology, Vol III* (New York: Academic Press, Inc., 1975) 118-121.

taxonomy, botany, and ecology, including W.C. Allee's famous textbook, *Principles of Animal Ecology*. After serving in the Army in the Second World War, Overton pursued a formal education in these general fields at Virginia Polytechnic Institute and State University, focusing on game management.[60] Following the completion of a Master's degree in Wildlife Management at Virginia Tech in 1950, Overton began work as a biologist for the Florida Game and Freshwater Fish Commission, conducting statewide surveys and population studies on game species such as deer and quail. The heavily quantitative nature of the work sparked his interest in statistical sampling theory, methods, and modeling.[61]

Capitalizing on this interest in wildlife statistics and nine years of field experience, the thirty-three-year-old Overton opted to return to graduate school at

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[60] Conversation with Scott Overton, 05 May 2005. Overton received two degrees from Virginia Tech: a Bachelor's degree in Forestry and Conservation in 1948 with a senior year specialization in game management and a Master's degree in Wildlife Management in 1950.

[61] Conversation with Scott Overton, 05 May 2005. Overton's job at the Florida Fish and Game Commission also provided him with another valuable inspiration towards his later work in systems modeling. When visiting several University of Florida graduate students who worked in conjunction with the Fish and Game Commission, Overton made the acquaintance of a young ecology professor in an adjacent laboratory, H. T. Odum. Odum would later be recognized as the leading authority (and even considered a prophet in some circles) in systems ecology. But in 1951, when Overton met him, Odum was a new PhD from G. Evelyn Hutchinson's lab at Yale University and was just beginning his first job at Florida. Odum was also embarking on his landmark study of the Silver Springs in Florida in 1951, a study employing the trophic-dynamic concepts of Lindeman to a quantitative study of nutrients and energy through a defined ecosystem. Although Scott Overton would engage in such work in the years to come, the encounters with the young Odum provided him with an understanding of ecological theory and a friendship that would greatly stimulate his thinking.

North Carolina State University for a PhD in wildlife statistics, rather than enter the administrative ladder of the Florida Commission. Overton focused his doctoral research on sampling techniques and methods of estimation for ecological data sets, completing his PhD in Statistics and Zoology in 1964.[62] His interests in statistics and modeling grew during his short tenure as an associate professor in the Department of Biometry at Emory University in Atlanta. While at Emory, Overton's colleagues developed a compartmental model to understand the action of pharmaceuticals in the human body. The modeling strategy deeply impressed Overton and eventually resurfaced as he designed the linked subsystem structure of the CFB model.

Overton's early wildlife background was instrumental in securing a job at Oregon State University's Department of Statistics in 1965, a position subsidized through consultation work with the Oregon Department of Fish and Wildlife. Overton enjoyed the consulting work and the "legitimate projects" it provided but described himself as "interested in doing other things" by 1968. Specifically, Overton's supplementary reading led him further into quantitative systems ecology. The ongoing research of his long-time acquaintance, H.T. Odum, on analogizing ecosystems to electrical networks proved inspirational, as did the publications of the research group at Oak Ridge National Laboratory, which produced the first mathematical descriptions and models of ecosystem flow

[62] Oregon State University Archives, RG 139; subgroup 2; VI; SR 2/2/7/20 and SR 2/2/7/30, accessed 23 September 2004. Overton funded his doctoral work through concurrent work as a statistician at NC State.

processes.[63] Overton still feared the field of systems ecology, especially as expressed by Odum, seemed underappreciated among the broader ecological community despite the great leaps the field had taken in the previous decade.[64]

Overton expanded his teaching repertoire beyond his normal courses in sampling theory and methods for the Statistics Department to include a graduate seminar on systems ecology in 1966. The seminar was productive for professor and students alike, as one member of the seminar pointed Overton toward the writings of the general systems theorist, Ludwig von Bertalanffy; the next year, he discovered the work of George Klir, another purveyor of general systems theory, whose approaches were a central influence Overton's modeling strategy in the Coniferous Forest Biome.[65] Overton quit the consulting work in 1968 and sought other funding through a partial appointment in the School of Forestry at Oregon State. The timing of his new affiliation within the university gave Overton a direct link into the planning of the Coniferous Forest Biome, where he naturally assumed leadership of the modeling component of the project. Overton viewed the large integrative project aimed at modeling a complex ecosystem as a tremendous scientific opportunity to demonstrate the potential of systems thinking in ecology,

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[63] The Oak Ridge group focused on ecosystem modeling that I refer to here is comprised of Jerry Olson, Bernard C. Patten, and, for a short time between 1964 and 1966, George Van Dyne. Stanley Auerbach directed the Radiation Ecology section of the ORNL's Health Physics Division and, later the Eastern Deciduous Biome Project of the IBP at ORNL.

[64] Conversation with Scott Overton, 16 April 2005; On H.T. Odum, see Chunglin Kwa, *Mimicking Nature* (Ph.D. dissertation, University of Amsterdam, 1989) 72; also Peter J. Taylor, "Technocratic Optimism," *Jour. Hist. Bio.* Vol. 21, no. 2 (Summer 1988) 232.

[65] Conversation with Scott Overton, 05 May 2005.

and he plunged into the work with great personal commitment. The Coniferous Forest Biome, Overton believed, was the systems ecology project for which he had spent his career preparing.

Developing the CFB Modeling Strategy

The approach that Scott Overton took to the Coniferous Biome model was largely inspired by the work of George J. Klir, a Czech-born systems theorist who immigrated to the United States in 1966. Klir's method and its product particularly intrigued Overton: a general systems theory compiled from a survey of all systems theories in the literature, as opposed to an entirely unique theory like that of H.T. Odum.[66] Klir's technique resulted in a set of definitions for systems, the two most important for Overton's work being the holistic, behavior structure and the mechanistic, "Universe-Coupling" structure. Phrased another way, Overton referred to the first behavior structure as addressing "the *WHAT* questions, such as, what is the nature of the system?" The second mechanistic structure dealt with "the *HOW* questions, such as, how does the system work?"[67] These definitions formed the two dimensions of Overton's modeling strategy. First, for a model of a complex system like the Coniferous Biome, Overton deemed it important to be able to see the whole system as a single object. With the full detail of its quantitative data, however, a model of entire system would be a gigantic task and far too complex to see the connections between compartments of

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[66] Telephone conversation with Scott Overton, 01 March 2005.
[67] W. Scott Overton and Curtis White, "On Constructing Hierarchical Model in the FLEX Paradigm, Illustrated By Structural Aspects of a Hydrology Model," *Int. J. General Systems* 6 (1981) 197.

the system plainly. Thus, the mathematical functions of the model could be generalized to a set of qualitative relations. [68] If the function of daily rainfall to yearly tree rings was the object of study, for instance, a scientist could work from averaged values and construct a coarse relation of patterns of rainfall to patterns of tree rings, whereas the sheer amount of quantitative data would have obstructed a clear view of the relation. With a rough, qualitative understanding of the relation or system, the scientist could fine tune the model parameters to fit the data more precisely.[69] This strategy, which Overton called the FLEX system, could "easily accommodate structural changes with a minimum of cost and effort" by elucidating the qualitative system structure first.[70]

Overton developed the other aspect of his modeling scheme, inspired by Klir's Universe-Coupling structure, in order to describe the behaviors of subsystems and couplings of subsystems. Each of the subsystems could be viewed holistically as in the sense of the first behavioral structure. Yet, subsystems are often linked, and the behavior of one subsystem could potentially affect the behavior of others. For such systems, Overton devised a variable, dubbed the *g* or "ghost" variable, to represent an input that reacts to the system it is an input to.

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[68] W. Scott Overton, "Toward a general model structure for a forest ecosystem," in *Research on Coniferous Forest Ecosystems: First Year Progress in the Coniferous Forest Biome, US/IBP*, eds. Jerry F. Franklin, L.J. Dempster, and Richard H. Waring (Portland, Oregon: Pacific Northwest Forest and Range Experiment Station, Forest Service, U.S. Dept. of Agriculture, 1972) 38.

[69] Many thanks to Scott Overton, who provided this example of the way Klir's holistic system definition can accommodate qualitative understandings of total ecosystem in a telephone conversation on 01 March 2005.

[70] Overton and White, "Evolution" 91.

For example, in studying the physiology of a leaf, if the temperature of the air surrounding the leaf (the input to the system) increases or decreases due to the temperature of the leaf itself (a factor of the system), then the input of air temperature itself changes. By devising another model system, called REFLEX, Overton accounted for these problematic variables, yielding "the capacity of integration and control over the subsystems" along with "the potential of whole system behavior which transcends the behavior of the individual subsystems."[71]

By developing the consistent notational scheme lacking in the Grassland Biome and the unique modeling strategies using Klir's general systems theory, Overton proved himself the innovative mastermind of the theoretical and technical aspects of modeling in the CFB. Despite this, Overton never directly embraced the resource management objectives of some of his Biome colleagues, nor did he ever tout the potential of the models in practical applications outside the sphere of pure research.[72] Rather, by understanding the common misunderstandings of that potential, Overton strove to lay down a philosophy for the modeling effort that would focus on gaining theoretical understanding and empower the model to drive

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[71] Overton and White, "The FLEX Paradigm" 193; In addition to Klir, Overton's REFLEX system reflects the influence of the concept of a "holon," coined by Arthur Koestler in his book, *The Ghost In The Machine*. A holon was a system that was itself nested within a system, which is how Overton hierarchically envisioned the subsystems of the Biomes. The *g* or "ghost" variable was named thus as a nod to Koestler's book.

[72] Telephone conversation with Scott Overton, 01 March 2005; Overton felt that, in terms of goals for modeling, "direction of research is more important than management, as far as I'm concerned." To the author's knowledge, none of Overton's writings have touched the subject of the models' application to issues of forest management, with exception of some sections of the Biome proposals (whose authorship was no doubt composite and, therefore, suspect).

the research toward an improved conceptualization of forest ecosystems. Due to the fact that Overton encountered "sufficiently many opposing views among modelers and sufficiently many misconceptions among subject specialists," his articles contain many reiterations of his perspectives and philosophies.[73] The following passage is typical of such verbalizations:

> One may have several objectives in building a model. A final form of the model may be desired for prediction, experimentation, description or any of a number of other activities. A comparative treatment of several or many forms may be desired for insight into how the system works; for the advancement of the paradigm and theory of the subject science. But whatever the purpose, it must be recognized that the process of constructing a model may often be of equal or greater importance than the end result… *Models* are structured knowledge, but *modelling* allows the individual to question presently accepted structures. *Models* are excellent vehicles for hypothesis expression, but the *process* of *modelling* teaches one how to ask good questions and formulate meaningful hypotheses.
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> In a rapidly changing science, the need is not for answers, but for the ability to find answers. The need is for approaches over methods; for strategies over tactics. The need is to treat a model, not as a product, but as a stage in the study of a system from which one may learn something new or surprising about the system, or from which one can develop insight into how the system works.[74]

The emphasis on the value of the modeling process over the actual finished model, in some ways, flew in the face of the CFB's interpretation of the US/IBP objectives. In the Year Two proposal for the Biome, directly following a section

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[73] W. Scott Overton, "Toward a general model" 38; the "subject specialists" to which Overton refers are the ecologists and other scientists who headed up field research of the various subsystems.

[74] W. Scott Overton and Curtis White, "Evolution of a Hydrology Model – An Exercise in Modelling Strategy," *Int. J. General Systems* Vol 4 (1978) 89; even though this passage was written in 1976, well after Overton ceased involvement with the Biome, it echoes Overton's sentiments on the importance of process over product.

detailing the modeling approach based on Klir, the goals for the Biome's model appear in direct opposition:

> The aim of the ecosystem model is to guide the further research and to *provide answers to specific questions*. A variety of manipulations will occur because of outside events or will be proposed for experimental evaluation. The model should be useful to predict the outcomes of such manipulations.[75] (emphasis added)

The same section, entitled "Applications of the model," goes on to explicitly state the main manipulation of interest is timber harvest, and that, on the questions of clearcutting methods, management alternatives, and biological and physical consequences, "the model will be expected to provide precise answers, and in particular to point out effects and interaction that might otherwise be overlooked."

Under Overton's philosophy of modeling, building a model to provide answers for resource management problems was naïve, especially as the very nature of models negated the possibility of fulfilling such expectations. Models could be used to aid the process of decision-making on resource issues but never to supply a fully-explanatory and comprehensive representation of the forest. "Even if the finished model is the desired objective," Overton wrote, "the false leads and blind alleys, and the reasons for not using an alternate form become an important dimension of the finished form."[76] Elsewhere, Overton argued that most working models must, for the purposes of simplicity, "emphasize some elements of knowledge and neglect others," such that "in practice the models we use are

[75] Coniferous Forest Biome, Year Two Proposal, Vol. 1 (Seattle, Washington: University of Washington, Coniferous Forest Biome Central Office, May 14, 1971) 1.10.
[76] Overton and White, "Evolution" 89.

something less than 'complete.'"[77] The conflicts between the Biome administrators who primarily valued the CFB model as a product and the scientists who understood models as a process and tool for increasing the understanding of the complex forest ecosystem would ultimately cause major breaches within the research efforts.

Overton's statements on the process of modeling also conveyed his belief that modelers needed to be central ecological theorists within the overall Biome study, integrating and synthesizing the data and knowledge of the various subsystem specialists. In addition, the cycle of model development, and therefore the wheel of theory development, would be critical in directing further research within the subsystem studies.[78] As a corollary to this conception of modelers, the role of the biologists in the various subsystems also demanded a particular definition. "It is important, then," the Year Two Biome proposal stressed in May

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[77] Overton, "Toward a general model structure" 37; See also Coniferous Forest Biome Bulletin 7, pages 206-214; there was some recognition within the Biome leadership of the limits of modeling. In Bulletin 7, an extensive discussion on modeling philosophy noted that "modeling has been oversold in the past." With this in mind, models can be designed to represent a system with specific simplifications to aid computability and comprehension of a complex system, such as the collapse of a discrete time step into a continuous one or extrapolation of unknowns from other studies. However, the author of this section realized that the more of these "transformations of the real system" were incorporated into the model, the less able the model would be to handle "direct questions and give predictable answers." This discussion, while still upholding the administrative hope that models could be product and tool for addressing management problems, is reminiscent of Richard Levin's postulate, which states that manageable models attempt to balance the qualities of generality, realism, and precision, often sacrificing one quality in order to maximize the other two. (See Levins' article, "The Strategy of Model Building in Population Biology," *American Scientist, 54*, 4, 1966.)

[78] Conversation with Scott Overton, 05 May 2005.

of 1971, "that participating scientists consider the understanding of systems modeling to be a major responsibility." It continued:

> The necessary modeling cannot be done by, say, a systems engineer detached from the real system; it must be done by a biologist, perhaps in association with a systems engineer, biometrician, or biomathematician. It will not be necessary for each participant to become a full-fledged systems engineer or biomathematician (although it would be highly desirable if a few did so), but it will be essential that each participant reorient his approach and philosophy somewhat, for all should become system modelers, in one capacity or another.[79]

Modelers would be the central integrators of the Biome and biologists were to be generalists with an eye toward integration. While these roles looked good on paper, many subject specialists and administrators were reticent to adopt the plan in practice, especially if it meant models dictated the course of their research. Much of this resistance sprang from misconceptions about modeling and modelers that Overton sought to counteract: the faulty idea of the model as a product and the exclusion of modelers from the development of ecological theory.

Building the Hydrology Model

With the systems ideas of Klir fresh in his mind and the promise of a large integrated project to be guided by an ecosystem model, Overton launched into

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[79] Coniferous Forest Biome, Year 2 Proposal, Vol. 2 (Seattle, Washington: University of Washington, Coniferous Forest Biome Central Office, May 14, 1971) 8.157; again, the philosophy echoes the ideas of Klir: "A system theorist cannot master the various disciplines in which he will work sufficiently to enable him to solve all the specialized problems that may arise. But a specialist…can easily grasp the foundations of general systems theory in a relatively short time. He would then be called a *generalized specialist*." From Klir, "The Polyphonic General Systems Theory" in *Trends in General Systems Theory* (New York: John Wiley & Sons, Inc., 1972) 13.

constructing the Biome model with great zeal. Overton's enthusiasm and innovative strategies met a receptive audience in many of the other scientists who submitted proposals for research under the broader goals of the Coniferous Biome. Among these was Norm Anderson, an aquatic entomologist at Oregon State who had begun a weekly discussion group in 1967. One result of Anderson's weekly meetings was the creation of a Coniferous Biome discussion group in 1969 to generate ideas and work through the theoretical problems of conducting the large ecological study.[80] The sessions were informal gatherings that promoted intense, collective brainstorming. A group member would pose a question or problem, after which the room would remain silent for as long as ten minutes while the scientists mulled over the topic.[81] During the year before formal funding of the Coniferous Biome, this style of weekly discussions proved extremely helpful in the formulation of the overall model design and direction.

In addition, the Central Modeling group, headed by Scott Overton, initiated a series of more formal workshops in 1971 called "Round One." These sessions officially identified the subsystems of the Biome, as well as the linkages between them. A symposium on the Biome's first year of progress, held in Bellingham, Washington, on March 23-24, 1972, served as a forum to discuss and explain the

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[80] Another outgrowth of Anderson's weekly discussion groups was the "Stream Team," a group of aquatic biologists, stream ecologists, and others interested in the dynamics of the aquatic/terrestrial interface that, as discussed in Chapter 3, coalesced during the IBP years and became a crucial part of the post-IBP work in the Andrews under the LTER. The Stream Team still continues its tradition of informal Monday morning seminars every week.

[81] Conversation with Scott Overton, 16 April 2005.

internal structures of the various subsystems. By all accounts, the Bellingham conference was productive and concluded with the general sentiment that the Biome was "on the right track."[82]

In these meetings and workshops to plan the structure of the models and research, the dynamics of water appeared immediately as a critical factor to understanding the overall structure of the ecosystem. The realization that variables such as rainfall had major impacts in the transport of nutrients led to an early focus of the modeling efforts on hydrology as the key component to which the other subsystem models would be linked.[83] The objective site of the hydrology model was Watershed 10 in the H.J. Andrews Experimental Forest. At the time Overton began constructing preliminary hydrology models, however, Watershed 10 data was unavailable. Dennis Harr, an OSU forest hydrologist, conducted much of these hydrological studies on Watershed 10 after modeling work had begun. Thus, Overton's modeling group implemented the early budgets and models using the vast sets of historical data on temperature, precipitation, and streamflow compiled by the U.S. Forest Service for the nearby Watershed 2. Working with Curtis White, his graduate student and an adept programmer, Overton adjusted the hydrology model through successive versions, modifying the structure at the

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[82] Telephone conversation with Scott Overton, 01 March 2005. The proceedings from the Bellingham meeting were published as: *Research on Coniferous Forest Ecosystems: First Year Progress in the Coniferous Forest Biome, US/IBP*, Jerry F. Franklin, L. J. Dempster, and Richard Waring, eds (Portland, OR: Pacific Northwest Forest and Range Experiment Station, Forest Service, U.S. Dept. of Agriculture, 1972).
[83] Conversation with Scott Overton, 16 April 2005.

qualitative, relational level to correct for deviations from realistic behavior before introducing new variables or strict parameters on the model.[84] The data on Watershed 2 provided a basis for comparison as the data from Watershed 10 became accessible and its hydrologic behavior understood.[85]

The hydrology model had the capacity to reasonably predict hydrological behavior by 1973, but several factors eventually stalled further progress on the model. The hydrology model itself had major defects. Neither summer streamflows nor the snow/snowmelt component could be faithfully reflected in the model behavior.[86] The amount of snowmelt was extremely difficult to predict due to the complex dynamics of snowpacks, even with thorough data on temperature and precipitation. Overton's team needed better data on these complicated systems from which to build new budgets and models, leading them to conclude "that further development is academic until this part is improved."[87] After several years of intense work, the construction of the hydrology model was effectively terminated.

There were other serious stumbling blocks to model development that arose within the structure of the Biome, rather than the structure of its models.

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[84] Overton and White, "Evolution" 94-95; I thank Phil Sollins for giving me a cursory overview of the experience and specialties of the members of the modeling group in our interview of 15 September 2004. White's background was "pure programming," according to Sollins, with no experience in biological science.
[85] Overton and White, "Evolution" 98-99.
[86] Overton and White, "Evolution" 94-95.
[87] Overton and White, "Evolution" 100; Scott Overton elaborated on the problems with the snow/snowmelt portion of the model in our telephone conversation on 01 March 2005.

From the early weekly meetings of the Biome group, a marked difference in approach appeared between the scientists working toward a systems understanding of the forest and the other, more traditional scientists, including the administrators for the Oregon portion of the Coniferous Forest Biome, Jerry Franklin and Richard Waring.[88] Mainly, the gap between the systems-oriented scientists and the scientists administering the Biome manifested itself in misunderstandings about the objectives of each other's work, as well as in practical disagreements about the ultimate goals of the CFB. These dissimilarities of opinion, in turn, caused confrontation. Scott Overton's attempt to promote field research based on his systems understanding of the Biome was one such example. Sparked by the work of Bill Denison on nitrogen cycling in the old-growth Douglas fir forests, Overton began to see, through the lens of systems thinking, how central nutrient cycling and soil microbiology was to all ecosystem dynamics and biological functions. Shortly after the Bellingham meeting in March 1972, Overton approached Franklin, then deputy director of the Biome, regarding the importance of nutrients and the need to direct more resources into nutrient research. Overton remembers Franklin as acting "uninterested completely" in the suggestion.[89]

Some of the Biome's subject specialists adhered to the total modeling objectives. A commitment to integration of individual goals with the modeling

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[88] Franklin and Waring never became regular attendees of these early weekly brainstorming sessions. According to Overton (16 April 2005), when either administrator attended, the silent spells of thinking would be interrupted by Waring or Franklin: "They couldn't let questions hang… they would not let people sit and think."

[89] Telephone conversation with Scott Overton, 01 March 2005.

effort and a willingness to accept the role of modeling as central to theory development and research direction was far from universal, however. This is not surprising, given the diversity of individual research agendas that fell under the umbrella of the Coniferous Forest Biome. At the outset, the Biome administration solicited proposals from a range of individual researchers, including forestry, zoology, botany, and fish and wildlife. The final Biome proposals to the NSF represented a composite of many projects loosely associated with biological productivity in the coniferous forest, rather than a cohesive body of research driven by modeling and dedicated to developing ecosystem theory. Many of these individual investigators applied to the Biome in order to gain easier access to competitive NSF funds rather than to show support for the ideals of project.[90]

Overton believed the goals of the majority of the Biome workforce were at impasse with his systems outlook by the time he traveled to a 1973 systems ecology conference in Georgia hosted by Bernard Patten. In a paper given at the meeting, Overton listed the shortcomings of the Biome's integrative research, bemoaning the fact that "modeling has not been close to the field activity and, with

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[90] Kwa, "Modeling the Grasslands" 136-138. In his analysis of the Grassland Biome, Chunglin Kwa posited that this method of petitioning individual contributions under the aegis of a larger Biome project gave researchers the advantage of receiving NSF money as part of a block where their singular projects would not have otherwise been funded. Kwa argues this was especially true for land-grant universities (like Colorado State University, in the case of the Grassland Biome) that lacked status within the scientific community. It is not my objective to determine whether the same argument would hold for Oregon State University within the CFB, however it seems likely that some OSU researchers benefited from the prestige of their top colleagues and of the University of Washington's reputation as a "tier one" school for ecology and forestry.

few exceptions, attempts to incorporate systems perspectives and model needs into field activities has been resisted."[91] Overton further charged that Biome investigators disregarded the central modeling goals of the Biome, citing his opinion that researchers still conducted field data collection and analyses with an orientation toward "traditional questions" rather than assuming a reorientation toward systems ecology.[92] Despite the language of the national IBP effort as a project centered on the construction of a systems model, Overton claimed it had become abundantly clear that the Coniferous Biome's emphasis was not on the modeling effort, at least not in his conception of a systems analysis approach to modeling.

In addition to weathering the unresponsiveness of both administrators and field-based scientists toward modeling, Overton comprehended the active marginalization of his work and authority within the Biome. In preparing the 1973/1974 grant renewal, the Biome denied Overton's requests for support in continuing work on parameter estimation techniques and other model-specific research. In late 1972, the Biome administration made the decision to restructure the modeling efforts, breaking Overton's Central Modeling group into two separate sections, one in charge of "conceptualization and study of model behavior" and the other concerned with model assembly. Overton protested the split vehemently, arguing that the two tasks could not be achieved in isolation.[93]

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[91] Overton, "The Ecosystem Modeling Approach" 134.
[92] Ibid.
[93] Overton, "The Ecosystem Modeling Approach" 133.

Frustrated by a series of such setbacks to his vision for the Coniferous Biome model and confronted with seemingly overriding resistance of systems research objectives and misunderstanding of the potential of ecosystem modeling, Overton left the Coniferous Biome when the 1973/1974 grant ended.[94]

Overton believed that instances such as the 1972 split of his Central Modeling group was the Biome administration's attempt to make the modeling effort more relevant to its non-systems objectives, rather than letting an evolving total ecosystem model guide the research and theory development. To say that administrators did not see any value in Overton's promotion of modeling as a process of driving research and increasing basic understanding, however, is inaccurate. Likewise, Overton's sweeping fears that "sufficiently many misconceptions" about models existed among subject specialists discounted many of those scientists who truly appreciated the limitations and potential of systems modeling. Within the weekly Biome brainstorm meetings, systems modeling constituted the primary mode of operation for approaching and developing research. Although never an integral part of the weekly discussion group and a specialist in tree physiology, Waring made a point to attend Overton's lectures on Klir's systems theory.[95]

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[94] Telephone conversation with Scott Overton, 01 March and 05 May 2005.

[95] According to Overton, however, the misunderstandings of Biome participants regarding models were significant at times. In our conversation of 05 May 2005, Overton related an incident when Jerry Franklin was urging the modeling group to "build the best model they could" of the coniferous forest. For Overton, the Biome model was a dynamic stage of theory development. Overton felt that Franklin's desire for a concrete, finalized product, or even a completely faithful

Overton's ideas certainly met with fundamental misunderstandings, but the problems encountered by his modeling work were more practical than conceptual, especially from the standpoint of the Biome's administrators. Waring viewed Overton's philosophical approach to the modeling scheme as highly beneficial in the initial stages of planning. Even so, Waring believed the model never guided Biome research because the cycle of field study, modeling, and validation simply did not roll forward fast enough under Overton's watch. As the elaborate box-and-arrow diagrams and qualitative mapping of possible ecological interactions began to improve, the models required more empirical, quantitative data from the fieldwork on Watershed 10. Waring recalled that neither Overton nor White, his programmer, took the initiative in demanding data from the Andrews field researchers. To complicate matters, according to Waring, White was hesitant to proceed on projects without constantly double-checking with Overton.

Waring and Franklin had obligations to report progress annually to the NSF, in order to secure continued funding for the Coniferous Biome. A program with the finite lifespan of the CFB required its administrators to eliminate those people and projects that were not producing results efficiently. Waring and Franklin feared that Overton's unyielding commitment to his modeling ideals and slow, deliberate research style would not enable a system model to be calibrated in

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depiction of the system, was missing the point. Overton recollected walking to the chalkboard and writing the word "MODEL" in big, bold letters, implying Franklin's ideal of a static, finished Biome model would be as helpful in understanding of a complex system as a word in chalk, if not an outright impossibility.

five years.[96] Regardless of how good his theories and conceptual, "black box" models were, Waring said, "there was nothing coming out of the box to show the NSF!"[97] With two years left in the US/IBP, Franklin and Waring allocated fewer resources toward Overton's research in the 1973/1974 grant renewal and divided the Central Modeling group, both attempts to take some of the authority out of Overton's hands and accelerate the Biome's modeling effort. These actions, along with many abrasive interactions between the modeler and the administrators, increasingly marginalized Overton until he resolved to leave the Biome entirely.

Despite Overton's convictions that his colleagues underappreciated the significance of his work and the potential of systems analysis in ecology, there were many reasons behind the stagnation of the systems modeling work besides simple conceptual misunderstanding of models. The clash of strong personalities aggravated the relationship of the modeler and the administrators, a situation already made volatile by fundamentally different philosophies of modeling and divergent approaches to the Biome objectives. The administrators liked the grand idea of a Biome model as a tool for theory development and cohesion of diverse strains of forest science, but their role in overseeing the Biome's function necessitated a more pragmatic, financially-minded vantage. Within the short-term framework of a five-year Biome program, the modeling efforts need to serve two primary roles. The modeling effort should produce a workable model. Whether viewed as a process or a product, the administrators of the Biome needed the

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[96] Conversation with Richard Waring, 18 May 2005.
[97] Conversation with Richard Waring, 02 December 2005.

outcome of the modeling effort to be functional evidence that the NSF's money was well-spent on those objectives. Ideally, the model would also efficiently integrate the disparate areas of research that were thrown into cooperation for the duration of the Biome, acting simultaneously to increase communication between different disciplines and field sites. Biome-wide modeling workshops initially spurred conversation, but modeling under Overton was ultimately not the desired integrator of ideas, research, people, and institutions that it had been envisioned. The problem may have been Overton's own style of science or his colleagues' lack of receptivity. Regardless, as Overton became more marginalized in funding and participation, the administrators sought to add another modeler to the project who could salvage the Biome's modeling effort as the program headed into its final two years of official operation.

Phil Sollins and the Carbon-Hydrology Model

An advertisement in the "Personnel Placement" section in *Science* on January 5, 1973, read: "Ecosystem Modeler needed immediately for Coniferous Biome of International Biological Program through 1974."[98] Jerry Olson, the path-breaking systems ecologist at Oak Ridge National Laboratory (ORNL), suggested that his recently-graduated Ph.D. student, Phil Sollins, should submit an application. Sollins was a strong candidate for the CFB ecosystem modeler position, and the Biome ultimately offered him the job. Between 1973 and 1976, Sollins experienced many of the same tensions between the goals and philosophies

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[98] *Science*, Vol 179, No. 4068 (Jan. 5, 1973) 99.

of the modelers and the primary investigators that surfaced during Overton's tenure. In hiring Sollins, the Biome management hoped to find a modeler who would efficiently produce a model to show the NSF and achieve the goals of truly integrating the whole program. Whereas Overton's conflict with his superiors concentrated on conceptual misunderstandings of models as scientific tools, Sollins' problems centered on the unrealistic responsibility of maintaining a cohesive, institutional function that Biome administrators placed on the models and the modeling personnel. An account of Sollins' work and his struggles with the inherited burden of synthesis demonstrates how the disagreements over the CFB model did as much to destroy integration and cooperation within the Biome as it did to instill these attributes in the program.

At the time Sollins responded to the CFB job opening, the twenty-eight-year-old scientist already had impressive experience and many connections within the small community of systems ecologists. As an undergraduate at Swarthmore College, Sollins had the opportunity to work for two summers at El Verde, H.T. Odum's tropical rainforest ecosystem study in Puerto Rico. At El Verde, Sollins met Jerry Olson, his future advisor at ORNL, who was developing biomass equations for the tropical forest. "They cut down trees and weighed them," Sollins recalled, "and I did everything else," referring to his early involvement with computer applications in ecology as part of the El Verde project. The experience furnished Sollins with his first publication and the admiration of Odum, who offered him a graduate scholarship in the Zoology Department at the University of

North Carolina in 1966 after the Swarthmore biology major completed his degree.[99] Sollins felt he never fit in with the zoology program at UNC and, in 1968, readily accepted a doctoral fellowship at the University of Tennessee and the associated ecology program at ORNL. As the center of systems ecology research, the atmosphere at Oak Ridge was exciting for Sollins, in terms of the intellectual caliber of its personnel, the enormous financial and technological resources at its disposal, and its setting as a crossroads for top ecologists from around the country. In addition, Oak Ridge had also been chosen as the site for the Eastern Deciduous Forest Biome by 1968, affording Sollins the opportunity to engage in cross-Biome workshops and meet some of his future colleagues in the CFB, including Dale Cole of the University of Washington and Jerry Franklin of the Forest Service Research Station in Corvallis.[100]

Sollins' connections in the Pacific Northwest served him well. After Mike Newton, a forest scientist at Oregon State University, returned from a sabbatical at ORNL with glowing remarks about the young ecologist, Dick Waring invited Sollins to Corvallis for an interview in 1972. Waring offered him a post-doctoral fellowship to engage in modeling work with the CFB, but Sollins declined. When he responded to Douglas Chapman of the University of Washington regarding the

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[99] Sollins' first article appeared in "the big 'Tropical Rainforest' book" as: P. Sollins and G. Drewry, "Conductivity and Flow Rate of Water through the Forest Canopy," in *Tropical rain-forest*, USAEC Report No. TID-24270 (Springfield, VA: Nat. Tech. Inf. Serv., 1970).

[100] Conversation with Phil Sollins, 02 September 2004. Supplementary information, such as exact dates of graduation, was taken from Sollin's curriculum vitae, graciously provided by Sollins.

January 1973 advertisement in *Science* and flew to Seattle for an interview in February, he had no idea that the position was the same one the Corvallis scientists had extended to him.[101] Evidently, Dale Cole and Stanley Gessel, the main CFB administrators at UW, had failed to comprehend the active recruitment of Sollins by Franklin and Waring, either. As a result of the incomplete disclosure of information and the patent mistrust between the Seattle and Corvallis cohorts of the CFB, Sollins remembered his Seattle interview as "moderately unpleasant." Whether due to resentment over the embarrassing situation or Sollins' junior status, Gessel offered him the job as a "Research Assistant Professor" at the University of Washington's College of Forest Resources, a created position that denied Sollins a proper professorship but placed him on a higher tier than a post-doctoral fellow. Sollins accepted the opportunity, which was the first paid, full-time appointment of his career and promised to be well-funded and interesting work in terrestrial ecosystem modeling.[102]

After Sollins moved to Seattle in the spring of 1973, the tensions between his new home institution and his early contacts in Corvallis became even more apparent. Sollins found his work and obligations tenuously stretched between Stanley Gessel and Dale Cole at the University of Washington and the Corvallis

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[101] Conversation with Phil Sollins, 22 September 2004. Sollins decided to stay on at ORNL half-time while waiting for a pending post-doctoral position in Great Britain. Late in 1972, Jerry Franklin also attempted to recruit Sollins to Corvallis. The position in England had failed to materialize, and Sollins began to correspond with Franklin by January 1973, when he simultaneously pursued the advertised Biome position.

[102] Conversation with Phil Sollins, 22 September 2004.

administrators, Jerry Franklin of the Forest Service and Dick Waring at Oregon State University. As Sollins recollected the situation, he "was dragged through the offices" of the four administrators, who collectively gave him two main tasks. First, the administration expected the new modeler of the Biome to ensure that the modeling process integrated the divergent groups of the Biome and promoted cooperation between the institutions and the various areas of research.[103] By bringing a fresh face to the modeling effort, the Biome leaders also hoped Sollins would succeed in making the ecosystem model a vehicle of synthetic research where Overton had failed to do so. On the side of the Corvallis scientists, at least, there were additional hopes that their familiarity with Sollins would promote better relations with UW, another role that Overton neglected to fulfill.[104] Sollins felt he was expected to act more as a courier or double-agent, abetting the inter-institutional competition, rather than a mediator aiding Biome-wide cooperation. Regardless of the administrators' intentions in charging their new modeler with the task of making the Biome personnel work together, Sollins soon realized the futile nature of such a commission. The strategy of pinning the responsibility for synthetic research on a computer model was, Sollins believed, equally impracticable. In retrospect, Sollins mused that the Biome's goals of integration and synthesis were "like New Year's resolutions: everybody agrees they should be done, but they never happen."[105]

[103] Conversation with Phil Sollins, 02 September 2004.
[104] Conversation with Dick Waring, 18 May 2005.
[105] Conversation with Phil Sollins, 02 September 2004.

The Biome administrators' second request seemed less political and more feasible, given Sollins' expertise: "to build a model of something." In early 1973, with Scott Overton edging out of Biome participation and the systems modeling portion of the project stymied, the administrators were eager to produce a functional ecosystem model that would build upon Overton's foundational concepts and deliver on the promises of the NSF proposals. During his time at ORNL, Sollins grew interested in understanding the dynamics of nutrients and organic carbon in forest ecosystems and, for his PhD thesis, developed one of the first budgets and models for forest organic matter. Thus, as a starting point in his modeling for the CFB, Sollins set out to link this carbon model to Overton's hydrology model.[106] The executive council of the CFB officially assigned Sollins as chairman of the Biome's modeling committee in August of 1973, formalizing Sollins' role as the new director of the Biome's modeling functions.[107]

Sollins built the carbon-hydrology model according to Overton's protocols for structure and computer code, a modeling strategy that the younger scientist considered valuable and "ahead of its time." By the time Sollins arrived, the sampling sites on Watershed 10 in the H.J. Andrews Forest, and the hydrologic processes were well-characterized. As a result, the Oregon group at the Andrews provided a good set of water data from which to work. More importantly, however, the intellectual atmosphere at the Andrews proved conducive to

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[106] Conversation with Phil Sollins, 02 September 2004; Also drawing upon Sollins' stated research foci from his curriculum vitae, provided by the interviewee.
[107] Conversation with Phil Sollins, 22 September 2004.

accessing the appropriate data from the field researchers for the model. Early in the IBP era, the Oregon group decided against involving many graduate students in Biome research at the Andrews, opting instead to hire post-docs whose time, attention, and obligations were not divided between coursework, degree requirements, and the demands of professors. At UW and the Cedar River site, the workforce was largely composed of graduate students, an institutional approach that promoted a closed, proprietary attitude in its researchers and stifled the possibility for collaboration. Given this environment at UW, Sollins found it difficult to accumulate the data he needed for his modeling work. The process of "putting the pieces together" was much easier within the OSU research group on the Andrews' Watershed 10, where individual post-docs did not have degrees dependent on their research and the colleagues they shared it with. Consequently, the Oregon portion of the Biome was more collaborative and forthcoming with data for the modeling effort.[108] The Oregon post-docs also constituted a more coherent and congenial group of peers for Sollins, which, combined with the modeler's own willingness to go out into the field with them, led Sollins to make close friends among the Corvallis Biome participants. Sollins began to spend increasing amounts of time in Corvallis and at the Andrews by 1974. Although he

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[108] Conversations with Phil Sollins, 02 September 2004 and 15 September 2004; Much of the information regarding the CFB's decision to hire post-docs was confirmed and supplemented by my conversations with Dick Waring, 02 December 2004 and 18 May 2005.

still worked for the UW side of the CFB, Sollins largely based his carbon-hydrology model of the coniferous forest on the work at the Andrews.[109]

The linked carbon-hydrology modeling project became functional by the time IBP funding ended in 1976 in the form of a model called "CONIFER." Sollins also began to compile a budget for the forest's nutrient flux in 1975. Like Overton before him, Sollins recognized the centrality of nutrients to the ecosystem's function, and from the planning stages of the CFB model, its designers envisioned a nutrient cycle forming a third leg of the total ecosystem model, alongside the hydrology and carbon components. While in Seattle, Sollins became good friends with Dale Cole, the UW Biome administrator and a specialist in elemental cycling. Cole significantly influenced Sollins' thinking about the nutrient budget for the Andrews, and discussion between the two scientists sparked many fruitful branches of the nutrient research. Cole's input aided Sollins' conceptualization of an H^+ budget to better understand the dynamics of acidity, anions, and cations in forest ecosystems. Sollins also built on the nutrient budget work to explore the then poorly-understood movements and implications of dissolved organic carbon and dissolved organic nitrogen, course woody debris, and soil organic matter within the forest.[110]

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[109] Conversation with Phil Sollins, 15 September 2004.

[110] I draw largely on the extensive discussion of research interests in Phil Sollins' CV for this information. It should be noted that Sollins' work regarding dissolved organic nitrogen seems to have been pertinent to the contemporary discussions over clearcutting and other management techniques. In the 1960s, Likens and Bormann's work at the Hubbard Brook forest uncovered the phenomena of massive and detrimental losses of nitrogen as inorganic and highly soluble nitrate

As Sollins began to spend more of his time with the Oregon group and the Andrews data, his friendship with Cole grew strained. In choosing to build the Biome model that focused primarily on non-UW-based research, Sollins guessed he "was not following orders" in Cole's estimation, and Cole began to withdraw his assistance from the modeler and his evolving nutrient budget. Eventually, in order avoid further tension and expedite Sollins' work, Cole decided to permanently relocate Sollins to Corvallis in 1975.[111] Through this transition, Sollins' nutrient budget suffered from a lack of Cole's mentorship, only to be turned down for publication in *Ecological Monographs* in 1978. Between 1974 and 1976, Sollins also struggled to develop his H^+ model single-handedly and write a manuscript for the carbon-hydrology model, projects hindered by a disheartening lack of support from any of his superiors within the Biome during its last years of operation.

Sollins eventually published the nutrient budget in a frequently-cited 1980 paper, entitled *The Internal Element Cycles of an Old-Growth Douglas-Fir Ecosystem in Western Oregon*. The nutrient budget, however, never became a model, much less a component in a total systems model.[112] When a proposal for

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from a clear-cut forest system. His work at the Andrews showed much greater rates of nitrogen loss through dissolved organic nitrogen, thus disproving the universality of the Hubbard Brook results and reopening the questions of nitrogen dynamics and bio-availability after deforestation, as well as the debate over the pros and cons of clearcutting.

[111] Conversation with Phil Sollins, 15 September 2004.

[112] See Phil Sollins, C.C. Grier, F.M. McCorison, Kermit Cromack, Jr., R. Fogel, and R.L. Fredriksen, "The internal element cycle of an old-growth Douglas-fir

post-IBP funds was submitted to the NSF in 1976, neither Sollins' modeling work nor total ecosystem modeling projects of the kind Overton imagined were part of the research plan. Sollins' exclusion from the 1976 "Watershed" grant forced him to seek other funding and areas of research. Sollins obtained several small grants that yielded successful publications on tree growth, tree mortality, and wood decomposition. Ultimately, a large grant to develop his growing interest in soil organic matter led Sollins to change the direction of his research away from ecosystem modeling, a field that he never returned to in a serious manner.[113]

Sollins' term as the director of the CFB modeling operation shows most pointedly the differences in goals and philosophies between the modelers and the administrating scientists. Those scientists who were sympathetic to the modeling effort tended to think about the model as a way of establishing a solid cycle of research, cooperation, and communication in the long run. The administrators focused on a more immediate and practically-motivated set of goal that would ensure the model's survival through the years in the Biome, choosing to promote the elegant but implausible ideal of a large, integrative model. The Biome was in desperate need of cohesion due to the uncomfortable partnership between UW and OSU; the idea of a total system model for the whole project promised to fill that need. Many smaller, successful process models emerged from the Biome years. Still, some discernible progress on the large ecosystem model was necessary to

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ecosystem in western Oregon," *Ecological Monographs*, vol. 50, no. 3 (September 1980): 261-285.

[113] Conversation with Phil Sollins, 15 September 2004.

keep the NSF dollars flowing into the research effort. It is interesting to note that, in striving to stay true to their initial goals for an integrative model, the administrators actually helped to reshape the Biome's objectives by funneling money to scientists who were most efficient and productive. Due to the fact that the total system model would take too long to become a functional part of the research cycle, the funding and objectives shifted away from modeling and toward areas of new field research that had proved themselves exciting and worthy of continued study. The field components of the Biome-era research, particularly at the Andrews, eventually offered a more prolific legacy of scientific questions, experimental sites, and intellectual collaboration.[114]

Initial Critiques of the Biome Models

Given the manner in which different research avenues of the CFB eventually overshadowed the original modeling objectives of the US/IBP Biome programs, the tendency to view the modeling effort as a failure is natural. In the years following the official dissolution of the US/IBP in June of 1974, many in the

[114] Sollins' experience within the CFB points to a number of other valuable conclusions worth further historical investigation. First, due to his dual allegiances to OSU and UW, Sollins' modeling work was caught in the fray of a long-standing institutional rivalry between two forestry schools with differences in research style. As a result, the modeling effort suffered, and old animosities between the two schools of forestry within the compass of the Biome frustrated the integration of research. Also, in comparison to Overton, the productivity of Sollins' work in building the linked carbon-hydrology model seems to have been rooted in issues of personality, research style, and age. Furthermore, the distribution of research tasks among independent post-docs with the Oregon group aided Sollins' output substantially, a fact that necessitates a more extensive treatment of the relationship of the organizational structure of large research programs and the quality and circulation of the resulting findings.

scientific community condemned the Biome's modeling groups for falling embarrassingly short of the anticipated total ecosystem models. In 1975, the Committee to Evaluate the IBP, a cross-disciplinary panel of scientists and engineers commissioned by the National Academy of Science, produced a review of the US/IBP's ten year lifespan.[115] The committee concluded:

> Although the U.S. Program failed to realize certain objectives such as the production of workable, large-scale models for entire ecosystems and the establishment of readily accessible data banks, it convincingly demonstrated the effectiveness of the multidisciplinary approach to research on complex problems. We conclude that, overall, the U.S. performance was creditable and that substantial scientific contributions were made.[116]

Later evaluations of the Biomes and their models were not so generous. Philip Boffey's 1976 article in *Science*, candidly titled "International Biological Program: Was It Worth the Cost and Effort?" expressed shock at the Committee to Evaluate the IBP's optimistic assertion, given its own admission of numerous problems in IBP organization and the almost complete failure to achieve its major objectives. Boffey examined the major "grandiose" goals that the proponents of the US/IBP argued for in the 1967 Congressional Hearings – the development of systems analysis models, the increase of ecological knowledge for management

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[115] National Research Council, Committee to Evaluate the IBP, *An evaluation of the International Biological Program* (Washington, D.C.: National Academy of Sciences, December 1975) 1-2.

[116] Committee to Evaluate the IBP 62-63; For the NAS committee, the dearth of adequate enthusiasm on the part of many participants, foresight planning of funding, and coordination of research were greater problems than the failure to meet specified research goals. "The coordination of research projects left something to be desired," the committee decided, "but we doubt that under the prevailing circumstances a substantially more cohesive program could have been developed."

purposes, the improvement of international cooperation, the training of scientists – discovering only the last of the objectives fully met. Evaluation of the IBP, therefore, showed that "the American IBP effort, while making 'major contributions,'… failed to live up to its own rhetoric."[117] Another 1976 report for the NSF by the Battelle Laboratories in Columbus, Ohio, compared the findings and efficiency of the Grassland, Tundra, and Desert Biomes, similarly concluding that many goals were only partially met. The Battelle report stated the Biomes' models demonstrated "the difficulties of handling detailed ecosystem functions at the present state of the art of describing nature," an experience that "taught a painful and expensive lesson" about both the contributions of models and the organization of large research programs.[118] The funding of the Biomes' modeling objectives yielded a "spotty record," in Boffey's analysis, and, at best, a wide range of results. As examples of a larger body of criticism, these articles also show the skeptical and ambivalent scientific environment in which the Biome projects took place.

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[117] Philip M. Boffey, "International Biological Program: Was It Worth the Cost and Effort?" *Science*, Vol. 193, No. 4256 (September 3, 1976): 867.

[118] Rodger Mitchell, Ramona A. Meyer, and Jerry Downhower, "An Evaluation of Three Biome Programs," *Science*, Vol. 192, No. 4242 (May 28, 1976) 865; This article is a synopsis of the much longer Battelle Report to the NSF, officially titled "Evaluation of three of the biome studies programs funded under the foundation's International Biological Program (IBP)" and available through the National Technical Information Service, Springfield, Virginia. The Battelle report also contrasted the large Biome projects to the smaller Hubbard Brook ecosystem study, concluding that complex, expensive studies such as the Biomes were significantly less effective compared to smaller-scale projects.

In a 1972 *Science* article, Allen Hammond analyzed the various Biomes' modeling philosophies and strategies, addressing the debate over the efficacy of the "big biology" approach needed to sustain a large modeling effort. Although the Biomes shared a central emphasis on ecosystem modeling, Hammond drew attention to the diversity of modeling approaches taken by the different Biomes. He observed that, "because ecological research of this type is still relatively new, there is some disagreement as to what approach will produce the most realistic models." While the Tundra and Grassland project chose to forge ahead with total system models, Hammond explained, the efforts at the Eastern Deciduous and Western Coniferous Biomes shifted their focus toward basic process models. For its part, the Desert Biome surrendered to the difficulties of ecosystem models, attempting instead to understand the system's complexity through fine-resolution species models.[119] Despite the models' shortcomings, Hammond remained optimistic. He thought that the variety of modeling strategies might actually serve "as a fortunate development that will increase the chances of eventual success" though "several more years of research at the least, will be needed before the models will be sufficiently developed."[120]

That the Coniferous Biome fell short of its grand goals of producing ecosystem models to increase understanding of ecological complexity and to guide

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[119] In regard to Hammond's view of the CFB, it was an accurate observation that the CFB was "emphasizing process models," in so far as the modelers expended most of their energies on subsystem models. Hammond neglects to acknowledge the intention of eventual linkage of those process models.
[120] Allen L. Hammond, "Ecosystem Analysis: Biome Approach to Environmental Research," *Science*, Vol. 175, No. 4017 (January 7, 1972): 47-48

resource management remains unquestionable. To fairly evaluate the contributions of the Biome modeling work and the CFB in total, however, we must look beyond the narrowly-defined goals of the proposals. With the advantage of hindsight, the highly productive field research that originated during the IBP era at the H.J. Andrews Forest is evidence that the time and energy expended on the Biome studies was not a complete loss.

To be fair, most of the critics writing in the mid-1970s thought their evaluations of the Biomes were "clearly premature," recognizing that "no final judgment of the program will be possible for many years."[121] Hammond's 1972 observations of the Biomes while the US/IBP was still ongoing were the most prescient in this regard. While recognizing the program's deficiencies, Hammond concluded that "even if (the Biome program) does not achieve (its) goals, the training of a new type of ecologist seems certain to advance the attempt to understand ecological processes." [122] Also, Hammond noted that the importance of the individual findings from the Biomes was pale in comparison to the significance of "the changing type of observations and the way in which they are reported." He remarked:

> Increasingly, observational studies are focusing on the flows of both material and energy within an ecosystem and on the basic processes which control those flows, rather than just on the components of the ecosystem themselves.[123]

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[121] See Blair's *Big Biology*, page164; Boffey 867; The "IBP Synthesis" volumes had not yet been produced by 1976, nor had Phil Sollins yet completed his linked carbon-hydrology model, a functional, integrative systems model.
[122] Hammond 48.
[123] Hammond 47.

The modeling framework, which initially forced this focus on ecological processes to the forefront, declined, and the true products of the Coniferous Forest Biome's modeling effort appeared neither in a finalized ecosystem model nor in a growth of total ecosystem modeling. Rather, the concentration on process and system studies in observational and field research characterized the post-IBP research at the Andrews forest and profoundly affected a new generation of ecologists whose research aimed at understanding the structures and functions of ecosystems.

CHAPTER 3: The Field Studies of the Coniferous Forest Biome

Given the number of field-based studies that were ongoing in the H.J. Andrews Experimental Forest during the years of the IBP, a comprehensive survey of the research, its goals and outcomes, and the personnel involved would be a project beyond the scope of this study. This chapter focuses on a few case studies to illuminate the type of exciting research and unexpected findings associated with the CFB work at the Andrews. This section will also illustrate the connection of this work to the Biome's motivating questions regarding ecosystem structure and function. As in the rest of the Biome, field studies in the Andrews Forest intersected in widely varying degrees with the Biome's modeling effort. Nevertheless, the attempt to build a total system model promoted a focus on process and function that stimulated the field researchers to look at the forest's species, structure, and ecology from a different vantage point. Although the CFB total ecosystem model never approached complete integration, the requisite functional questions of modeling effort led to a richer understanding of undisturbed old-growth forests, encouraged multidisciplinary interaction and collaboration, and sparked a long-term and fruitful research program in forest ecology.

A Synthetic Appraisal: *Ecological Characteristics of Old-Growth Douglas-fir Forests*

When IBP funding ceased to flow into the Coniferous Forest Biome in 1976, a group of scientists from Oregon State University and the Forest Service

Research Station in Corvallis managed to obtain a successor grant from the National Science Foundation. By comparison with the IBP budget, the "Watershed" grant, as it was dubbed by the scientists, was meager. Nevertheless, the successor grant enabled continued investigations of old-growth Douglas-fir forests at the H. J. Andrews Experimental Forest. In keeping with the spirit of the Biome objectives, the field work at the Andrews after the IBP brought together a number of disparate specialties and scales of research, from geomorphology studies of Watershed 10 to work on the ecological roles of forest fungi.

The experience of spending summer field seasons working together on the same watershed research sites fostered a great deal of camaraderie and collaboration among the Andrews research teams. It also provided the opportunity to formally synthesize the knowledge accumulated during the CFB years. A work session in February of 1977, sponsored by the U.S. Forest Service and led by Jerry Franklin, convened at the Wind River Experimental Forest in southern Washington. In the face of rising concerns about harvesting old-growth trees, the Forest Service wished to mandate some protective measures for the ancient stands. The agency, however, lacked an adequate, workable definition of "old-growth." Therefore, the scientists at the 1977 Wind River meeting sought to distinguish the attributes of an old-growth forest from those of younger stands and to make management recommendations.[124] Over the next few months, Franklin compiled the input and writing of the group's various members: soils ecologist Kermit

[124] Luoma 140-141.

Cromack, botanists Bill Denison and Art McKee, mammologist Chris Maser, aquatic biologist Jim Sedell, geomorphologist Fred Swanson, and Glen Juday, then a doctoral candidate in forest ecology. The resulting report outlined the unique and intricate structural, functional, and compositional attributes of old-growth stands, as well as potential forest management strategies. Franklin's summary conveyed the first scientifically-grounded articulation of the ecological complexity and significance of ancient coniferous forests.[125] Emerging into a field that had long valued efficient cultivation of successive crops on forestlands rather than preservation of undisturbed stands, the conclusions of the Wind River meeting delivered a daring challenge to the accepted customs of forestry. The Forest Service eventually published the highly influential paper in 1981, under the title *Ecological Characteristics of Old-Growth Douglas-Fir Forests*.[126]

Most of the historical analyses of the *Ecological Characteristics* monograph focused on its implications in the heated dispute over the management of old-growth forests that occurred in the late 1980s and early 1990s. Despite these future repercussions of the seminal report, *Ecological Characteristics* also served as an elegant synthesis and retrospective of many important findings from the Coniferous Forest Biome's field investigations at the Andrews during the 1970s. The monograph's discussion of the science reflected the actual division of labor within the field research, exhibiting three separate but frequently

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[125] Jerry F. Franklin, et al., *Ecological Characteristics of Old-Growth Douglas-Fir Forests* (Portland, OR: U.S. Dept. of Agriculture, Forest Service, Pacific Northwest Forest and Range Experiment Station, 1981) 1-2.
[126] Luoma 141-144.

interconnecting approaches to characterizing and understanding the forest. First, Andrews scientists wanted to elucidate the forest's composition: what species were present and how abundant were they? Secondly, researchers sought to decipher the function of the forest's ecological processes, such as the pathways and flow rates of nutrient, energy, and water cycling. Finally, the interdisciplinary team at the Andrews considered their findings on the structure of the forest, meaning how trees and logs were spatially arranged in the forest.

The central conclusion of the *Ecological Characteristics* paper argued four structural components – "(1) the individual, live, old-growth trees; (2) the large standing trees or snags; the large, dead down trunks or logs," both (3) on land and (4) in streams – were both unique to and "of overwhelming importance in an old-growth forest." Further, the authors of the 1981 paper concluded:

> most of the unique, or at least distinctive, compositional and functional features of old-growth forests can be related to these structural features; that is, these structural components make possible much of the uniqueness of the old-growth forest in terms of flora and fauna (composition) and the way in which energy and nutrients are cycled (function).[127]

In recounting their new, integrated understanding of the forest's ecological interrelationships, the Andrews scientists easily drew the connection from the presence of large live trees, dead logs, and standing snags to the roles that these features fulfilled as habitat for numerous species and as vehicles for the circulation of energy and nutrients in the system. These hallmark features of lush Western coniferous forests that the scientists showed to be so crucial ecologically were the

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[127] Jerry F. Franklin, et al., *Ecological Characteristics* 20.

very components of woodlands that traditional foresters viewed as decadent, over-mature, and unproductive, if not utterly wasteful.

Historically, the scientific process by which the four crucial structural components became clear seems to have occurred in the opposite direction. An examination of some of the breakthroughs in the conceptualization of the forest reveals that such findings were primarily field-based studies motivated by questions about the forest's function, not its structure. Rather than querying the role that decaying logs might play in preservation of an appropriate microclimate for an insect species or in storage of nitrogen and other nutrients, scientists in the Andrews were curious about the sources and sinks of nutrients such as nitrogen and organic matter. In large part, the modeling effort of the Biome necessitated this new style of observation. The construction of conceptual models and budgets required qualitative information and quantitative data about the components of the ecosystem. This data included information on the storage and transformation of the energy, water, carbon, nutrients, or ions being modeled, as well as the processes connecting the components and the rates of flux between them. Heading into the forest with this orientation toward ecosystem processes and functional interconnections, scientists were able to comprehend aspects of the forest's composition and structure that investigations of isolated components of the system had not elucidated. A few cases studies of functionally-driven observational research in the IBP era will illustrate the motivations and goals of the field researchers and tease apart the relationship of field work and modeling in the Coniferous Forest Biome.

A Case Study: The *Lobaria* Breakthrough

The classic example of the Biome's unexpected discoveries in studying ecosystem function came early in the research effort. Even before NSF funding of the CFB in September of 1970, mycologist Bill Denison was scouring the forest floor in the H. J. Andrews. A recent addition to Oregon State's Department of Botany and Plant Pathology in 1966, Denison soon volunteered for the Biome's preliminary work on the cycling of nitrogen in the old-growth system. Specifically, Denison sought to find the forest's source of biologically-accessible nitrogen, a form of the nutrient essential to protein formation and plant growth. In order for the critical element to become available to plants, certain organisms convert or "fix" atmospheric nitrogen, N_2, as ammonia, which is then incorporated into the amine groups of amino acids or oxidized to other biologically-accessible forms, such as ammonium and nitrate. Such nitrogen-fixing organisms often live symbiotically with specific plants, like the *Rhizobium* bacteria residing in the root nodules of legumes. Despite the abundance of nitrogen in the soils of the lush Andrews forest, the apparent absence of biological nitrogen fixers and their associated plant species mystified Denison as he hunted for an organism capable of supplying the forest's enormous biomass with the crucial element.[128]

[128] Luoma 50-53; The stories of the *Lobaria* breakthrough are as related to the journalist Jon Luoma by Denison in the mid-1990s. There are some inaccuracies to Luoma's historical details and scientific explanations. I relied on his narrative

During his careful surveys of the forest floor in 1969, Denison noticed pieces of a prevalent species of pale green lichen, later identified as *Lobaria oregana*, which fell to the ground from the branches where it grew high in the canopy. Frustrated in his attempt to find soil-dwelling nitrogen fixers, the mycologist wondered if the mutualistic alga-fungi might be conducting the forest's nitrogen fixation far above the ground rather than beneath it. Using the acetylene reduction technique, Denison tested the lichen for nitrogenase activity, the enzymatic pathway responsible for the reduction of atmospheric nitrogen to ammonia, as well as the conversion of acetylene to ethylene. Still in the lean days before Biome funding, Denison improvised an acetylene chamber by placing a piece of *Lobaria* in a plastic bag, sealed except for a plastic tube with a rubber septum. Through this septum, Denison removed the air in the bag with a syringe, replacing it with a quantity of acetylene gas obtained from an old miner's lamp. After exposing the lichen to acetylene for an hour, Denison harvested the resulting gas by syringe.[129] Chemical analysis showed the gas had indeed been converted to ethylene, proving *Lobaria* to be the abundant, nitrogen-fixing organism Denison had been seeking in the Andrews forest.

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of Denison's activities in the Andrews, however, as I was unable to secure an interview due to Dr. Denison's poor health. He passed away on April 8, 2005 after a long illness.

[129] William C. Denison, "*Lobaria oregana*, a nitrogen-fixing lichen in old-growth Douglas fir forests," in *Symbiotic nitrogen fixation in the management of temperate forests: Proceedings of a workshop held April 2-5, 1979*, eds. J.C. Gordon, C.T. Wheeler, and D.A. Perry (Corvallis, OR: OSU Forest Research Laboratory, 1979) 268.

The discovery of *Lobaria oregana* opened several new vantages on the large, living, old-growth trees. The work of Denison in 1969 and 1970 revealed the canopy of the ancient forests as an unexplored niche in the forest and an uncharted area in forest science. The findings on *Lobaria* invited an extensive survey for other distinct epiphytic plant and animal communities that used the canopy as habitat. During the course of the 1970s, Andrews scientists found more than 1,500 species of invertebrates in a single stand of old-growth Douglas-fir, many arrayed in specific regions horizontally and vertically on a tree. The canopy also supported the exclusive habitat of several vertebrate species, famously including the Northern spotted owl.[130] The researchers in the Andrews identified more than one hundred species of mosses and lichens on the trunks and limbs of mature Douglas-fir trees, finding single trees on which the total dry weight of epiphytic species reached well over 50 pounds.[131] In most cases, at least half of total

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[130] As an OSU undergraduate in wildlife biology, Eric Forsman first identified the Northern spotted owl while working as a fire lookout in the Willamette National Forest in 1968. His interest in the rare owls carried through his Master's and Ph.D. degrees in wildlife biology at OSU, during which he tracked eight of the birds in the old-growth stands of the Andrews and showed definitively the owl's dependence on the ancient forest as nesting and hunting grounds. See Luoma's discussion in *The Hidden Forest*, pages 155-160. Also, Eric D. Forsman, E. Charles Meslow, and Howard M. Wight, *Distribution and biology of the spotted owl in Oregon* (Washington, D.C.: Wildlife Society, 1984), a paper based largely on Forsman's 1975 MS thesis, entitled *A preliminary investigation of the spotted owl in Oregon*, and Forsman's 1980 Ph.D. thesis, entitled *Habitat utilization by the spotted owl in the West-Central Cascades of Oregon*.

[131] Franklin, et al., *Ecological Characteristics* 24-26.

epiphytic biomass was *Lobaria oregana*, which Denison approximated as 5 percent of the biomass of the Douglas-fir's own foliage.[132]

The research methods employed in the canopy surveys were as novel as the findings they helped to establish. For Douglas-fir that routinely soar to heights between 165 and 295 feet, the task of studying the unfamiliar territory in the canopy posed a serious practical challenge, which required new techniques in order to access and sample the canopy. A year after the initial epiphany about the nitrogen-fixing capability of *Lobaria oregana*, Denison arranged to have an old Douglas-fir felled in the summer of 1970, in order to bring the canopy down to the ground for easier study. The experiment was a disaster "because the surface of the trunk which hit the ground was destroyed and the branch systems with their epiphytes were shattered and scattered."[133] As an alternative, Diane Tracy, Denison's undergraduate research assistant with rock-climbing experience, suggested the giant trees could be climbed safely by implementing the same techniques used to scale sheer rock faces. During an initial ascent, the climber was safely belayed by another climber on the ground as she bolted steel hangers into the tree. Then, on a fixed climbing rope attached near the top of the tree, climbers pulled themselves up through the canopy with webbing stirrups and

[132] Denison, "*Lobaria oregana*" 269.
[133] William C. Denison, Diane M. Tracy, Frederick M. Rhoades, and Martha Sherwood, "Direct, nondestructive measurement of biomass and structure in living, old-growth Douglas-fir," *Research on Coniferous Forest Ecosystems: First Year Progress in the Coniferous Forest Biome, US/IBP*, Jerry F. Franklin, L. J. Dempster, and Richard Waring, eds (Portland, OR: Pacific Northwest Forest and Range Experiment Station, Forest Service, U.S. Dept. of Agriculture, 1972) 147.

specialized rope clamps called jumars. Once aloft, the climbers installed a horizontal spar on the tree's trunk that enabled them to extend their observations far out into the branch system.[134] By the 1972 Bellingham conference, five trees had been rigged for climbing, enabling a more complete understanding of the structure of a large tree and the composition of species that use the old-growth canopy as habitat.

The ability to research epiphytes in the high canopy with minimal disturbance to the system allowed thorough quantitative study of the forest's nitrogen cycle, as well. Though old Douglas-fir played the obvious roles as the forest's "photosynthetic factory" and a "storehouse" of organic matter, Denison's work revealed the canopy of the large trees as "a complex system which accumulates, stores, and releases nutrients in ways paralleling those of the forest floor."[135] In addition, research on the ecology of canopy epiphytes unlocked a new appreciation of the forest's climactic buffering capacity and the canopy's unique role as habitat. Unlike most other forest systems, including young-growth Douglas-fir and mixed hardwood-coniferous stands, old-growth canopies can hold up to 264,000 gallons of water per acre, a characteristic that produces an inverse relation between moisture and temperature in the canopy. As precipitation increases, the average temperature of the canopy decreases. Likewise, in dry periods of the summer, temperatures in the canopy can exceed 104 degrees

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[134] Denison, et al., "Direct, nondestructive measurement" 148-153; this article provides thorough explanations and descriptive diagrams of the access techniques used in studying the canopy.
[135] Denison "*Lobaria oregana"* 267.

Fahrenheit. Denison found that these two environmental factors – moisture and temperature – had an impact on the nitrogenase activity of *L. oregana*'s blue-green alga. When the lichen's moisture content dropped below seventy percent, nitrogenase activity ceased, and the lichen assumed a state of dormancy that, Denison surmised, provided protection against extreme temperatures. Thus, *Lobaria oregana*, the dominant epiphyte and the main nitrogen-fixer in old-growth Douglas-fir forests, appeared to require these unique "habitats where moist conditions are always associated with cool temperatures."[136]

In effect, a simple question regarding the origin of nitrogen in the forest ecosystem precipitated a more nuanced and complex view of living, old-growth trees beyond the obvious recognition of their vital role as the primary sites of photosynthesis. The compositional, structural, and functional aspects of the interaction between *Lobaria oregana* and its exclusive habitat in old-growth Douglas-fir often overlapped, reflecting the nature of the complex system. Nevertheless, the initial objective of finding the source of nitrogen acquisition in the forest was a process-oriented inquiry, an attempt to explain the function of the system that naturally led to new discoveries about the structures that accomplish those functions.

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[136] Franklin et al., *Ecological Characteristics* 24-25; Significantly, hydrated *L. oregana* transplanted to stands of young Douglas-fir failed to thrive because the temperature and moisture regimes of the newly-established trees held "insufficient moisture for adequate thermal buffering."

A Second Case Study: Seeing the Fallen Tree's Unseen World[137]

In addition to their roles as distinctive habitat and crucial contributors to nutrient and water cycling, large Douglas-fir trees were the sole source of the other critical structural features of an old growth forest: standing snags and fallen logs. During the years of the Biome's operation, scientists slowly began to recognize dead trees, especially logs on the forest floor and in streams, as unique to old forests and important components of the ecosystem's ecology. Living trees had obvious roles in the ancient forest, yet dead wood had been largely ignored prior to the 1970s. Thus, the discovery of the role of fallen trees and the revelations it yielded about the ecosystem's complex structure and function were even more startling, in some ways, than the new views of their living counterparts.[138] Jerry Franklin has humorously mused about the great number of times he quite literally stumbled over dead and rotting logs in the forest before realizing their importance to the ecosystem.[139] Botanist Art McKee told a similarly ironic story from the Andrews experience, in which scientists performing baseline surveys for biomass

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[137] This section heading makes reference to another influential Forest Service technical report, entitled *The Seen and Unseen World of the Fallen Tree* and published in 1984. The full citation will be referenced in the later discussion of the report and its content.

[138] Although standing snags served as important nesting habitat for birds and rodents, their role in energy and nutrient cycling was essentially the same as down trees. The primary functional difference between them was found to be the rate of deterioration, with standing snags decomposing much faster due to weather exposure and use as habitat. (Franklin, et al., *Ecological Characteristics* 29.)

[139139] Busby, footnote 101; Dr. Ron Doel was very kind to send me an electronic copy of Ms. Busby's thesis, in which the page numbers did not match the table of contents. Though I cannot be sure of the proper page, footnote 101 is the citation for the Franklin anecdote.

in the streams of the Andrews grew frustrated by clambering over the tangle of fallen wood "that they otherwise weren't paying attention to."[140] A nuisance to the scientist maneuvering among them and "nearly as conspicuous as the large, live trees," jumbles of massive logs in various states of decomposition were actually the principal components in the forest's floor and streams.[141]

Unlike Denison's sudden insight regarding *Lobaria oregana* and the nitrogen fixation of the old-growth forest, the realization of the ecological importance of dead logs was a gradual one, built upon many small studies conducted at the Andrews Forest during the Biome years. The pieces of contributing research came from a range of scientific specialties, but the studies that contributed most substantially to the new understanding of dead wood in ancient forests all arose from questions about the forest's function and structure. How did the forest conduct its most vital processes, like, primary production, decomposition, consumption, and storage of nutrients and water? At what rate did energy, nutrients, and water flux through the system? Which organisms or groups of organisms carried out those processes, and what interrelationships existed between them? These inquiries, often directed at organisms or systems for which logs and snags seemed of little consequence, proved productive, as the Andrews scientists slowly began to see the structural and functional roles of dead wood in forests.

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[140] Luoma 84-85.
[141] Franklin et al., *Ecological Characteristics* 31.

Logs, often called "course woody debris" by the Andrews researchers, began to receive scientific attention early in the project, when the Biome reconnoitered the composition and biomass of the stands of old-growth Douglas-fir and the small creeks running through them on Watershed 10. During the first two granting periods of the CFB, a team of freshwater ecologists and fisheries biologists concentrated on the abundance and significance of detritus from the terrestrial system, called allochthonous material, as an input to the aquatic food webs in the Andrews. Previous studies of the highly managed forests in the eastern United States found that particulate organic matter from the floor and canopy of a forest was the chief source of energy for primary production processes in small woodland streams.[142] Based on this literature, the scientists, a group consisting of Jack Lyford, Jim Hall, Norm Anderson, and Dave McIntire in the first years of the CFB, focused on the contributions of leaves, twigs, and other litter, rather than the larger trunks that formed a labyrinthine network of wood in and around the stream.[143]

Gradually, as the stream researchers attempted to compile budgets and calculate fluxes for organic matter, their strategy and conceptualization of streams in old-growth, Douglas-fir ecosystems underwent reorientation. The streams in the eastern studies had only a small amount of large woody debris, due to decades

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142 CFB, Year 2 Proposal, Volume 2 Appendix 8.93.

143 CFB, Year 2 Proposal, Volume 2 Appendix 8.93; These scientists, along with fisheries specialist Jack Donaldson and Chuck Warren were the original faculty members of the "Stream Team," a highly productive stream ecology research group that made major contributions during the IBP and afterward as part of the LTER.

under a management strategy that deemed wood-clogged streams as inefficient and the dead logs in the waterways as impediments to fish.[144] By comparison, wood filled the streams in the Andrews, and the stream ecologists found themselves forced to account for the large amount of carbon bound up in the large masses of water-logged dead wood. Soon, Norm Anderson began to notice a diverse spectrum of invertebrate communities on the logs in streams, thriving on the microbial and algal accumulations on the soft wood and, in specialized cases, on the wood itself. The stream ecologists also determined that some microbes on the moist surface of the wood were nitrogen fixers, slowly increasing the concentration of the critical nutrient in the log as it decomposed and providing up to ten percent of the stream's nitrogen supply.[145]

Scientists also began examining the role of course woody debris as components of the structure of small streams. Far from impeding fish activity, large logs provided fish with cover from predators and trapped sediment and gravel, forming shaded spawning habitat. Fred Swanson also demonstrated the role of logs in shaping stream morphology, stabilizing the stream's bed and banks and retaining organic matter that would otherwise be transported downstream.[146] Taken together, the findings of the "Stream Team" in the CFB studies in the

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[144] Luoma 87-89.
[145] Chris Maser, James M. Trappe, Steven P. Cline, Kermit Cromack, Jr., Helmut Blaschke, James R. Sedell, and Frederick Swanson, *The Seen and Unseen World of the Fallen Tree*, Chris Maser and James M. Trappe, Technical Editors (Portland, Oregon: Pacific Northwest Forest and Range Experiment Station, U.S. Dept. of Agriculture, 1984) 42-46.
[146] Franklin et al., *Ecological Characteristics* 39.

Andrews signified major advances in the understanding of aquatic primary productivity and nutrient cycling and startling realizations about the role that logs and other detritus played in those processes.

Within terrestrial research, functional questions about nutrient cycling and decomposition led to equally surprising and complex outcomes. In Watershed 10, down logs occupied approximately a quarter of the forest floor's area and averaged a weight of 85 tons per acre.[147] The sheer mass of the fallen trees represented an enormous accumulation of carbohydrate produced by photosynthesis and an equally large supply of nutrients like nitrogen and phosphorous that trees pulled from the ground during their lifetime.[148] Microbial and fungal decomposers were known to break down and channel the stored nutrients and primary productivity to the ecosystem in accessible forms. Therefore, in the Biome's research, the responsibility for elucidating the processes by which this vast supply of nutrients, water, and carbon became accessible fell to mycologists and microbiologists.[149]

At Oregon State, Bill Denison and his fellow OSU fungi specialist, Jim Trappe, sought to catalog the soil fungi in coniferous forest soils and their

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[147] Franklin et al., *Ecological Characteristics* 31.
[148] Franklin et al., *Ecological Characteristics* 31; Per acre, logs were found to contain as much as 192 pounds of nitrogen and 7.6 pounds of phosphorus. In addition to this citation, see R.L. Edmonds, ed., *An Initial Synthesis of Results in the Coniferous Forest Biome, 1970-1973*, Coniferous Forest Biome Bulletin No. 7 (Seattle, Washington: University of Washington, Coniferous Forest Biome Central Office, September 1974) 64, as well as C.C. Grier and Robert S. Logan, "Old-Growth *Pseudotsuga menziesii* Communities of a Western Oregon Watershed: Biomass Distribution and Production Budgets," *Ecological Monographs*, Vol. 47, No. 4 (Autumn 1977): 373-400.
[149] CFB 1970 Proposal, Appendix C, Research Proposals A-128, A-129.

associations with living plant hosts and non-living litter and wood. In addition to their role in decomposition of forest detritus like fallen logs, fungi were critical in facilitating nutrient cycling by acting "as 'bridges' through which materials are exchanged, both between the root systems of vascular plants and the soil and between the root systems of different individuals, or even different species."[150] Denison shortly discovered the nitrogen-fixing epiphytes and turned his attention to the canopy, but Trappe remained concentrated on soil fungi, specifically species of fungi that form symbiotic relationships with the roots of plants called mycorrhizae.[151]

Trappe's research for the Biome related to the role of mycosymbionts in nutrient cycling. This work had little to do with down logs, however, until the mycologist met Chris Maser, a mammologist who focused on small rodents like squirrels and voles. Working together, Maser and Trappe established that many small mammals depended on mycosymbionts as a food source and, in return, spread the spores of fungal species that have no other mean of distribution. Due to the fact that fungal symbionts feed their associated tree roots with water, nutrients, and minerals they extract from the soil, Trappe also knew the mycorrhizae were important in the healthy establishing of trees. Trappe and Maser eventually realized the relationship of the fungi, the mammals, and the trees highlighted an unforeseen role of dead, fallen trees, as well. In addition to providing protected nesting sites in their soft, rotting wood, down logs secured small mammals safe

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[150] CFB 1970 Proposal, Appendix C, Research Proposals A-84.
[151] CFB Year 2 Proposal, Volume 2 Appendix 8.86.

passage into open areas of forest caused by cuts, windfall, or other disturbances. The rodents spread the necessary fungal spores on and around the rich seedbed of the decomposing "nurse" logs, thus aiding the formation of the fungal hyphae and promoting the survival of tree seedlings in exposed, disturbed areas.[152]

In both the terrestrial and the aquatic research programs, studies motivated by a lack of understanding concerning ecosystem function also led to increased knowledge about the structural and habitat components of the forest. As the scientists of the Biome clarified both the structure and function of the forest, a broader function of the ecosystem became evident, the forest's resilient mechanism of response and recovery after disturbance. Terrestrial logs are extremely long-lasting features, often present on the forest floor longer than the lifespan of the trees that formed them. The work of Maser, Trappe, and others demonstrated that the persistence of terrestrial logs as structural components in an old-growth forest provided the system with a nearly continual supply of nutrients and moist habitat for rodents, amphibians, fungi, and a wide array of insects, all of which had the function of shielding the forest against disturbances and helping it to regenerate afterward.[153] In the Andrews forest, CFB investigations of course

[152] Franklin et al., *Ecological Characteristics* 34-35.
[153] Luoma 85; This finding had obvious implications for management practices, that often prescribed clearing or burning of slash after clearcutting and salvage logging of dead wood in forests. Maser and Trappe, according to Luoma, were the first federal scientists to claim that contemporary forestry practices contained serious flaws. (See Luoma 144) On page 47-49 of *The Seen and Unseen World of the Fallen Tree*, the two scientists warned of the dangers of removing logs from Douglas-fir ecosystems. "We must not," Maser and Trappe insisted, "sacrifice the

woody debris showed the longevity of the forest's structures and an interrelated web of functions both strengthened the individual components of the ecosystem and increased the resilience of the forest as a whole.

Dynamics of the Field and the Mainframe

The critical ecological roles played by seemingly mundane or overlooked forest structures, like *Lobaria oregana* and dead wood, often came as complete surprises to the field researchers, a fact that attested to the power of the functional vantage. The innovative focus of the Biome's field-based observational studies also seems to resonate with the modelers' goals of expressing and predicting ecosystem functions through computer simulations. A shared orientation toward questions of process and investigations of ecosystem function seemed to have perpetuated, if not directly caused a new view of the forest's complex interrelationships. Within the framework of Biome research, the flux of water, nutrients, and carbon served as common denominators in the system, causing the barriers of taxonomic classification and disciplinary specialization to break down. Interdisciplinary collaboration, like that of Maser and Trappe, as well as the intricate network of dependency between trees, fungi, and animals that the two scientists elucidated, became apparent with the ecosystem approach and its functional questions.

As evidenced by the variable success of Scott Overton and Phil Sollins in integrating the Biome's modeling work with the daily collection of data, the field

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options of future generations on the altar of cost-effectiveness through decisions based on insufficient data."

research of the CFB fluctuated widely in its dedication to and incorporation of the original modeling objectives for the overall project. An ecosystem perspective and a functional approach to fieldwork did not always equate with a commitment to the construction of an ecosystem model. This seems to imply that, despite common interests in forest ecology and ecosystem function, many of the Biome participants maintained or developed research goals other than the modeling objectives specified by the Biome programs. As this discussion is not an attempt to give a comprehensive overview of the CFB's field-based projects and their motivations, two examples will help to elucidate the nature of these subsidiary, field-based goals. Although these examples existed at opposite ends of the spectrum of integration with modeling, both instances were productive areas of field work and illustrate the dynamics between the research on the ground and the integrative modeling work on the mainframe.

First, Jerry Franklin and his Forest Service colleague, Ted Dyrness, eager to learn more about the growing field of ecosystem ecology, decided that such ecological work would benefit from more information regarding vegetation communities in the Oregon Cascades. The Forest Service scientists began a major reconnaissance-level study of forest vegetation in the Andrews in 1967. Dyrness and Franklin spent much of the three field seasons prior to the initiation of the Coniferous Biome hiking through the Andrews describing vegetation in as many different locations and conditions in the forest as possible.[154] Before the CFB

[154] Conversation with Ted Dyrness, 17 December 2004.

submitted its first proposal in December 1969, Dyrness and Franklin compiled a preliminary classification of forest vegetation assemblages, which arrayed themselves in strata based on moisture and temperature gradients. The quality of the stratification scheme made it a natural project to receive funding from the Biome, as it provided the basis for extrapolation of research results to a watershed level through a series of well-studied "reference stands" for each documented vegetation community.[155] The work added substantially to compositional aspect of the synthetic picture of the forest conveyed in the *Ecological Characteristics* monograph. Yet, the functional questions that arose through the modeling projects never directly motivated Dyrness and Franklin's project. In fact, the vegetation classification was one of several research projects that pre-dated and outlived the IBP era and its modeling objectives, a fact that suggests subsidiary goals in projects such as Franklin and Dyrness' were operating independently but intersecting briefly with the overarching modeling goals of the CFB, possibly as a way of tapping into the IBP's vast funds to continue their research.[156]

As a second example, the group of Oregon State University stream scientists that coalesced around aquatic-terrestrial interactions and stream ecology represented a very different relationship between fieldwork and models. During the Biome years, Jim Hall, Norm Anderson, Jack Lyford, and Dave McIntire, with

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[155] C.T. Dyrness, Jerry F. Franklin, and W.H. Moir, "A Preliminary Classification of Forest Communities in the Central Portion of the Western Cascades of Oregon," Coniferous Forest Biome Bulletin 4 (Seattle, Washington: University of Washington, Coniferous Forest Biome Central Office, October 1974).
[156] I am indebted to Paul Farber for suggesting the overarching and subsidiary goal structure as a way of logically conceptualizing the Biome.

the eventual additions of Jim Sedell and Stan Gregory, formed the core of a strong and effective research program in stream ecology that playfully dubbed itself the "Stream Team."[157] Over the years, the composition and structure of the research team was flexible, ebbing and flowing depending on available grant money and incorporating peripherally-involved scientists, such as Phil Sollins, Kermit Cromack, and Fred Swanson, as it benefited the stream research to do so.

The scientists made groundbreaking discoveries about the food web in streams of the Andrews, simultaneously producing a subsystem model. Primarily built by McIntire, the stream model reflected the findings on trophic dynamics. In addition, the model performed the scientific functions originally envisioned for models by the CFB: to generate hypotheses, to synthesize the results of field and laboratory work, and to generally help guide the cycle of research.[158] McIntire's approach echoed Scott Overton's emphasis on modeling as a process rather than a product. McIntire believed that researchers can "learn so much in the process of modeling that modeling and fieldwork should work hand in hand." According to McIntire, this close relationship of modeling and field studies "worked pretty well

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[157] Conversation with Stan Gregory, 09 December 2004.

[158] CFB 1970 Proposal, A103-A104; C. David McIntire, *A Tutorial and Teaching Guide for the Use of a Lotic Ecosystem* (Corvallis, Oregon: OSU Department of Botany and Plant Pathology, c. 1995) 4-5; Dr. McIntire generously lent me a copy of this self-published handbook. The Stream Model is presently used in an educational capacity, as well as for generation of new hypotheses on trophic processes in streams.

in the aquatic program," though he remembered Overton's central modeling effort "had virtually no sway over fieldwork."[159]

Why was McIntire's Stream Model able to synthesize and guide the research of the Stream Team using the same philosophies as Overton and Sollins, whose own attempts at a total ecosystem model for integration and direction the Biome research fell short of their goals? Several answers to this question are immediately apparent. First, the Stream Team constituted a much smaller research group, thus facilitating communication and direct cooperation. The Biome expected its total system model to serve a similar purpose: to aid collaboration in a research project with far-flung research sites and researchers of many specialties. It seems, however, that a model was no substitute for the close, productive relationships of a tightly-knit research group. The Stream Team also had a number of dynamic leaders, all committed the acquisition of ecological understanding about stream systems through a cycle of fieldwork and modeling. Moreover, perhaps, the Stream Team of the IBP era was able to find a productive balance of research styles and personalities. Stan Gregory referred to this balance in their group as its "yin and yang." Whereas Jim Hall was very orthodox, thoughtful, and reserved, Jim Sedell had an "energy and spark" that invigorated the whole group and encouraged its members to work together. Similarly, Cromack's patient listening balanced Phil Sollins' often fiery temper. Swanson was sensible and congenial, working well with everyone on the team to expand the process research

[159] Conversation with Dave McIntire, 13 December 2004.

to a landscape level.[160] Such balances and collaboration proved difficult to establish across a project as large as the CFB. Finally, whereas the models of Overton and Sollins never fully proved their utility, the Stream Model demonstrated itself to be a useful tool in the cycle of research.

The Stream Team still actively engages in vigorous research at the Andrews, much of it fieldwork built upon the foundational discoveries from the 1970s. In other ways, as well, aspects of the Stream Team's present work remain artifacts of the IBP era. McIntire built a stream subsystem model for the IBP according to the conceptual framework of Overton, and the model, like the research team, is constantly being modified and implemented in current research.[161] As Stan Gregory put it, if one "looks for what is under the hood" of the model, the extent to which the Stream Model still borrows from the IBP modeling ideas is marked.[162] This example requires a reevaluation of the Biome's central modeling effort as a "failure." The presence and vitality of the stream subsystem model, a subsidiary yet highly productive project of the IBP, speaks to the importance of the Biome and its modeling work as catalysts for many research projects, even in the absence of the idealized objective of a total system model.[163]

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[160] Conversation with Stan Gregory 09 December 2004.
[161] Conversation with Dave McIntire 13 December 2004.
[162] Conversation with Stan Gregory 09 December 2004. In addition to his clever phrasing, I thank Dr. Gregory for his example of IBP models surviving beyond the termination of the IBP, as it sparked valuable revisions to my conceptualization of the relationship of modeling and field-based goals in the Biome.
[163] It is worth noting that there were a number of isolated modeling projects going on within the CFB during its years of operation. At the Cedar River site in Washington, Ken Reed strove to model forest succession, producing a model

These examples reveal possible relationships between the overall modeling goals and the various subsidiary goals that a superficial look at the inputs and outputs of a large program like the Coniferous Forest Biome cannot discern. In the first example, Franklin and Dyrness' classification scheme shows that there were subsidiary goals that were pre-existing to, working underneath, or perhaps even unconnected with the attempt at constructing a total system model. At the very minimum, there was a financial relationship linking the two sets of goals. Autonomous studies, which gleaned no direct motivation from models or even functional processes in the forest, still had the opportunity to access NSF funding through the IBP, if promoted in a way that appealed to an ecosystem perspective. Occasionally, however, as in the case of the Stream Team, the modeling objectives, at least at the subsystem level, had vast importance within a largely field-based study, if not a direct impact on how the research was directed.

The CFB's Legacy at the Andrews: Long-Term Ecological Research (LTER)

Regardless of whether the field researchers in the Andrews adopted the objectives of Scott Overton's modeling effort and the fundamentals of his philosophy, the post-IBP scientists embodied his desire for a far-sighted research endeavor. The frugal budget of the 1976 "Watershed" grant demanded discontinuation of the modeling effort that had been so central to the original

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called SUCSIM (see CFB Bulletin 11), and Paul Jarvis and others, including Waring and his students, attempted to model aspects of tree physiology. Many of these models tested ideas with field and drew upon Overton's philosophies and procedures; however, most of them seem to have been largely unconnected with the central Biome modeling group.

concept of the Biome projects. Nevertheless, the attempt at a whole Biome model indirectly influenced the course of field work. Research in the Andrews after the IBP period carried forward a number of discoveries that, initially, had been stimulated by the need to look at the forest and its processes in the detailed manner that the modeling efforts of the CFB required. The fruitfulness of the more observational investigations during the CFB effectively produced a legacy of place, people, and scientific questions at the Andrews that continued, hardened, and, eventually, became institutionalized when the forest became part of the NSF's new network of Long-Term Ecological Research (LTER) sites in 1980.

In conversations with members of the Andrews research group, an appreciation for the physical location of the Andrews forest and the personal and professional friendships fostered there remains evident. For many of the scientists drawn to the study of forests, this appreciation, as Jerry Franklin described it, was "an intuitive feeling that these forests have value."[164] Elsewhere, members of the Andrews group have repeated different variations of this theme, commenting that their work as scientists is "motivated by a sense of place… it just had an internal resonance that very few things I'd ever encounter had, and it almost invariably necessitated a kind of awareness of the landscape."[165] Beyond aesthetic or spiritual appeal, the Andrews served the critical role as a "seedbed of discovery" during the Biome years, forming a lasting, productive ecological research program

[164] Dietrich 102.
[165] Antypas 150; Antypas quoted liberally from interviews with Biome participants but decided to withhold the identities of his interviewees, perhaps due to a confidentiality issue or a desire to be objective in his sociological analysis.

through "long-term relationships between people on one piece of real estate."[166] Breakthroughs in the ecological understanding of the forest rarely happened suddenly, however. The watersheds on the Andrews served as communal places where senior scientists to graduate students alike lived in dilapidated camping trailers and worked side by side, often "slogging through mud and climbing steep slopes" as teams.[167] The physical and social experience of working in the Andrews over many field seasons facilitated easy exchange of ideas. This form of research also bound the scientists together as a definable research unit, even leading them to adopt the moniker of "the Andrews group" for their team.

The legacy of the IBP-era in the Andrews manifested itself in several other ways. At the national level, the NSF's support of ecological research through the LTER sites was a direct result of the relationship the ecological community established with the governmental funding agency during the IBP.[168] Locally,

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[166] Conversation with Fred Swanson, 24 September 2004.
[167] Conversation with Phil Sollins, 15 September 2004.
[168] Conversation with Dick Waring, 07 July 2004. Another significant outcome of the CFB's interaction with the NSF was the precedent it set for scientific research supported on soft money at OSU's School of Forestry. Carl Stoltenberg, the dean of OSU Forestry in 1970, viewed the plan of studying undisturbed forests at odds with traditional forestry (since he believed that the old forests would probably be harvested within ten years) and frowned on supporting faculty solely on soft money. The scale of the CFB grants – approximately $1 million dollars every year in today's currency – was unprecedented for a single research project in OSU's Forestry program, both before and since the IBP. The Biome administered these grants, however, so Stoltenberg had no voice in the money's distribution and usage. The enormity of the CFB soft money also enabled the Biome to hire many researchers who effectively "diluted the field with people who did not think like foresters." The OSU College of Forestry still bears the marks of these events: an ecologically-minded "Forest Science" department, quite distinct ideas and research agendas from its neighbors in Forestry, has grown up on the strength of CFB-era

many of the scientists who worked in the Andrews during the Biome years as post-doctoral fellows and graduate students eventually obtained professorships in OSU's College of Forestry or jobs with the Forest Service Research Station in Corvallis that enabled them to continue their research in the Andrews. Trained in a research program that emphasized ecological processes, these individuals embodied the "new type of scientist" that Allen Hammond's analysis of the US/IBP felt certain the large research endeavor would produce.[169] Stan Gregory, who first came to Oregon State as a master's student in fisheries in 1971 and received a professorship in 1986, now leads the Stream Team and its research on the physical and trophic dynamics of streams. Mark Harmon, initially a student of Franklin toward the end of the Biome era, is at the beginning of a centuries-long study of decomposition of woody detritus and the process by which the forest's fungi, bacteria, and insects make the nutrients and organic matter of wood available.[170] Coming to the Biome work from a geology background, Fred Swanson "tagged along" with the Andrews group in the 1970s, gradually uncovering the connections between the shifting and slumping of the Andrews' soils and slopes and the forest ecosystem that was slowing moving with it.[171] By

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work in the Andrews and soft money is now the rule in research science, rather than the exception.

[169] Hammond 48; Writing in 1977, W. Frank Blair's analysis of the impact of the IBP in the United States also lists a similar observation as a reason that the US/IBP constituted the major advances in ecology: "the genesis of a whole generation of ecosystem modelers, generally identifiable by their youth, their haircuts (long) and their attire (informal)." See Blair, *Big Biology: The US/IBP* 163.

[170] Luoma 78-80.

[171] Luoma 175-176; conversation with Fred Swanson, 24 September 2004.

the late 1970s, Swanson was an invaluable member of the Stream Team and the Andrews group more generally, and, in 1986, he took over direction of the Andrews Forest when Jerry Franklin left Corvallis to join the faculty at the University of Washington's College of Forest Resources.

The present-day Andrews scientists who cut their teeth on their experiences during the CFB represent a continuity of scientific inquiry in the Andrews. Many of the scientific questions regarding processes and functions of Douglas-fir forests that the Andrews group began asking in the IBP era are still in play today. How does the forest obtain and circulate its energy, nutrients, organic matter, and water? What structures or ecological connections enable the forest to function in this way? After witnessing and studying the decimation and rebirth of forest communities around Mt. St. Helens after its eruption in May of 1980, the Andrews scientists returned to questions regarding how the forest's persistent, long-lived structure and interconnected function respond to major disturbances.[172]

The current reputation of the Andrews scientists, however, emerged from another set of questions. From the perspective of the new ecosystem understanding of the old forests at the Andrews, how do current forest management practices compromise the ability of the forest to persist and regenerate itself? How can a forest be managed to preserve its ecological integrity? During the 1980s and 1990s, the Andrews group became an active expert body in the ongoing debates over resource management, issues that sparked

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[172] Luoma 13-15.

U.S. involvement in the IBP in the first place. Ironically, the vehicle for addressing these concerns was not, ultimately, the tools of systems analysis that initially constituted the core of the CFB research program. Rather, the rich vein of field-based research that grew out of the CFB's modeling efforts proved to be a powerful influence on the course of forest ecology at the Andrews and on forest management policy. The legacy of the Coniferous Forest Biome at the H.J. Andrews Experimental Forest has, in this way, come full circle.

CONCLUSION: The "Delayed Relevancy" of the Coniferous Forest Biome

In the introduction to a 1997 volume entitled *Creating a Forestry for the 21st Century: The Science of Ecosystem Management*, Jerry Franklin reflected on the lessons learned in the forests of the Andrews, observing that, "(i)f nothing else, the most important result of ecological research on forest landscapes and ecosystems has been an appreciation of their complexity and the limitations of our knowledge."[173] Indeed, the initial investigations in the Andrews Experimental Forest during the IBP period produced scientific justification and humbling recognition of the intricate structure and function of old-growth forests. The Andrews team discovered a unique and thriving ecosystem in forests previously thought to be overgrown, decadent, and inefficient. This realization made both the scientists and their work in the Andrews critical factors in the debates in the United States over the value of old-growth forests and forest policy in the late 1980s and early 1990s. Franklin himself became an icon in the drive to mediate the void between the new ecological knowledge and contemporary forest management practices. Building on the discovery that the structural components of downed logs were critical to the recovery of a forest after natural disasters, Franklin and his Andrews colleagues suggested that forest harvesting should attempt to mimic large

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[173] Kathryn A. Kohm and Jerry F. Franklin, "Introduction," *Creating a Forestry for the 21st Century: The Science of Ecosystem Management*, edited by Kathryn A. Kohm and Jerry F. Franklin (Washington, D.C.: Island Press, 1997) 5.

fires, landslides, or windfall, leaving behind a few large, living trees, snags, and down trees to maintain habitat and aid the healthy regeneration of trees.[174]

As Franklin and the Andrews group began to promote this conception of a "New Forestry" in the mid-1980s, public concern and pressure from environmental lobbyists regarding logging practices was rising, especially in the Pacific Northwest. By the early 1990s, the endangered Northern spotted owl had become an emblem of the old-growth debates that ground federal timber sales to a halt. Simultaneously, the Andrews team, many of them Forest Service employees, continued to develop their ideas on ecologically-minded methods of logging, reforming federal forest management policy from within the ranks of the agency.[175] In response to a call by the newly-inaugurated President Clinton to end the "gridlock" over logging in the Pacific Northwest, the Forest Service announced in 1993 that it would begin to implement the ecosystem management approaches and alternatives to clearcutting.[176] By integrating knowledge of the forest ecosystem that surfaced from field studies at the IBP-era Andrews into forestry practice, Franklin's "New Forestry" program could be viewed as producing the "more intelligent management and use of lands" that the Williamstown meeting in 1966 and the CFB's initial proposals envisioned as the outcome of whole ecosystem studies.

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[174] Luoma 160-163.
[175] Luoma 162-163.
[176] Jack Ward Thomas, "Foreward," *Creating a Forestry for the 21st Century: The Science of Ecosystem Management*, edited by Kathryn A. Kohm and Jerry F. Franklin (Washington, D.C.: Island Press, 1997) *x*.

Fred Swanson described the Andrews work of the 1970s as exhibiting "delayed relevancy" in debates over the value and management of old-growth, Douglas-fir forests. These implications of CFB research remain an important legacy of the IBP. Many historical and popular treatments of the forest ecology at the Andrews, however, focused narrowly on the social, political, and environmental fallout of the ecological breakthroughs while ignoring the motivations and events that surrounded the breakthroughs themselves. This thesis attempted to correct for a portion of this deficiency. By honing in on the critical period of the Coniferous Forest Biome, a shift in research objectives comes into view, pivoting from the construction of a total system model to a series of foundational field studies in the Andrews forest. The exciting new findings on the forest's complex structure eventually overshadowed the initial modeling objectives, but the need for functional understandings of nutrient and water cycling in the ancient forests initiated a new observational style that enabled the CFB's field scientists to see the composition and structure of the forest more clearly.

A focused study of the CFB science also revealed the ecosystem research program as a product of the dynamic and tenuous state of ecosystem ecology in the 1960s. Ecosystem ecology emerged as the central feature of a large-scale biological study, due in large part to intriguing quality of the ecosystem concept, its resonance with rising anxieties about human survival in a changing environment, and the momentum of its proponents' own enthusiasm for its potential. The weaknesses of ecosystem theory and the disagreements over the role of systems analysis and computer simulation as the vehicle of theory development compromised the CFB's modeling effort. Contrary to the hopes of the modeler Scott Overton, the attempt at the total system model of the coniferous forest failed to develop general ecological theory. Contrary to the expectations of the administrators, the model never achieved the desired purpose of integrating a large, multidisciplinary study.

How useful, then, are ecosystem models within big ecological studies? How fruitful are large, interdisciplinary ecological research projects at all? From the example of the Coniferous Forest Biome, the most productive research settings were most certainly the ones in which scientists from different specialties areas collaborated around a campfire in the Andrews forest or in small groups, as was the case with the Stream Team and the weekly Biome brainstorming meetings. The modeling efforts of small teams were also far more successful than the total system model attempted by Overton during the Biome years. Most likely, the small-scale models of the Biome, such as the stream model of McIntire, Reed's model of forest succession, and the various models of tree physiology, benefited from the same kind of easy communication and close cooperation as other small groups of scientists and principal investigators in the Biome. The total modeling effort, however, suffered from unrealistic expectations of its ability to integrate diverse research and forge better communication between personnel and institutions. In addition, the Biome's limited duration and obligations to its funding agency required a workable model to be produced as an end goal. As vehemently as

Overton fought against the transformation of his modeling work into a product, he might well have agreed that a highly refined though, by definition, imperfect model for the use of resource managers was possible. The point of historical importance, however, was that Overton was not building a model for the implementation of ecological knowledge in a management setting. Rather, Overton intended his model as a vehicle for the development of ecosystem theory, a process that was ongoing and necessary before a model could be valid for practical use.

Ultimately, the modeling effort of the Coniferous Forest Biome fell victim to its own priority among the original goals of the Biome projects. The structure of the research effort was basically hierarchical, with observations and data intended to flow upward from the field and laboratory work into the overarching, synthesizing model. Echoing the concerns of American scientists while the IBP was still in its planning stages in the early 1960s, researchers within the Biome were not willing to sacrifice or submit their own research goals to the collective model-building effort. Likewise, the very nature of scientific models as tools seems to have prohibited the total Biome models from becoming the encompassing focus of the Biome research. As Stream Team modeler Dave McIntire indicated, the fact that modeling was a process in which model development and field research worked "hand-in-hand" means that the goal of building predictive models of ecosystem dynamics can never be a singular one. Rather, in the most productive instances with the CFB, the model and the fieldwork contributed equally to the cycle of process study, modeling, and field validation, all toward the overall goal of bettering ecological understanding of the forest's complex system.

Evaluation of a large scientific research project like the Coniferous Forest Biome, therefore, requires a nuanced perspective that looks beyond the narrowly-conceived goals of the initial proposals and the constricted timelines imposed by limited funding. Consideration must be given to the large objectives of the overall project and its outcomes, as well as the subsidiary goals of individuals and small groups working within and underneath the broader project. Programs of the size and geographical distribution of the CFB seem to prove inefficient, despite the attempts of meetings and models to integrate their efforts. Within the Biome, small, productive research subunits coalesced and pursued the various subsidiary goals. Often intersecting briefly with the larger modeling effort through an initial functional question, these subsidiary projects diverged as the fieldwork developed different objectives.

In addition, the example of the CFB demonstrates the danger of judging the outcomes of a large project exclusively within the timeframe of its operation. The risk of such a restricted evaluation is especially true for ecological studies because of their focus on long-term relationships in nature. Despite many harsh criticisms, commentaries on the Biome projects during and directly after their conclusion realized the short-sightedness of their perspectives. The knowledge of the "delayed relevancy" of the ecosystem research of the Coniferous Forest Biome and this expanded picture of the Biome's changing ideas and methods of forest ecology during the 1970s enables a more thorough evaluation of the Biome.

The Coniferous Forest Biome, however, represents a much bigger story and suggests many additional routes of investigation that would enrich the present study. Aspects of the inter-institutional interactions between the University of Washington and Oregon State University, including the differences in individual styles, goals, and values between the two forestry research schools, provides a rich avenue through which to explore the intersection of ecology and traditional forestry in the 1970s. The intra-institutional relationships of the Andrews scientists with their employers, Oregon State University and the Forest Service, also offer venues in which to examine the borderlands between forest ecology and traditional forestry and provide a forum for the examination of how opposing points of view shape the future of an institution through ideological, political, and economic struggles. A comprehensive analysis of the place of the CFB within the environmental history and forest policy in the Pacific Northwest would be critical in a full and accurate understanding of the complex regional dynamics between ecological science, timber-based economics, and environmental values. Comparisons of the CFB's work with that of the four other Biome programs, in terms of the modeling efforts, overall scientific output, and the local contexts in which these research programs operated could yield valuable insights into both the potential and the limitations of the Biomes and their ecosystem-based research program. Finally, at the largest scale, the participation and contribution of the

CFB and the other Biomes to the international scientific efforts and resource management objectives of the IBP is an area that has yet to be fully explored and that would be a valuable addition to the growing body of historical research on internationalism in science.

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C. David McIntire 13 December 2004
Ted Dyrness 17 December 2004
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W. Scott Overton 05 May 2005 (Dr. Overton's residence, Philomath, OR)
Richard Waring 18 May 2005

For corroboration of dates and details discussed in the conversations, I relied in several instances on the archived personnel files from the College of Forestry at Oregon State University, currently housed in the Oregon State University Archives, The Valley Library, Corvallis, OR. These documents can be found in Record Group 139; subgroup 2; VI; SR 2/2/7/20 and SR 2/2/7/30. The following primary publications of the Biome were also helpful for confirmation of the Biome timelines related in the interviews:

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