

CAN WE DIAGNOSE THE HEALTH OF ECOSYSTEMS?

by


THOMAS CHARLES DEWBERRY

A DISSERTATION

Presented to the Department of Philosophy
and the Graduate School of the University of Oregon
in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy

June 1995

"Can We Diagnose the Health of Ecosystems?", a dissertation prepared by Thomas C. Dewberry in partial fulfillment of the requirements for the Doctor of Philosophy degree in the Department of Philosophy. This dissertation has been approved and accepted by:




Dr. Cheyney Ryan, Chair of the Examining Committee

June 18, 1995

Date

Committee in Charge: Dr. Cheyney Ryan, Chair
 Dr. John Baldwin
 Dr. Mark Johnson
 Dr. John Stuhr



Vice Provost and Dean of the Graduate School

An Abstract of the Dissertation of

Thomas Charles Dewberry for the degree of Doctor of Philosophy
in the Department of Philosophy to be taken June 1995

Title: CAN WE DIAGNOSE THE HEALTH OF ECOSYSTEMS?

Approved: _____

Dr. Cheyney Ryan

This study is a philosophical examination of the question, "Can we diagnose the health of ecosystems?" Two senses of this question are investigated: 1) Are ecosystems the kind of entities to which "health" applies? 2) Assuming that ecosystem health is a coherent concept, how do we diagnose the health of ecosystems?

This study begins with a distinction, first made by Michael Polanyi, between machines and holistic inanimate objects, such as thunderstorms. Thunderstorms are reducible to the laws of chemistry and physics, while machines are not. Machines have two levels of control, the laws of physics and chemistry, and operational principles, which harness the parts to achieve the purpose of the machine. The importance of this distinction is that the concept of health only applies to objects which have two or more levels of control. This study concludes that the concept of health applies to ecosystems. Ecosystems are not reducible to their parts.

The diagnosis of ecosystem health is similar to a medical doctor diagnosing the health of a patient, because ecosystems and humans are both members of the class of objects to which "health" applies. The diagnostician, with each specimen observed, simultaneously modifies the standard of normality for the class of object, at the same time

the individual is appraised according to the standard. Diagnosing the health of a patient is a skill which cannot be reduced to an objective measurable standard. However, ecosystems are not individuals, so diagnosing the health of ecosystems is not exactly analogous to diagnosing the health of a human or horse.

This study has important implications for resource management and policy. Procedures, such as the federal interagency watershed analysis, which are built on a hierarchical theory based on the rate of processes, make ecosystem health incoherent. The federal strategy appears to hold the implicit assumption that ecosystems are reducible to their parts. Watershed analysis is also an ambiguous procedure at best. It rejects the medical model, and it may destroy the skill of diagnosis, by attempting to replace it with a measurable standard.

CURRICULUM VITA

NAME OF AUTHOR: Thomas Charles Dewberry

PLACE OF BIRTH: Lansing, Michigan

DATE OF BIRTH: January 25, 1949

GRADUATE AND UNDERGRADUATE SCHOOLS ATTENDED:

University of Oregon
Oregon State University
Michigan State University

DEGREES AWARDED:

Doctor of Philosophy in Philosophy, 1995, University of Oregon
Masters of Science in Fisheries and Wildlife, 1978, Michigan
State University
Bachelor of Science in Multiple Disciplinary Social Science,
Michigan State University

AREAS OF SPECIAL INTEREST:

Philosophy of Science
Philosophy of Biology
Philosophy of Ecology

PROFESSIONAL EXPERIENCE:

Teaching Assistant, Department of Philosophy, University of
Oregon, Eugene, 1990-1991

Research Technician, U.S.D.A. Forest Service, Forest Sciences
Lab, Corvallis, 1981-1991

ACKNOWLEDGEMENTS

The author expresses sincere appreciation to my graduate committee: Professors Cheyney Ryan, John Baldwin, Mark Johnson, and John Stuhr. Special thanks are due Cheyney Ryan, who saw this project through from beginning to end.

TABLE OF CONTENTS

Chapter	Page
I. INTRODUCTION: CAN WE DIAGNOSE THE HEALTH OF ECOSYSTEMS?.....	1
Introduction.....	1
The Scope of This Study.....	8
The Structure of This Study.....	15
Notes.....	18
II. POLANYI'S REJECTION THAT BIOLOGY CAN BE REDUCED TO THE LAWS OF CHEMISTRY AND PHYSICS, AND ITS IMPLICATION FOR THE CONCEPT OF HEALTH.....	21
Introduction.....	21
Polanyi's Rejection of the Views of Francis Crick and Barry Commoner.....	25
Rejection of the Premise that Life is Reducible to the Inanimate Laws of Chemistry and Physics.....	27
Polanyi's Rejection of Francis Crick's Position.....	37
Boundary Conditions in Living Organisms and Their Implications for the Concept of Health.....	42
Polanyi's Position Necessitates Holding a Hierarchical Theory of Nature.....	49
Summary.....	55
Notes.....	59

	Page
III. IS ECOSYSTEM HEALTH A COHERENT CONCEPT IN CONSERVATION BIOLOGY?	62
Introduction.....	62
Development of the Ecosystem Concept in Ecology.....	66
Ecosystems as “Hierarchical” Systems.....	77
A Critique of Robert Costanza, et al.’s View of Ecosystems as Systems.....	81
A Critique of O’Neill, et al.’s View of Ecosystems as “Hierarchical” Systems.....	88
Is Ecosystem Health a Coherent Concept in Ecology?.....	105
Clarification of Position of This Study with Regard to the Current Debate in Conservation Biology.....	127
Notes.....	141
IV. CAN WE DIAGNOSE THE HEALTH OF ECOSYSTEMS? PART II.....	149
Introduction.....	149
The Nature of Skills and Their Role in the Aquisition of Scientific Knowledge.....	151
The Skill of the Medical Diagnostician and Its Implications for Ecosystem Health and Watershed Assessment.....	160
Summary.....	170
Notes.....	171
V. THE IMPORTANCE OF THIS STUDY FOR RESOURCE MANAGEMENT AND POLICY.....	173
Introduction.....	173
Critique of the Current Federal Watershed Analysis Process.....	175
“Ecosystem Health” is Ambiguously Defined	

	Page
in the Document.....	179
The Concept of Hierarchical Structure is Ambiguously Defined in the Document.....	186
Watershed Analysis is Assumed to Best be Conducted by a Team of Specialists.....	190
This Method has a Tendency to Minimize the Role of Skills in the Process.....	193
The Knowles Creek Watershed Assessment.....	200
The Physical Setting of the Knowles Creek Basin.....	201
The Conceptual Model.....	202
The Idealized Functional View of the Knowles Creek Basin.....	206
The Historical View of the Knowles Creek Basin.....	211
The 1990's Picture of the Knowles Creek Watershed.....	215
Comments on the Knowles Creek Analysis.....	218
Summary.....	222
Notes.....	225
 BIBLIOGRAPHY.....	 230

CHAPTER I

INTRODUCTION: CAN WE DIAGNOSE THE HEALTH OF ECOSYSTEMS?

Introduction

Recently, the question, "Can we diagnose the health of ecosystems?" has become one of the most hotly-debated topics in conservation biology¹ and some areas of ecology. The proponents of the concept of ecosystem health believe: 1) that the concept of health can meaningfully be applied to ecosystems and 2) that we can assess the health of ecosystems.² They often claim that we can extensively draw from an analogy with medicine. Like medical doctors or veterinarians who diagnose the health of their patients (human beings or animals), ecologists diagnose the health of their patients (ecosystems). The proponents of "ecosystem health" argue that we need to look to medicine, its history, and its methodologies for guidance.

Many opponents of "ecosystem health" doubt that the concept of health can be meaningfully applied to ecosystems. Therefore, they believe any claim to an analogy between "ecosystem health" and

medicine is spurious. Other opponents believe that while "health" can be meaningfully applied to ecosystems, ecosystems are not like human beings or animals. Therefore, they are leery of any analogy between medicine and ecosystem health. They see a parallel fraught with difficulties.

I believe the momentum of the debate has now shifted in favor of those who oppose the idea of ecosystem health. Although opponents of ecosystem health generally are represented by a number of widely different philosophical commitments, I have found that most argue that a paradigm shift has occurred in biology and with the new paradigm, ecosystem health has become problematic. For example, Botkin provides a cogent description of this paradigm shift.³ Earlier in the century, ecology was dominated by the organismic theory of biological communities. According to this theory, communities are structurally and functionally like organisms. Communities have fixed identities. They have a final stage, the climax vegetative state. Therefore, biological communities are normative like organisms. Since communities have a normative condition, then we can also apply the normative condition of health to communities. According to Ehrenfeld, "If the organismic theory of

biological communities was still dominant, i.e. if the idea that communities have a normative equilibrium position were still widely accepted, then the idea of ecological health would pose few problems..."⁴ However, in the 1970's ecologists recognized that disturbances such as fire played a dominant role in shaping the character of ecosystems. As disturbances of ecosystems moved to the forefront of ecological research, ecologists came to see ecosystems as constantly in flux with no normative standard condition. If there is no longer a normative condition that ecosystems are moving toward, then ecosystem health becomes problematic. This paradigm shift from the organismic theory to the disturbance theory of ecosystems has made ecosystem health problematic. As a result of this paradigm shift, some ecologists recommend that ecosystem health be abandoned.⁵

I am one of the proponents of "ecosystem health".⁶ I partially agree with Botkin and Ehrenfeld. It is true that many, if not most ecologists, have abandoned the organismic theory for the disturbance theory of ecosystems. That paradigm shift does make "ecosystem health" incoherent. However, I am not convinced that the evidence supports a rejection of a normative view of biological communities or

ecosystems. It is not that I do not acknowledge the existence and role of disturbances in ecology. Rather, I am not convinced that the radical shift to a non-normative disturbance theory is necessary to adequately capture the nature of change in ecosystems over time. Those ecologists who have made the shift to the disturbance theory of ecosystems have not taken seriously the implications of such a theory. They have given up too much.

It is conceivably true that ecosystems should be viewed as non-normative, and that "health" may not adequately be applied to them. However, a careful examination ought to be made prior to rejecting a normative viewpoint. Such a test will be proposed in this study, and it will show that ecosystem health is a coherent concept in conservation biology. The opponents of "ecosystem health" argue that the evidence is overwhelmingly against the position that ecosystem health is a coherent concept. Their methodology is faulty. It a priori rules out the possibility that ecosystem health is coherent. Their methodological problem is the result of prior philosophical commitments. It is their philosophical commitments that have made "ecosystem health" problematic prior to any examination.

While this debate is being largely waged within cocktail conversations and conservation journals, its implications go much farther. This debate is about what paradigm will be used to try to manage natural resources. For example, here in the Pacific Northwest, President Clinton's proposed strategy for managing federal lands (Option 9) centers on assessing watersheds. To try and break the gridlock in the Pacific Northwest a team of scientists called "the Gang of Four" outlined a series of options and assessed the consequences of each.⁷ Option 9 was the one chosen by the President. Out of this option has come the Aquatic Conservation Strategy which is comprised of four components. One of them, watershed assessment, is germane to this study.⁸

The work of "the Gang of Four" brought risk assessment or assessing watershed health to center stage in resource management. Conservation biology is now engaged in a battle over how public lands will be managed.⁹ What will watershed assessment look like? Many questions are being actively debated, including: What paradigm will be followed to assess the risks to watersheds? Can we assess the health of watersheds and assign risks to features of watersheds? Or is this far beyond our capabilities? How will the U.S.

Congress and the Executive branch of the national government prioritize ecological risks? What authorizing language, which implicitly or explicitly will include a watershed risk assessment paradigm, will they establish as law? Will "health" be a foundational component of such a paradigm? The answer to these questions and the decisions which will follow have the potential of dramatically affecting our lives here in the Pacific Northwest.

Much of the debate about watershed assessment and risk assessment is about "ecosystem health" and if it can be measured. "Ecosystem or watershed health" is central to my own work. In 1992, I was Project Director of the Oregon Rivers Council's National River Policy Development Project. This project brought together 17 of the nation's leading stream ecologists to write a white paper for the U.S. Congress outlining what is ecologically necessary to protect and restore the nation's rivers.¹⁰ This work was subsequently incorporated into a book.¹¹ This white paper and subsequent book were utilized by the Clinton Administration in drafting their conservation strategy for the Pacific Northwest. "Ecosystem health" is a foundational component of this work. It is also a major component in my field work. I have assessed the health of

ecosystems and conducted evaluations of stream and watershed restoration projects mostly for the U.S. Forest Service for the past 13 years¹² In 1991, I was one of the cooperators in the Knowles Creek Project. The aim of this project was implementation of a new paradigm of watershed restoration in the Pacific Northwest. This project also was built on the concept of ecosystem health.

This debate about “ecosystem health” also raises questions about the relationship of conservation biology, ecology, and medicine to science. Are any of these endeavors science, or are they applied disciplines somehow related to science? These questions raise more questions within the philosophy of science and the philosophy of biology, such as what is science? What is the relationship of biology to chemistry and physics?

The “ecosystem health” debate is not a theoretical discussion isolated within a discipline of science. It is not a debate about which of two rival theories best accounts for the facts in a particular science. It is a debate about the unity of the biological and ecological sciences. It is about ecology's and biology's common principles, acknowledged facts, and accepted methodologies. It is a broad, complex issue deeply entwined with the foundational philosophical

issues in the philosophy of science and the philosophies of ecology and biology. The debate about "ecosystem health" is underlain by the different philosophical commitments of the proponents and opponents within the discussion.

The Scope of This Study

The discussion can be broken down into two major issues: 1) Can "health" be meaningfully applied to ecosystems? 2) If it can, then how do we diagnose the health of ecosystems? This study will develop the concept of health in ecology and biology as a step toward determining if it can be applied to ecosystems. I will explicate what is logically necessary for ecosystem health to be a coherent concept in conservation biology.

A major portion of this study will show that much of the debate about "health" in conservation biology stems from different philosophical commitments of the major participants in the debate, and that these previous commitments cause the concept of ecosystem health to become incoherent. This study will also examine the conceptual frameworks of two recent works which utilize the concept of ecosystem health. I will show that both of these frameworks result in "ecosystem health" becoming incoherent.

I will also examine some of the major objections that have been raised against "ecosystem health". Not all opponents would agree on all the specific objections. The objectors to the concept of ecosystem health hold a number of widely divergent philosophical perspectives. Therefore, some of the objections raised against ecosystem health are not logically related with other objections. For the purposes of this study, I will not assume that the objections are related. I will examine each of them in isolation.

Some of the more substantial objections that have been raised against the concept of ecosystem health include the following:

1) Ecosystem health has never been clearly defined. Any use of the term is ambiguous. Therefore, before we can discuss and criticize the "health of ecosystems", we need to clarify it.¹³

Those who hold fast to this objection claim that without a clear definition of "ecosystem health", it is not clear what claims the proponents of ecosystem health are making. Some opponents take this line of reasoning further and say that any reference to the concept of ecosystem health is problematic.¹⁴ Still others believe the concept of health can be applied in a limited way to ecosystems.

2) The level of biological organization called the ecosystem does not exist.¹⁶ Only a random assemblage of organisms exists, which is loosely organized into communities. Therefore, "ecosystem" is an empty concept.

These objectors argue that the level of organization called the ecosystem does not exist. Some claim that loosely organized communities are the highest biological level of organization. Others argue that there are no levels of biological or ecological organization above the level of individuals. They further argue that ecological features, which appear to be the result of higher ecological levels of organization, are simply the result of interactions among individuals.

17

3) The level of biological organization called the ecosystem exists, but not as an organism.

The strong form of this objection is that ecosystems are not organisms. Therefore, "health" does not apply to ecosystems. The concept of health applies only to organisms like human beings or trees. It does not apply to ecosystems. A milder form of the argument is also made. Ecosystems are organisms, but they are not organisms like human beings. The concept of health does not apply

well to ecosystems. Therefore, the analogy between ecosystem health and human health is of limited use.

4) The concept of health, in itself, is problematic in science.

These opponents argue that "health" is a normative concept which recognizes certain states as desirable and others as less desirable. Therefore, the concept of health goes beyond description and fact. These objectors also raise the issue of what scientific criteria can be used to establish the standard of health. From their perspective, "health" is not a scientific term; it is a term fraught with problems.

5) The concept of ecosystem health is dangerously related to teleology and contradicts current evolutionary theory. Therefore, the concept of ecosystem health is problematic.

Those who bring this objection argue that Darwin's theory of evolution is the foundation of biological science, including ecology. Darwin established materialistic principles of analysis and natural mechanisms that would enable him to escape vitalistic or teleological notions, such as the perfect adaptation of an organism to its environment.¹⁸ Any theory which contradicts Darwin's theory ought to be rejected. "Ecosystem health" is teleological and contrary to

Darwinian evolution; therefore, "ecosystem health" should be rejected in ecology.

I will address each of these objections interactively by placing this work in the debate on "ecosystem health". Interacting with each of these objections will help clarify the perspective of this study. I will also examine in detail the analogy with medicine.

In this study, I will utilize some key concepts from Michael Polanyi's perspective to develop and clarify the concept of health in biology and ecology. While Michael Polanyi did not directly address the issue of ecosystem health, a notion of it can be developed within his intriguing theories of biology and biological health. Polanyi claims his unique perspective is aimed at refuting many widely-held views within the philosophies of science and biology. My intention in addressing ecosystem health using key concepts from Michael Polanyi's view is not to provide a limited scholarly study of a radical philosopher. My intention is just the opposite. Michael Polanyi is the strongest spokesman against the dominant view of biology and philosophy of science in the later half of the 20th century.¹⁹ For example, Polanyi explicitly rejects the philosophy of science and philosophy of biology espoused by Francis Crick, one of the co-

discoverers of DNA ²⁰. It is Polanyi's explicit rejection of probably the dominant philosophy of biology and philosophy science that is of greatest interest for this study of ecosystem health. What is striking is Polanyi's claim that "health" becomes problematic if biology can be reduced to the laws of chemistry and physics. ²¹ Therefore, there is probably no quicker way into the core of the philosophical issues related to "ecosystem health" than through Polanyi's viewpoint.

Polanyi's starting point is that mechanists like Crick misunderstand the nature of machines. The result is that they believe that machines can be reduced to the laws of chemistry and physics. He claims that both Cartesian scientists ²² like Francis Crick and their modern objectors, like Barry Commoner, have missed a fundamental distinguishing characteristic of machines. Polanyi argues that machines can never be reduced to the laws of chemistry and physics. They are built by man, so they are not the spontaneous result of the laws of chemistry and physics. Machines are built with operational principles which harness the laws of chemistry and physics. These operational principles function as higher level control to accomplish the purpose of the machine. They serve as the rules of rightness for the machine and class membership is determined by

the operational principles. Machines can break down and fail. When that occurs, the parts are no longer functioning to accomplish the purpose of the machine. The laws of chemistry and physics are still operating the same. What is different when the machine fails is that the higher level of control has failed. Polanyi claims that the attempt to reduce the workings of machines to the laws of chemistry and physics neglects the operating principles which serve as the rules of rightness for the machine. The result is there is no standard of rightness. Therefore, there is no way to determine what the machine is or if it is properly functioning.

Polanyi argues that holistic inanimate systems like flames and thunderstorms lack the dual level of control found in the structure of machines. These systems are logically different from machines. There are no rules of rightness. It makes no sense to consider a thunderstorm failing. There is no rule of rightness for thunderstorms. Their function is merely the result of the laws of chemistry and physics. Polanyi argues that biological organisms are more like machines than thunderstorms. They are systems which can fail. This is Polanyi's distinction that is of fundamental import

for this study. The question is, "Are ecosystems like machines and organisms or are they like flames or thunderstorms?"

Several key concepts from Michael Polanyi's theory of personal knowledge will also be utilized in the second section of this study. Polanyi claims that there are two types of knowledge: explicit and tacit.²³ Explicit knowledge refers to maps, formulas, and texts, while tacit knowledge is inarticulate knowledge. Polanyi's perspective is aimed in part at showing how much tacit knowledge exists in science. According to Polanyi, tacit knowledge is a major component of all knowledge. A paradigm example for him is the role of tacit knowledge in medicine. In order to be a good diagnostician, one must spend considerable time learning the skill of diagnosis from an expert. Much of this skill is tacitly learned while apprenticing with a master; it can only be learned by doing. This discussion will play a central role in examining how we would diagnose the health of ecosystems.

The Structure of This Study

Chapter II will develop and clarify Polanyi's perspective that biology can never be reduced to the laws of chemistry and physics

(matter in motion). Polanyi argues that if one views biology as ultimately reducible to the inanimate laws of chemistry and physics, then normalcy and health become problematic. It is this debate about reductionism and related issues that underpins the discussion about ecosystem health. This chapter will also develop Polanyi's central view within biology which is relevant to our discussion. Most of this discussion will come from Part Four of *Personal Knowledge* and several of his later journal articles.²⁴

Chapter III will address the question, "Can 'ecosystem health' be a coherent concept in ecology?" Section 1 is a brief historical overview of the development of the ecosystems concept in ecology. Since World War II, the investigation of ecosystems from the perspective of general systems theory has been a major theme. Section 2 is a critique of two current influential books from a general systems perspective. I will show that the general systems perspective in common use in ecology and conservation biology has the unfortunate result of making the concept of ecosystem health incoherent. Section 3 examines whether "ecosystem health" is coherent from the framework which was developed in chapter II. Drainage basin ecosystems from 20-200 mi² are the specific ecosystems chosen for examination. A crucial portion of the discussion will center around the concept of an organism and whether an ecosystem is an organism. I will investigate whether ecosystems are like machines and organisms or whether they function more like flames and thunderstorms. The conclusion of this

study is that ecosystems are more like machines and organisms than they are like flames and thunderstorms.

Chapter IV addresses the question, "How do we diagnose the health of drainage basin ecosystems?" Having shown in Chapter III that the concept of ecosystem health is coherent, this chapter develops the methodology for diagnosing the health of ecosystems. This chapter utilizes Michael Polanyi's concept of tacit knowledge and the skill that a diagnostician has in diagnosing the health of patients.

Chapter V discusses the importance of this study for policy and natural resource management. It begins with a critique of the current watershed analysis procedure under the Aquatic Conservation Strategy in light of the findings of this study. Second, it moves to a discussion of the Knowles Creek basin analysis which I did prior to designing a restoration strategy for the basin. The Knowles Creek watershed analysis focuses on several aspects not found in the Federal process. For instance, watershed "digestion" is not, and has not been, identified in any Federal watershed analysis. This chapter clarifies why such a central concept is not identified in the Federal documents.

Notes

¹ Conservation biology is a field closely related to ecology. The task of conservation biology is to provide intellectual and technical tools that will anticipate, prevent, minimize, and/or repair ecological damage. Conservation biology arose as a response of the scientific community to a wave of global environmental change that is threatening to extinguish a very large fraction of the world's species of organisms (bio-diversity).

²e.g. T. Kuchenberg, 'Measuring the Health of Ecosystems', *Environment* 27-2(1985), pp. 32-37, pp. 32ff. Also T.C. Dewberry, 'Protecting the Biodiversity of Riverine and Riparian Ecosystems: The National River Public Land Policy Development Project', *Transactions of the 57th North American Wildlife and Natural Resources Conference* (1992). Special Session #6, pp. 424-432, p. 426.

³ D.B. Botkin, *Discordant Harmonies: A New Ecology for the Twenty-first Century*. (Oxford University Press, New York, 1990) (This is a major theme throughout the book). Also see D. Ehrenfeld, 'Ecosystem Health and Ecological Theories', In R. Costanza, B.G. Norton, and B.D. Haskell (editors), *Ecosystem Health: New Goals for Environmental Management*, (Island Press, Washington, D.C., 1992), pp. 135-143, p. 137.

⁴Ehrenfeld, *Ibid.*, p. 137.

⁵e.g. D.J. Rapport, H.A. Regier, T.C. Hutchinson, 'Ecosystem Behavior under Stress', *American Naturalist* 125-5 (1985), pp. 617-640, p. 619; D.W. Schindler, 1987. 'Detecting Ecosystem Responses to Anthropogenic Stress', *Canadian Journal of Fisheries and Aquatic Sciences* 44(1987), pp. 6-25, p.7; R.T. Lackey, 'Ecological Risk Assessment', *Fisheries* 19-9(1994), pp. 14-18, p. 16.

⁶e.g. T.C. Dewberry, 'Protecting the Biodiversity of Riverine and Riparian Ecosystems: The National River Public Land Policy Development Project', p. 426.

⁷ R.T. Lackey, 'Ecological Risk Assessment', pp. 14-15.

⁸ The Aquatic Conservation Strategy came out of the Forest Ecosystem Management Assessment Team (FEMAT). Forest Ecosystem Management: An Ecological, Economic, and Social Assessment Report of the Forest Ecosystem Management Assessment Team. (U.S. Gov. Printing Office: 1993-793-071, Washington, D.C., 1993)

⁹ A good description of current thinking on watershed assessment is: D.R. Montgomery, G.E. Grant, and K. Sullivan, 'Watershed Analysis as a Framework for Implementing Ecosystem Management', *Water Resources Bulletin* 1995 (in press).

¹⁰T.C.Dewberry, Protecting the Biodiversity of Riverine and Riparian Ecosystems: The National River Public Land Policy Development Project, p 424-32.

¹¹B. Doppelt, M. Scurlock, C. Frissell, J. Karr, *Entering the Watershed* (Island Press, Washington, D.C., 1993) .

¹² e.g.T.C. Dewberry, *Historical Reconstruction of Watersheds from Cape Perpetua to Heceta Head on the Mid-Oregon Coast* ,(U.S. Forest Service, Siuslaw National Forest, Corvallis, 1994), Final Report, Project 43-04To-4-8616

¹³ R. Costanza, et al., *Ecosystem Health*, p. 3.

¹⁴ R.T. Lackey, R.T. 1994. 'Ecological Risk Assessment.' *Fisheries* 19-9 (1994), pp. 14-18, p. 16.

¹⁵Costanza et al., *Ecosystem Health*, p. 3.

¹⁶Throughout the history of biology a number of levels of organization have been recognized in biology. The current list would include: genetic system, cell system, organ system, organism system, population system, community system, ecosystem. See E. Odum, *Fundamentals of Ecology*, (Saunders, Philadelphia, PA, Saunders Co, 1971), p. 5.

¹⁷These objectors stress the role of individual plants in invading and growing along environmental gradients. They deny the existence of higher levels of organization. They are the intellectual heirs of Henry Gleason.

¹⁸T. Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology* (Reidel, Boston, 1982), p. 4.

¹⁹ L. Gatlin, *Information Theory and the Living System* (Columbia University Press, New York, 1972). pp. 16-17.

²⁰ M. Polanyi, 'Life Transcending Physics and Chemistry', *Chemical and Engineering News* (Aug 21, 1967), pp. 54-64. And M. Polanyi, 'Life's Irreducible Structure', *Science* 160 (1968), pp. 1308-1313.

²¹ M. Polanyi, *Personal Knowledge*, (The University of Chicago Press, Chicago, 1958), p. 345.

²²For present purposes Cartesian scientists will be defined as those scientists who accept the following position: 1) Explanation of all objects in nature will ultimately lie at the atomic level. All objects can be reduced to atoms in motion. 2) A continuous theory of nature is assumed, i.e. all objects in nature differ by degree from each other.

²³M. Polanyi, *The Study of Man*, (The University of Chicago Press, Chicago, 1958), p. 12.

²⁴The main two journal articles are M. Polanyi, 'Life Transcending Physics and Chemistry', and M. Polanyi, 'Life's Irreducible Structure'.

CHAPTER II

POLANYI'S REJECTION THAT BIOLOGY CAN BE REDUCED TO THE LAWS OF CHEMISTRY AND PHYSICS, AND ITS IMPLICATION FOR THE CONCEPT OF HEALTH.

Introduction

Reductionism in biology is one of the major issues currently debated in the philosophy of biology.¹ A new biological era was ushered in with the discovery of the DNA (deoxyribonucleic acid) molecule, and Francis Crick's claim that biology was reducible to a molecular level. With the discovery of DNA, it was considered confirmed that bigger entities, such as organisms, are no more than the sum of all smaller parts. The whole is composed of nothing but parts. Thus, there are not "parts plus something else".² This older view, parts plus something else, was purged from biology once and for all. An organism is nothing but the molecules of which it is made; there is no life force or organizing principle which is necessary to make a living being.

This type of reductionism has been called ontological reductionism.³ According to Ruse in his book on the philosophy of biology, "no one wants to deny this type of reduction, or at least, no one that need trouble us."⁴ Thus, from Ruse's perspective, the ontological reductionism of biology to molecular biology is accepted by virtually everyone.

However, Michael Polanyi rejects this philosophical commitment. He believes that ontological reductionism has been a major destructive force within modern biology. Polanyi argues that one of the major problems with this reduction is that it causes the concept of health to become problematic in biology. The focus here is understanding why Polanyi believes that ontological reductionism results in making the concept of health incoherent. The aim of this chapter is not to present a scholarly discussion of scientific reductionism nor of Michael Polanyi's work. Both those endeavors are outside this study. The reason for examining Polanyi's criticism of both Francis Crick and his opponent Barry Commoner in their debate about reductionism in biology is simply to move as quickly as possible to the heart of the underlying philosophical debate about "health".⁵ It is true that Polanyi's criticism of Crick and Commoner

has received little attention, and it warrants attention in its own right. However, that is beyond this study.

The central theme of this chapter is Michael Polanyi's rejection of both Francis Crick's and Barry Commoner's positions. Polanyi believes that virtually everyone, including Francis Crick and Barry Commoner, have missed a fundamental distinguishing characteristic of machines. In other words, the metaphor of the machine has been misunderstood and misused. Polanyi argues that machines can never be reduced to the laws of chemistry and physics. Machines are built with operational principles which harness the laws of chemistry and physics. These operational principles function as higher level control to accomplish the purpose of the machine. They serve as the rules of rightness for the machine, and class membership is determined by the operational principles. Machines can break down and fail. When, that occurs, the parts are no longer functioning to accomplish the purpose of the machine. The laws of chemistry and physics are still operating the same. What is different when the machine fails is that the higher level of control has failed. Polanyi claims that the attempt to reduce the workings of machines to the laws of chemistry and physics neglects the operational principles. The result is that there is

no standard of rightness. Therefore, there is no way to determine what the machine is or if it is functioning properly.

Polanyi argues that holistic inanimate systems, such as flames and thunderstorms, lack the dual controls found in machines. These systems are logically different from machines; there are no rules of rightness. For example, it makes no sense to consider a thunderstorm failing. These systems are reducible to the inanimate laws of chemistry and physics. Their function is merely the result of the laws of chemistry and physics. This distinction between machines and holistic inanimate systems is of fundamental import for this study. Polanyi argues that Crick and Commoner both have misunderstood the nature of machines. They both believe that machines are reducible to chemistry and physics. Polanyi believes that machines are not reducible, and neither are biological organisms, which have operational principles serving as the rules of rightness (health) for each class of organism in an analogous fashion to machines. These are systems which can fail. For example, death is failure of the operational principle harnessing the laws of chemistry and physics. This chapter illustrates that failure to recognize the existence of operational principles causes "health" to become

problematic, because health is based on the operational principles which serve as the rules of rightness for the object in question.

In this chapter, the salient features of Francis Crick's and Barry Commoner's views are sketched. Then their positions are critiqued from Michael Polanyi's perspective. The discussion then moves to the implications of Polanyi's critique for the study of "ecosystem health". A crucial element of this chapter will be to uncover the theories of nature which are presumed by the various players. A major facet of this study is to show the connection between theories of nature (assumed or otherwise) and problems with the concept of ecosystem health.

Polanyi's Rejection of the Views of Francis Crick and Barry Commoner

In the Cartesian mechanistic view of science, the fundamental philosophical assumption is that all phenomena can ultimately be reduced to matter in motion. This is the ideal mechanical view which came to us through Galileo and Gassendi. Therefore, the aim of science is to explain all kinds of objects and experiences in terms of atomic data. In this mechanistic view, all sciences are ultimately reducible to mechanics. In his book, Of Molecules and Men, Francis

Crick states this explicitly, saying "the ultimate aim of the modern movement in biology is in fact to explain *all* biology in terms of chemistry and physics."⁶ The discovery of the genetic function of DNA and its ability to self-duplicate are viewed by Francis Crick as proving that living things can be interpreted, at least in principle, by the laws of inanimate physics and chemistry. Crick finds ridiculous Polanyi's claim that an enzyme cannot be totally described in terms of physics and chemistry.⁷ From Francis Crick's perspective, Polanyi is a vitalist who believes that machines and biological organisms cannot be reduced to the inanimate laws of chemistry and physics. To Crick, such a claim is nonsense. It contradicts the heart of the Cartesian project.

This Cartesian view of science is underpinned by a continuous theory of nature, i.e. phenomena in the world differ from each other by degree.⁸ All phenomena differ by differences in atoms and their motion. Thus, in the mechanical view, biology is a science to the extent that life is machine-like and can be reduced to the laws of physics and chemistry. The explicit premise is that all life can ultimately be accounted for by the laws of inanimate nature.

Rejection of the Premise that Life is Reducible to the Inanimate Laws of Chemistry and Physics.

The traditional rejection of the mechanical view agrees that biology is mechanistic to the extent which it can be reduced to the laws of inanimate physics and chemistry; however, the traditional rejection has been that living things are not wholly machine-like. For example, in the late 17th century, Cudsworth, a Neo-Platonist, and John Ray, a Christian naturalist, both opposed the mechanistic view by arguing that organisms were not machines. Driesch and his supporters argued 200 years later that life transcends the laws of physics and chemistry. Specifically, they argued that the regenerative powers of the sea urchin embryo were not explicable by a machine-like structure.⁹

Recently, Barry Commoner argued against Francis Crick, using a similar line of reasoning. Commoner criticized Crick's view by arguing that the self-duplication of DNA is not proven. For Commoner, if DNA is self-replicating, then living things are mechanisms and can be represented in terms of physics and chemistry¹⁰. DNA self-duplication would be a mechanical process. However, Commoner argued that it is not yet proven that DNA self-

duplication occurs. If it did, then Commoner would agree with Crick that biology could be reduced to chemistry and physics.

Michael Polanyi points out that there is a contradiction in Barry Commoner's refutations of Crick's mechanistic model. This contradiction is also found in virtually all of the traditional objections to the mechanistic view. The opponents of the mechanistic view hold two premises: ¹¹

1. When an object, composed of parts, has features which cannot be observed in the isolated parts, these comprehensive features are not explicable by the laws governing the parts. Therefore, in these cases there is no reducibility. Hence, when the parts are governed by the laws of chemistry and physics, their comprehensive features are not considered to be reducible to chemistry and physics.

2. The explanation of living functions in terms of mechanical models amounts to explaining them in terms of physics and chemistry.

These two premises are incompatible because machines, or living functions operating mechanically, are entities where the characteristic features are absent in their separate parts. Therefore,

since all machines or living functions have these comprehensive features, then all machines and living functions are not reducible to chemistry and physics. This is premise #1. But premise #2 claims that explaining living functions mechanistically amounts to explaining them in terms of inanimate physics and chemistry. Therefore, these two premises are contradictory.¹²

Polanyi rejects both premises used by Commoner to reject the mechanist's argument. He rejects Commoner's explication of irreducibility (Premise #1). Polanyi gives several examples to show that this definition of irreducibility is inadequate¹³. It does not adequately capture the nature of the key feature at issue. Consider that the sun is a sphere. Its separate parts are not spheres. Nor does the law of gravitation speak of spheres. The mutual gravitational interactions cause the parts to form a sphere. Therefore, in this case, the fact that the sun is a sphere is a feature that is not found in the parts. However, the laws of inanimate physics do explain the comprehensive feature.

Second, consider crystals. They can occur in 230 different patterns which are not observed in their parts. However, these patterns can be derived from the interaction of the component parts

by a natural law.¹⁴ Therefore, we have a case where we observe a comprehensive feature, the pattern of the crystal, which is not found in the parts, yet the patterns can be explained by physical and chemical laws.

Finally, consider thunderstorms and fires. They are holistic systems which have many comprehensive features which are not found in their parts. Yet their comprehensive features and their structure and function are explainable by the laws of physics¹⁵. Polanyi argues that the natural world is full of examples where the comprehensive features of a system are not observed in the parts, but these features can be computed from the laws of physics. In fact, we are unable to find holistic systems in inanimate nature that are not reducible to chemistry and physics. Therefore, Polanyi rejects Commoner's criterium of irreducibility. These inanimate holistic systems do not point to the existence of irreducibility in biology. In fact, they do the opposite. They suggest that the biological holistic systems, organisms, may act as these inanimate systems do.

Polanyi also rejects Commoner's second premise by claiming that no machine or machine-like feature of an organism can be

wholly explained in terms of physics and chemistry. While holistic systems in the inanimate world are reducible to physics and chemistry, machines are not. As human artifacts, machines are not reducible, since they have comprehensive features that are not due to the spontaneous integration of physical and chemical forces ¹⁶. They are created by man. Machines are shaped and designed to accomplish a specific purpose. They achieve their purpose by the interaction of their parts working in accordance with distinctive operational principles. An operational principle specifies how the characteristic parts (or organs) fulfill their special function and work together to achieve the purpose of the machine. It describes how each organ acts on other organs within the context of the whole. Polanyi wants this concept of operational principle understood in its widest sense. Like an inventor who wants to obtain the widest possible patent for a machine, he always tries to cover all possible embodiments of the operational principle and avoids as much as possible the mention of the particular parts of any actual machine, unless they are absolutely essential for the operation of the machine.

Machines, according to Polanyi, are classified by their operational principles, and these are outside of physics and chemistry. The operational principles function as the ideal. They describe the machine in good working order; they are the standard of perfection. Any clock, watch, or typewriter can be judged by this standard. It also helps one evaluate whether a machine is operating correctly or not. But it cannot say anything else about failure. The operational principles of machines are the rules of rightness for the particular class of machines. They determine class membership. From the perspective of physics and chemistry, which ignore operating principles, a group of objects which represented a class of machines would appear as a chaotic ensemble. Since physics and chemistry do not consider operational principles, they are blind to the success or failure of machines.

Holistic systems in the inanimate world do not have rules of rightness. They do not have operational principles like machines do. Therefore, it makes no sense to talk about failure. For instance, consider a thunderstorm. It may build and then dissipate. At no point do we think the thunderstorm fails. That would be a logical mistake.

Machines are subject to two sets of controls. One set includes the laws of chemistry and physics which operate on the parts. The other is the operational principle which harnesses the laws of chemistry and physics to accomplish the purpose of the machine. This dual set of controls does not interfere with the laws of chemistry and physics. The operational principle is working on a higher level than the laws of chemistry and physics. It is not interfering with the laws; it is working in addition to the laws.

It is crucial that we see that machines are logically different from holistic systems in the inanimate world. Consider the following case. We have an unknown object sitting on a table. The initial question is, "Is it a machine?" We could have a team of physicists and chemists examine and write a complete description of the object, but this would not tell us if the object is a machine. We could also have the team of physicists and chemists write a description of all future states of the object but they would still be unable to determine if it were a machine or not. You cannot put the question to the object, "Is it a machine?" unless you already know how machines work. Only if you know how clocks, typewriters, computers, telephones, and cameras are constructed and how they

function can you inquire whether what you have in front of you is a machine. An investigation of the chemical and physical aspects of an object does not help.

The implied question, "Does the object serve any purpose, and if so, what purpose, and how does it achieve it?" can only be answered by testing the object practically against known or conceivable machines. For example, we have Dr. Spock's tricorder before us,¹⁸ but we do not know what it is. Let a team of physicists and chemists inspect the machine, and let them use only their knowledge of physics and chemistry to try to determine if it is a machine. They could only describe the object and all its possible future states, but they could not test the machine against known machines because that is outside of the sciences of physics and chemistry. They would never be able to determine that it is a machine.

This result is foundational to Polanyi's position. Let me make the argument again from a slightly different perspective. A complete physical and chemical description of my watch (including the changes that it goes through) would not tell us what the object is. On the other hand, if we know what watches are, we would recognize an

object as a watch by the fact that its hands sweep around the face and we can read the hours and minutes of the day. We know watches and can only describe them by phrases like "telling time", "hands", "face", which are all incapable of being expressed by the variables of physics (length, mass, time, etc.). The impossibility is a logical kind. Operational principles are logically outside the laws of chemistry and physics.

A physical description of my watch might make it possible to identify this particular watch as an object. With the description, I might be able to pick it out from a number of other objects, but I would be unable to identify it as a member of the class "watch". For example, say I now place a digital watch on the table and ask the team of physicists and chemists what the object is. The physical and chemical description of this watch would be very different from the physical and chemical description of my watch (a traditional watch with hands). The point is the physicists and chemists would not be able to identify the objects as members of the class, "watch", using only the laws of physics and chemistry. The reason that the physical and chemical descriptions of the watches cannot be used to identify the class "watch" is crucial. As the physicists and chemists take the

watch to pieces and examine the separate parts carefully, each in their turn, they will never come across the principles by which a watch keeps time. Physics and chemistry are the study of inanimate nature. They can describe and analyze objects in utmost detail, but they cannot address the question whether the object is a machine, and if it is, what its purpose is. Physics and chemistry do not deal with the purposes of machines. The science of engineering deals at length with those issues. The class of objects like watches is defined by the purpose and how the parts are organized to accomplish the purpose. Again, remember that Polanyi wants the concept of operational principle defined in the widest sense, like an inventor trying to obtain the widest possible patent by trying to cover all possible embodiments of parts to accomplish the purpose. The operational principle for watches would include such things as telling time and being small enough to wear on one's wrist. These aspects are outside of the sciences of physics and chemistry. Therefore, the sciences of physics and chemistry can never tell us about machines as machines.

This formulation makes it clear that knowledge of physics and chemistry is ancillary to knowledge and understanding of the

operational principles. We must understand machines as machines first, and no amount of physical and chemical investigation can replace this. The true knowledge of a machine comes through understanding the operational principle of the machine, which spells out its purpose and the rational means for achieving it.

Polanyi's Rejection of Francis Crick's Position

Polanyi's rejection of Commoner's second premise is also Polanyi's rejection of Crick's position. As we saw earlier, the heart of the Cartesian position is the claim that objects can ultimately be reduced to the laws of inanimate chemistry and physics. Polanyi has argued convincingly that machines cannot be reduced to these laws, because they have two levels of controls.

In the Cartesian system, the ultimate explanation is found in the lower level of reality, according to Polanyi's scheme. The Cartesian system does not acknowledge the existence of dual controls in machines. The Cartesians believe that an explanation resides in the parts alone. It is now easy to see why Polanyi views Laplace as the logical end of Cartesian physics. The Laplacean mind, which if it could know the location of all atoms in the universe at one time, and

if all forces acting on the atoms were known to it, could determine all configurations at all times in the future. However, from Polanyi's perspective, this information could not be used to identify any objects. It could not identify a machine nor tell how it worked; it would ignore the existence of machines.

This result should not have been a surprise, for its conclusion can be seen from within Laplace's perspective. The starting point for the Laplacean mind is a knowledge of the location of all atoms in the universe. This initial starting point forms the frame of reference for the mechanical process, but these initial conditions are not determined by the laws of physics. If they were formed by the laws of physics, the Laplacean mind could have calculated them. Therefore, something more than the laws of physics is necessary. Physicists and chemists call this initial frame of reference "the boundary conditions". These set the limits upon which the laws of physics and chemistry work. The laws of physics and chemistry are differential equations which determine a definite system only within a set of fixed conditions. Physics is dumb without the gift of boundary conditions forming the frame of reference.¹⁹

The laws of chemistry work the same way. We set up a set of initial conditions, for example, so much of A and B at a given temperature, etc. These conditions are not determined by the laws of chemistry; they are set by the chemist. The boundary conditions are called "fixed conditions" in chemistry.

Polanyi sees the operational principle (the structure and function of the machine) as a type of boundary condition harnessing the laws of physics and chemistry. The operational principle is the higher level of control which harnesses the lower level, the laws of chemistry and physics. However, the focus of boundary conditions is different in engineering than it is in chemistry. In chemistry, the focus is on the chemical processes which are operating under the boundary conditions. In chemistry, the fixed conditions are necessary, but they are not highly significant. Their change does not greatly alter the outcome. When we do a chemistry experiment, we are interested in the reactions in the test tube, not in the test tube itself. Nature abounds with this type of boundary condition, such as when we are cooking soup on the stove. We are interested in the soup, not in the pan. This type of boundary condition is found in inanimate systems and was established by the history of the

universe. It is found in geology, geography, and astronomy, but it does not form systems of dual control.²⁰ In engineering, our focus is usually on the boundary condition itself. We are only ancillary interested in the lower level chemical and physical processes. There are many examples of this type of boundary conditions in everyday experience. When a sculptor shapes a stone or a painter composes a painting, we are not primarily interested in the chemistry and physics of the stone or the chemical and physical analysis of the paints and paper. These examples illustrate the fundamental distinctions between these two types of boundary conditions.

Polanyi is not claiming that the differences between these two types of boundary conditions is just one of perspective. He is claiming that these two boundary conditions are fundamentally different. Consider a tribe that is unfamiliar with books. They have no written language. A person from the tribe comes upon a book. The person has no idea what it is. Polanyi is arguing that no amount of analysis of the ink and paper will help determine what the thing is. Communication is something that can succeed or fail and it occurs at a higher level than chemistry and physics. No greater understanding of the chemistry and physics involved will help

without an understanding of how the parts function within the context of the whole.

The recognition that there are two kinds of boundary conditions enables us to see the logical difference between inanimate holistic systems and machines. The boundary conditions for machines are operational principles which function as the rules of rightness for the machines. Machines can succeed or fail, and the grounds for evaluating their success or failure are the operational principles which establish the standard for the machine. The boundary conditions of inanimate holistic systems which are established by the history of the universe are found in the domains of geology, geography, and astronomy. They do not form systems with dual control.²¹ Here, the boundary conditions do not function as rules of rightness for the inanimate systems. Therefore, thunderstorms and fires do not succeed or fail. From Polanyi's perspective, it is a logical error to question whether thunderstorms or fires can succeed or fail. There is a logical difference between the two types of boundary conditions. It is not just two different ways of viewing a system. Polanyi's point is that the logical structure of the boundary conditions of a machine is ontologically different from

the boundary conditions of a thunderstorm. A machine is not reducible to the laws of chemistry and physics, while a thunderstorm is reducible.

Boundary Conditions in Living Organisms and Their Implications for the Concept of Health

Polanyi believes that, to a point, living beings are analogous to machines. Living beings have boundary conditions more like machines than holistic inanimate systems. Boundary conditions of holistic inanimate objects are established by the history of the universe. These inanimate systems do not form systems with dual control like machines.²²

Boundary conditions forming a machine have two interrelated aspects: they consist in a distinctive structure sustaining a purposive operation, and the various functions of a machine are similarly sustained by its structure (morphology). Biological principles control the boundary conditions within which the laws of physics and chemistry carry on the business of life. These systems do appear to have boundary conditions which form dual controls. Take the organs of higher animals, organs which enable breathing, digestion, secretion, and thermal regulation. Think of their anatomy and of the

way they operate to accomplish their functions. None of these conceptions can be defined solely in terms of physics and chemistry. The essence of these organs is to provide their respective functions within the comprehensive whole of the animal, much like the essence of a machine is to serve a purpose created by the designer. To further see the parallel between machines and living things, consider that in both, functional disorders can interfere with normal achievements. These are systems which can succeed or fail in ways that holistic inanimate systems can not. In contrast to thunderstorms, it makes sense to talk about the failure of a machine or biological organism.

In the case of machines, we saw that it was the science of engineering which investigates the purpose and rational means of achieving the desired ends. In the case of living beings, it is the science of physiology which seeks to interpret the organism as a complex network of mechanisms. It is physiology which investigates the structure of the parts and how they accomplish their functions within the context of the whole organism.

However, the fact that machines are human artifacts while living beings are not, weakens the analogy. In machines, the

operational principles are imposed on the parts by the engineer. Polanyi is not claiming a tight parallel for this aspect of the analogy. But Polanyi argues that careful attention to the development of the organism (morphogenesis) suggests that the linkages here between machines and living beings may be closer than one might think.

In the case of machines, it is membership in a class of contrivances with its common operational principle which establishes the rules of rightness for the machine. The operational principle establishes the standard of achievement. Living beings seem also to work according to a dual system of control and have principles analogous to operational principles in machines. Organisms are appraised from a standard of merit appropriate for the specific organism. It is membership in a species which links members of a class of living beings. The structure of the organism serves as a boundary condition, harnessing the laws of physics and chemistry, and there are operational principles which specify how the organs interact and achieve the purpose of the organism. In this way, organisms have a standard of rightness by which they are appraised as being normal or abnormal, healthy or unhealthy. It is logically possible that they can succeed or fail.

However, living organisms have a more complex series of controls than machines. While machines have two levels of control, organisms can have many. At the lowest level are the vegetative functions, the basic functions of life. At a higher level are the controls of growth, reproduction, etc., but even these controls leave open further higher level controls such as behavior.

In addition, living beings have another characteristic type of dual control not found in machines. Many features of living beings are able to reorganize some of their operational principles almost indefinitely in the face of new circumstances. In animals, such as insects, there is an operational principle which describes how all the various parts of the insect are organized for the purpose of locomotion. If an insect loses a leg, it immediately reorganizes the operational principles to accommodate the loss of the leg and it can move with little or no perceptible loss of locomotion. It seems that the organism has at its disposal a large number of possible operating principles which can accomplish the purpose. Polanyi's term for this is "equipotential".²³

In many cases, there are so many possible configurations that they can not be due to the action of predetermined anatomical paths.

²⁴ Such reorganization is found throughout the animal kingdom, including humans. Polanyi cites Renoir as an example of this. During the last 20 years of his life, Renoir, who suffered from severe arthritis and lost the use of his hands and feet, learned to paint with the brushes lashed to his forearms. He painted a number of paintings which are hardly distinguishable in quality or style from those he had painted before. He had learned to use a new set of operating principles which were equipotential to his previous skill. ²⁵ Another form of this equipotential is seen in the use of knowledge to achieve certain purposes. At least down to the level of earthworms, living beings have the capacity of reorganizing their knowledge indefinitely in the face of new circumstances. In these cases, the parts of their knowledge can be organized into an almost limitless number of configurations, all with the aim of accomplishing a goal.

Another type of equipotential is found in morphogenesis, the formation of the right shape in animals. In classic biological experiments, Driesch and others determined that fragments, and in some cases, single cells, could be removed from embryos and these parts could grow into whole embryos. Some experiments on early embryos also showed that cells could be moved into a new part of

the embryo and would develop into the new organ in which they were placed. Driesch termed these observations as harmonious equipotential systems.

For Polanyi, morphogenesis (development into its characteristic shape) corresponds to the shaping of the machine. Through taxonomy, organisms are placed into classes based primarily on their shape. We all perform this basic task each time we identify a cat, a primose, or a human being. Each individual organism is appraised based on a standard appropriate for it as a member of a species. This is an endeavor much like determining the class of machines. We must determine what class it belongs to by its shape and function. Likewise, when an organism is considered normal or abnormal, healthy or sick, mutilated, malformed or well shaped, it must necessarily be based on the standard of rightness that is proper to the individual in question. The standard is common to the species to which the organism belongs.

A taxonomist's judgment of rightness operates on two levels. On the upper level, it establishes the physiological shape and function of the species, while on the lower level, it is applying these criteria for the appraisal of a single individual.²⁶ Thus, every time a

specimen is appraised, the standards of normality are adjusted to include the individual in question. These standards also can be evaluated in themselves by the taxonomist.

However, one must not assume that Polanyi believes that the description of species is arbitrary or subjective. Some species are well-defined; others are spurious. For example, we see a strange bird. We recognize it as an owl, but it is not like any owl that we have seen. Why is it that we do not consider it a new sparrow instead of an owl, and simply adjust our concept of sparrow? Polanyi argues that in the end the only answer that we can give is that it makes sense to modify our current concept of owl to make room for this individual, while it would be nonsense to modify our concept of sparrow to include this specimen. The former conceptual decision is right, and its implications are true; the latter decision is wrong and its implications false.²⁷

While Polanyi acknowledges that this explanation may lack the justification which we would like to see, it is the best that he can provide. Life and its unique features can not be specified in more impersonal facts. It is part of our tacit knowledge. For Polanyi, this position is a philosophical commitment. And it is a commitment

which can only be rejected by accepting an alternative philosophical commitment.²⁸

Polanyi's Position Necessitates Holding a Hierarchical Theory of Nature

Polanyi is claiming that there is a fundamental natural discontinuity between inanimate holistic systems like thunderstorms and machines and living beings. There is a qualitative difference between inanimate holistic systems on one hand, and machines and living systems on the other hand. It is the characteristic feature of having the dual control type of boundary conditions which differentiates machines and living beings from inanimate nature. Implicit in Polanyi's identification of the dual control type of boundary conditions is a theory of nature which is ultimately hierarchical, i.e. each level relies on the principles below it to work, even while it is not reducible to the lower principles.

In Polanyi's view, there usually are more than two levels in the hierarchy. For instance, at the lowest level are the inanimate laws of chemistry and physics. One level above that is the vegetative level, i.e., those functions which sustain life. This level leaves open a number of other functions which can operate on higher levels, such

as, growth, reproduction, and muscular movement. Even higher than that are behaviors. This is a rejection of the continuous theory of nature which claims that nature is ultimately reducible to an explanation of matter in motion.

To claim that a fundamental discontinuity exists in nature means that there is a qualitative difference between at least one set of two objects in nature. For something to differ qualitatively means that something, an object for example, has a defining characteristic not possessed by other objects. In geometry, there are triangles and rectangles. Triangles are three-sided, while rectangles are four-sided. It is a unique and a defining characteristic of triangles that they have three sides. It is also the case that rectangles are defined as having four sides. Triangles are qualitatively different from rectangles by the unique feature of having three sides.

A second feature of a qualitative difference is that there is a gap or discontinuity between their differences. No intermediate state between the two objects is possible. For example, triangles are three-sided; rectangles are four-sided. No object with an intermediate number of sides is possible between the two objects. We can not have a three and a half-sided object. However, if the two

objects that we have are a triangle and a pentagon, an intermediate (a rectangle) is possible, so this feature of qualitative difference must have the further stipulation that no intermediate objects are possible between adjacent members of a series. Triangles and pentagons are not adjacent members of a series. If we graphed these objects along an axis based on the number of sides of the objects, we would say that these objects are discontinuously distributed. There is a gap where no intermediate states are possible between adjacent members of the series.

Objects which differ continuously or quantitatively share a common feature (F); however, objects have differing amounts of (F). For example, two objects have the feature, length. While they both possess this feature, they may have differing amounts of (F). Here, it is always the case that intermediate lengths are possible. Objects graphed along an axis of length are distributed continuously. When two objects being compared differ quantitatively, we talk about one of the objects having more or less than the other object. Neither of the objects has a defining characteristic that is not possessed by the other object.

As all-encompassing views of nature, these two categories are contradictory. One or the other is correct. They cannot both be true. Either differences in nature are all differences of degree, or there are some objects which differ qualitatively from each other. (This is not to say that if the view of qualitative differences existing in nature is accepted and that no two objects would have features which differ quantitatively. It is a claim that the defining characteristics of some objects differ qualitatively). If nature is ultimately continuous, then no object will differ qualitatively from any other object. If a claim is made that one object in nature has a unique feature not possessed by any other object, then logically it necessitates acceptance of the view that in nature there are qualitative differences.

However, there are further complications. It is possible that objects may appear to differ in kind or degree, and yet these objects may not actually differ in that particular way. For example, colors are thought to be distributed continuously along an axis of wavelengths. The reason two colors may appear to differ qualitatively, or discontinuously, is that members of the continuum are missing. If all the intermediate colors were present, then the colors would be continuously distributed. Another example, which

will become important in the next section, is the question of kinds of plants and animals in nature. There at least appear to be kinds in nature. Differences of kinds suggest qualitative differences. The question is, are the differences in kinds real, or are they only apparent? Charles Darwin, for example, claimed that the differences in kinds were only apparent differences. He accepted a continuous theory of nature, and he explained the apparent differences in kinds by claiming that the intermediate members of the continuum were missing. If all intermediate members were present, then nature would appear continuous.²⁹

A second possible all-encompassing view of nature is that some objects do in fact differ qualitatively, but the differences are due to differing amounts of a particular feature. The objects might have a unique feature with no possible intermediates; however, these differences might be due to a feature that is continuously distributed. As the amount of this feature increases, there is a threshold over which the characteristic feature is qualitatively different. For example, objects in the world are found in one of three possible physical states: solid, liquid, or gas. Each state has unique defining characteristics and no intermediate states are possible.

Therefore, they are qualitatively different; however, this difference in kind is underlain by a quantitative difference. In this case, temperature differences are responsible for the qualitative differences. This theory of nature is held by the dialectical materialists. They agree with the Cartesians that nature is material and continuous at its root; however, they believe that qualitative differences are possible due to the operation of thresholds.³⁰

The third possible view is that qualitative differences exist and are not the result of quantitative differences. Rather, they represent real differences. For example, Aristotle's view was that there are discontinuities in nature. There are real differences between living beings and inanimate nature, or between plants and animals. These categories have unique defining characteristics, and no intermediates are possible. In this view, nature is seen as a set of hierarchies.

Michael Polanyi holds a hierarchical view of nature, and he has rejected the continuous view which underpins the Cartesian view. Polanyi is claiming that some objects are qualitatively different from other objects. Some objects in nature, such as organisms, have dual control boundary conditions, while other objects in nature, such as thunderstorms, do not have these dual control conditions. Therefore,

Polanyi either holds the second or the third view. It is not important for the purposes of this study to discuss which of the two hierarchical views Polanyi holds.

Summary

Polanyi showed that the existence of dual control boundary conditions in machines represented a discontinuity in nature between machines and holistic inanimate systems. The two levels of control were operational principles and the laws of physics and chemistry. The operational principles form the rules of rightness for the machine. It specifies the purpose of the machine and the rational organizations of the parts to accomplish its purpose. The lower level was the physical and chemical laws of the inanimate natural world. The higher level harnesses the physical and chemical laws to accomplish the purpose of the machine. The operational principle describes what is good working order for a machine of a particular kind. If we ignore the operational principle of machines, we cannot even recognize objects as machines, and we cannot tell if the machine is working successfully or not. We identify a machine by understanding it technically, i.e., by a participation in its purpose and

an endorsement of its operational principle. Physics and chemistry, which ignore operational principles, are blind to success and failure because they ignore the operational principles by which success or failure are defined. Similarly, Polanyi argued that it is illogical to consider thunderstorms, which are holistic inanimate systems, as succeeding or failing because they do not possess operational principles. They do not have rules of rightness. Thunderstorms do not have dual control boundary systems; therefore, they lack the standard by which success or failure could be measured.

Polanyi believes that living beings are more like machines than holistic inanimate systems. Living beings have dual control boundary controls; they have at least two levels of control. From Polanyi's perspective, there is a set of hierarchies which operate in every plant or animal. In living beings, processes at the lowest level are caused by the forces of inanimate nature. The next level of boundary condition controls are called vegetative functions, i.e., those functions that sustain life. They leave open the higher functions of growth, reproduction, and muscular movement. Included at this level is morphogenesis. The next level includes the principles which integrate the muscular actions into behaviors in animals. Each level

relies on the lower level. Once we reach the vegetative level, we have arrived at a level where we can talk about success and failure. To know a machine is to know its purpose and acknowledge the rationality of its operations. To know an organism is to acknowledge the existence of an individual and appreciate its form and function.³¹ By recognizing an individual as a member of a species, we recognize there is a type. There is an ideal shape and an ideal functional relationship between its parts. When we appraise the individual's shape and compare it to the type shape for that species, we call it normal or abnormal. This is an appraisal of the morphogenesis of the individual. It is an achievement that can succeed or fail. When we judge a species as healthy, we are acknowledging that this particular member of this species has all its parts functioning according to the type. Since health is a standard of rightness similar to operational principles in machine, we do not inquire into the cause of health; it is simply the standard. It is the expected condition. It needs no explanation.

Polanyi believes that intellectual assent to the reduction of the world to its atomic elements, acting blindly in terms of equilibrium of natural forces, has come to prevail since the birth of science.³² The

Cartesian philosophical commitment to the reduction of biology to physics and chemistry removes the standards by which we could evaluate the health of an organism. The health of an organism is an appraisal of the function of the particular member of a species against the standard for the species. If there are no higher level boundary controls above the level of the inanimate laws of chemistry and physics, then the concept of health has lost its standard, and it is incoherent to consider the concept of health of an organism. For as we saw in machines, physics and chemistry are blind to success or failure. It is engineering which focuses on the purpose of the machine and the operational principle. Likewise, Polanyi believes that physiology is logically related to organisms much like engineering is related to machines. Hence, physiology is teleological, for it must focus on the function a part plays within the whole of the organism.³³ Any attempt to study the physics or chemistry irrespective of the whole organism is doomed to meaninglessness. For example, a study of an organ for which its function in the animal is unknown, the physiologist must guess the role the organ plays within the organism, much like an engineer must guess at what an unknown machine is.

Notes

¹ M. Ruse, *Philosophy of Biology Today* (State University of New York Press, Albany, 1988), p. 24.

² The view that there is something more than the parts has pervaded biology since Aristotle. The modern term for it is vitalism. For individuals who share the Cartesian theory of nature, the aim of biology is to purge vitalism out of biology. In fact, many, including Francis Crick, claim that biology is a science only to the extent that organisms can be explained by the laws on inanimate chemistry and physics.

³ In a classic paper, Ayala identified three types of reductionism. Only ontological reductionism is relevant to this study. F.J. Ayala and T. Dobzhansky (editors), *Studies in the Philosophy of Biology*. (MacMillan, London, 1974) p. vii-xvi.

⁴ M. Ruse, *Philosophy of Biology Today*, p. 24.

⁵ Polanyi's starting point for his 1967 paper, 'Life Transcending Physics and Chemistry,' *Chemical and Engineering News*, Aug 21, 1967, pp54- 65, is the debate between Francis Crick and Barry Commoner. Commoner, in his book, *Science and Survival*, Viking Press, New York, 1966, Chapter 3, had criticized Crick's view. Polanyi begins by interacting with Commoner, then turns to Crick's view. I follow Polanyi's line of argumentation to clarify the underlying philosophical positions involved.

⁶ F.Crick, *Of Molecule and Men* (University of Washington Press, Seattle, 1966), p. 10.

⁷ F. Crick, *Ibid.*, p. xi.

⁸ The opposing theory of nature is that nature is hierarchical. In this view, objects can differ in kind. They can be qualitatively different from each other. Objects which are qualitatively different from each other are characterized by having unique features which are not

possessed by other objects. In the continuous theory, all objects differ only quantitatively. They have more or less quantity of atoms in motion. This distinction will be discussed in greater detail later in the chapter.

⁹ Driesch found that cells removed from sea urchins during the early stages of the development would develop into whole urchins.

¹⁰ M.Polanyi, 'Life Transcending Physics and Chemistry', p. 55.

¹¹ M.Polanyi, *Ibid.*, p. 56.

¹² M.Polanyi, *Ibid.*, p. 56.

¹³ M.Polanyi, *Ibid.*, p. 56.

¹⁴ The law is Snell's Law. M. Polanyi, *Ibid.*, p. 56.

¹⁵ M.Polanyi, and H. Prosch, *Meaning*, (The University of Chicago Press, Chicago, 1975) p. 170.

¹⁶ M. Polanyi, 'Life Transcending Physics and Chemistry', p. 57.

¹⁷ M.Polanyi, 1957. *Personal Knowledge*, (The University of Chicago Press, Chicago, 1957), pp. 328-9.

¹⁸ In the television show Star Trek, the tricorder is a machine for searching for life forms.

¹⁹ M.Polanyi, 'Life Transcending Physics and Chemistry', p. 61.

²⁰ M. Polanyi, 'Life's Irreducible Structure', *Science* 160(1967), pp. 1308-1312, p. 1308.

²¹ M. Polanyi, *Ibid.*, p. 1310.

²² M. Polanyi, *Ibid.*, p. 1308.

²³ M. Polanyi, *Personal Knowledge*, p. 335.

²⁴ M. Polanyi, *Ibid.*, p. 338.

²⁵ M. Polanyi, *Ibid.*, p. 337.

²⁶ M. Polanyi, *Ibid.*, p. 345.

²⁷ M. Polanyi, *Ibid.*, p. 111. It is beyond the scope of this study to critique Polanyi's argument here. For the purposes of this study it is sufficient that Polanyi has made good his claim that biology functions more like machines than inanimate holistic systems.

²⁸ This topic will be raised in greater detail in Chapter 4 of this study.

²⁹ A more complete discussion of this analysis of Darwin's theory will be given in Chapter 3 of this study.

³⁰ P. Edwards, *The Encyclopedia of Philosophy, Volume One* (MacMillan Publishing Co, New York, 1967) pp. 391-2.

³¹ M. Polanyi, *The Study of Man*, (The University of Chicago Press, Chicago, 1959), p. 74.

³² M. Polanyi and H. Prosch, *Meaning*, p. 62.

³³ M. Polanyi, *Personal Knowledge*, p. 320.

CHAPTER III

IS ECOSYSTEM HEALTH A COHERENT CONCEPT IN CONSERVATION BIOLOGY?

Introduction

In the last chapter, we examined Michael Polanyi's rejection of both the Cartesian position of Francis Crick and the position of Barry Commoner, one of Crick's critics. Polanyi rejected the premise, common to both Crick and his objector, that biology is mechanistic to the extent that it is reducible to the inanimate laws of chemistry and physics. Polanyi argued cogently that machines are not reducible to the laws of physics and chemistry because machines are hierarchically structured. They cannot be reduced to the laws of chemistry and physics. According to Polanyi, machines have dual control boundary conditions. The inanimate laws of chemistry and physics are on the lower level, while on the upper level are the operational principles which harness the inanimate physical laws to accomplish the purposes of the machine. The operational principles,

which are not reducible to the laws of chemistry and physics, establish the rules of rightness for the machine. The operational principles describe the machine in proper working order.

Polanyi showed that machines and living organisms have dual control boundary conditions which are absent in inanimate holistic systems such as flames and thunderstorms. The operational principles function as the rules of rightness (or standard of health) for the class of objects. Flames and thunderstorms, like all inanimate systems, lack operational principles. They are reducible to the laws of chemistry and physics. It makes no sense to consider success and failure of this type of system. Since they lack dual control boundary conditions, they also lack operational principles. There are no rules of rightness for them.

For Polanyi, machines and biological organisms are hierarchically structured. Therefore, Polanyi rejects the continuous theory of nature which underlies the Cartesian position of Francis Crick. He accepts a hierarchical theory of nature. The Cartesian perspective does not recognize the different logical structure between machines and biological organisms on one hand and inanimate holistic systems such as flames and thunderstorms on the

other. To the Cartesian, all systems are reducible to the laws of chemistry and physics.

Polanyi's framework is based on a recognition that at least two different logical structures of objects exist in nature, one for machines and biological organisms, and one for inanimate holistic systems. The import of this distinction for this study is that the concept of health can only be coherently applied to things which have dual control boundary conditions and rules of rightness. The concept of health is not applicable to flames and thunderstorms because they do not have dual control boundary conditions. The question which is the focus of this chapter may be phrased, "Is the nature of ecosystems more like machines and organisms or more like flames and thunderstorms?"

This chapter will examine ecosystems, in particular, watershed ecosystems between 20-200 mi², and will look at whether these systems have dual control boundary conditions like machines and biological organisms, or if they are more like inanimate holistic systems such as thunderstorms and flames. These watershed ecosystems are the current focus of conservation biologists in the Pacific Northwest. Governmental agencies, environmentalists, and

conservation biologists are currently trying to decide if "health" is applicable and what such an assessment would look like. These systems are also the focus of restoration efforts for the Pacific salmon.

I will begin with an historical overview of the development of the concept of an ecosystem in ecology from before 1935 when Alfred Tansley first penned the concept to Eugene Odum's modern conception. Although the question had not previously been asked whether ecosystems have control boundary conditions, tracing the development of the ecosystem concept will help clarify whether or not ecosystems do in fact have dual control boundary conditions. In last portion of the historical overview, I will examine ecosystems from the perspective of general systems theory. This has been a major area of research since World War II. The chapter then examines and critiques the concept of ecosystem health from two influential current perspectives. The critique of the two contemporary views in light of the historical overview shows that major problems have existed within the ecosystem concept beginning with Tansley. In conclusion, I will interact with the five objections which have been raised by the opponents to the concept of

ecosystem health. This will help clarify the position developed in this study and locate it within the current debate.

Development of the Ecosystem Concept in Ecology

The ecosystem concept was first penned by the British ecologist, Alfred Tansley, to correct what he considered to be dangerous speculative thinking in ecology.¹ At the turn of the century, ecology was largely emerging from the discipline of plant biogeography. The major activity was classifying the major plant communities based on regional climatic patterns.² A hierarchical approach was taken. Within each climatic region, a mosaic of vegetation types were recognized, such as bogs or meadows. Probably the first major development in ecology was the development of the idea of plant succession. The idea was that if plant communities were left to themselves, they would develop toward a characteristic plant community.³ F.E. Clements developed succession into a foundational ecological theory.⁴

Clements had proposed a view of a level of organization above populations. He referred to this level as the complex organism to differentiate it from recognized organisms, such as humans, elk, or

salmon. As Clements saw it, the vegetation of the earth's surface is arranged into groups of definite constitution, and more or less definite limits, based on broad regional climatic patterns. Such a group he called a plant formation. For example, he mapped forested regions in the humid areas, grasslands in areas with moderate rainfall, and deserts in more arid climates. Clements called these characteristic vegetations climax conditions. His idea was that these climax vegetations were complex entities which reacted to the environment as a group. He envisioned vegetation going through a series of deterministic successional stages to the climax formation. Ecological succession was the integration of interactions among the plant species. He saw an analogy between development of an organism and development of individual organisms.⁵ The roots for Clements's theory were deep in the idealistic tradition of European plant geography.⁶ His acknowledged influences were Oscar Drude and A. von Humboldt. What characterized their view was that a level of organization above the plants species existed, called the formation, which was ontologically distinct from the individual plants in it. The concept of a formation was similar to that in geology. The formation was typological, i.e. it had unique properties, but it arose

from statistical differences along a continuum.⁷ For Clements, also like his above mentioned predecessors, the formation was seen as the whole series of successional changes which the plant communities underwent from their initial stages to the climax stage.

In a series of five papers, a South African Ecologist named John Phillips attempted to ground Clements's concepts in a holistic philosophical system developed by a fellow South African, Jan Smut. As Smut saw it, matter, life, and mind were a more or less connected progressive series of the same "Process". This "Process" underlies and explains the character of all three. The key concepts in Smut's system are holism and emergent evolution.⁸ He identified unified structures called wholes that included bodies, minds, organisms, and personalities. Wholes were not viewed as a mechanical system. Machines, for instance, are only acted upon or act upon external objects. Wholes also have an added dimension of having inner tendencies which are transformed when the whole interacts with the environment. Smut also maintained that the emergence of properties of a whole required a special explanation, a kind of "soul".⁹ However, in Smut's system the ecological community was not a

whole. That was Phillips's creation. Phillips argued that Smut's system provided the philosophical grounding of Clements' theory.

The response to Phillips's articles came quickly. Arthur Tansley in his 1935 paper, "The use and abuse of vegetational concepts and terms", introduced the concept of the ecosystem as an alternative to Phillips's ideas.¹⁰ The ecosystem is a system which includes the organism-complex and the environment. Ecosystems are the fundamental units of ecology and can vary in size and kind from the universe to the atom. Tansley avoided all references to biological theories, specifically Clements' or Phillips's theories. He desired to avoid metaphors and analogies. He emphasized the physical features of the ecosystem and its relationship to physical systems in general.¹¹ He patterned the concept after physical theory founded on the concept of equilibrium. The closer an ecosystem is to its climax vegetational state, the closer it is to equilibrium and the greater its chance of survival. Tansley perceived that ecosystems tend to develop greater integration and become more finely adjusted over time. Thus, an ecosystem as Tansley described it is a theoretical concept; however, it was quite ambiguous. Tansley never clarified whether he considered "ecosystems" as objects of nature or

something else. He never made clear whether his concept existed in reality (ontological reality), or whether it was only concerned with our experience (epistemological reality). He never applied it to any of his own work, nor did he refer to it again.

What we can say about Tansley's view of an ecosystem is that it was largely grounded in mathematical theory. According to Tansley, a system is not artificial constructs of a perceiving mind. The elements had to have functional integrity and have a certain amount of stability. Such a system was not an organism. In his brief description, the scientific mind did not apprehend "objects" or "organisms" but only "events" or interrelated phenomena.¹² Perception was viewed as an act of "isolation". Abstracted isolates were "wholes" isolated from a larger environment. An ecosystem was a "whole" isolated from a larger environment.¹³

According to Golley, part of Tansley's motivation for his paper was to defend ecology from extreme philosophizing, and maintain its connection to mechanistic, reductionistic science, thus protecting ecology's reputation as a science.¹⁴ Such a physical system, an ecosystem, avoided biological or organismic theories which were at the heart of a potential split in ecology between the holistic

materialists and the reductionists.¹⁵ The holistic materialists are concerned with how the parts are organized to create a whole. Scientific holists can hold a wide variety of positions from an idealistic position, which believes that an immaterial organizing principle exists which could not be penetrated by science (vitalism), to a materialistic view, which only emphasizes how the parts interact. The reductionist divides the wholes into its parts and tries to understand the workings of the parts.

Tansley also wanted to identify the common foundation of ecology. He saw this grounding in a physical theory as a way of obtaining recognition for ecology as a serious science.¹⁶ At the center of ecosystem theory was the concept of physical equilibrium. By connecting ecology to physical science, with its emphasis on precise mathematics, Tansley believed that ecology would be on a good scientific foundation. He was a professor at Cambridge University, England, and he was well aware of the great advances that were being made in physics at the Cavendish Laboratory at Cambridge. A major concern was to move the model of science in ecology toward the model of science of the physics laboratory. He was not only trying to obtain recognition for ecology as a serious science, but he

was trying to obtain recognition for himself as a scientist of high skill.¹⁷ His paper was his attempt to steer ecology toward firm ground. He did not clarify his concept any further, nor was the ecosystem at the core of his research.¹⁸

Henry Gleason also was quick to respond to Phillips's papers. In a series of papers, he stressed the individual behaviors of single species in response to changes in the environment.¹⁹ While Clements and others saw the community as an organism, Gleason saw only a random association of species which happened to share similar environmental tolerances. When he plotted the distribution of plants along environmental gradients like moisture or temperature, he reported that species replaced one another all along the gradient. If plant communities were organisms, as Clements' theory suggested, then most species replacement should occur at a few key points along the gradient. There should have been a few key points of discontinuity which separated the communities which had characteristic features. These groups would have been type classes. Based on the observation that species replacement data did not fit Clements' theory, Gleason argued there was no such thing as a complex organism.²⁰ The debate between Clements's organismic

view and Gleason's population view has been a recurring debate up until the recent past. As we will see shortly, it has recently waned considerably.

The first ecologist to explicitly use Tansley's notion and adapt it as a conceptual model to explain the workings of a natural system was Raymond Lindeman. He and his wife collected field data on a small bog in Minnesota and attempted to quantify the workings of the bog. He organized the species data into what he termed trophic dynamic aspect.²¹ This perspective linked organisms according to what they ate. The biomass (amount of material) of each species was converted into calories so the energy flow could be traced through the system. He also linked the total productivity of the bog to the idea of succession of bogs and modified the theory of succession in lakes. The older model had held that lakes begin very unproductively and then increase in productivity and begin to fill in. This process continues until the lake fills in and develops through a bog and meadow to a climax forest. It was thought that productivity would increase at each level to the climax forest. Lindeman found in his study that the bog was far less productive than the later lake

stage, so lake systems do not increase in productivity throughout their development.

Probably the most widely accepted definition of an ecosystem today is that of Eugene Odum:

Any unit that includes all the organisms (i.e., the community) in a given area interacting with the physical environment so that a flow of energy leads to clearly defined trophic structure, biotic diversity, and material cycles (i.e., the exchange of materials between living and nonliving parts within the system is an ecosystem).²²

Odum's concept of an ecosystem is designed to be a broad one. Its main function in ecology is to emphasize the obligatory relationships, interdependence, and causal relationships that form the functional unit. The trophic components include both the primary producers, which produce carbohydrates from simple inorganic substances using the energy of sunlight, and the organisms which feed on other organisms. The latter category includes the bacteria or other decomposers, which feed on dead organic material. For descriptive purposes, an ecosystem has a number of components:

- 1) inorganic elements which are involved in chemical cycles originating from rock or the atmosphere, such as carbon, nitrogen, carbon dioxide, and water;
- 2) organic compounds which are produced and move through the system;
- 3) the climatic regime;
- 4) primary

producers (mostly green plants); 5 macroconsumers; 6) decomposers.

²³ From a functional standpoint, an ecosystem can be analyzed in terms of energy circuits, food chains, nutrient cycling, development, and control cybernetics.²⁴ Ecosystems are considered to be systems because the parts (populations of species) are coordinated in a tightly-knit organization. In another context, Polanyi argues that it is easiest to grasp this notion of coordinated organization by considering cases where an organization does not have this coordination. Consider a group of people shelling peas or a chess team. If the members are separated from each other, does it affect the amount of peas shelled or the number of games of chess won? If the answer is no, and I think that it is the case for the two examples given, shelling peas and a team playing chess, then these organizations are not coordinated. If the members cannot be separated without affecting their production, then they are coordinated.²⁵ Ecosystems are clearly coordinated organizations or systems. If the green plants were not there, the herbivores or primary consumers would have no food supply. Thus carnivores would die.

In Odum's view, an ecosystem can be of almost any size, from a pond to the biosphere.²⁶ However, the most convenient and most often studied ecosystems include ponds and drainage basins.²⁷

Drainage basins can easily be shown to be coordinated organizations or systems. First, they have recognizable boundaries. (Watersheds are technically the ridges which separate water flow from one basin to the next). Most of the physical and chemical processes found in a basin are associated with the movement of water through the basin, so the drainage basin is a recognizable functional unit. It is delimited by the watersheds. Also, if we separate the functional groups of organism which produce living matter (green plants) from the group of organisms which eat those plants (herbivores) in a drainage basin, we will affect the amount of production the system can produce. Clearly then, drainage basins have recognizable boundaries and are coordinated systems.

Drainage basins from 20-200 mi², which are the focus of this study, easily fall within Odum's concept of an ecosystem. Drainage basins this size or smaller have been a major experimental unit throughout the history of ecology and conservation biology. For the

rest of the study, unless otherwise specified, references to ecosystems refer to drainage basins from 20-200 mi².

Ecosystems as "Hierarchical" Systems

Arthur Tansley coined the term "ecosystem" and modeled it after physical systems and equilibrium theory in physics. He believed that such a concept would establish ecology on sound scientific grounds. Considerable progress has been made both in ecosystem theory and the theory of systems in general. This section will examine the development of the ecosystem concept from the perspective of general systems theory. Especially after World War II, the general systems approach has been a dominant perspective of ecosystem ecologists.

During the 20th century, systems have been extensively studied in their own right, and interest in this research has developed around a general systems theory. After World War II, Ludwig von Bertalanffy and others have created a whole discipline around a general systems theory.²⁸ They widened the application of systems from physics to ecology and on into the social sciences. They have described a number of classes of systems which can be used to

describe a diverse array of phenomena. For instance, some systems appear to be self-regulating or purposive (teleological), such as organizations, machines, and organisms, while other systems are not. L. von Bertalanffy attempted to make the concept of the self-regulating system the basis for a new "organismic" theory. This new theory went far beyond what Tansley had wished and was precisely what Tansley was trying to escape.²⁹

Some systems in this approach are considered to be closed and others open. In a closed system, no matter or energy is entering or leaving the system. Closed systems are closely linked to the study of thermodynamics, and the second law of thermodynamics states that there will always be a loss in energy over time (increase in entropy) in closed systems. Matter and energy are often quantified at a particular time and then it is predicted what their relationship or state will be at a later time. In open systems, matter and energy are moving into and out of the system. In fact, it is often the case that the structure of an open system is maintained by a constant supply of energy coming into the system. Open systems are often used by biologists and ecologists. Organisms and ecosystems are often considered to be open systems, as are flames and thunderstorms.

Today, ecosystems are commonly viewed by ecologists using a minimal systems theory, with no teleological implications.³⁰ Ecosystems are seen as components which interact at various time and space scales. Emphasis is placed on the existence of multiple levels within a "hierarchy", and close attention is placed on the time and space scales at which processes are occurring. This view rejects the organismic theory of Clements, Lindeman, and to some extent, Odum. There is no ideal or standard or normative condition of ecosystems, nor is their developmental change. Earlier, Clements saw plant succession as moving toward the climax plant community, and Lindeman saw ponds going through a succession from lakes to bogs, meadows, and then to forests.

The shift in paradigm away from any notion of normality within ecosystems occurred during the last 15 years when ecologists began to examine the role of disturbance, both natural and human caused, in maintaining the organization and function of the ecosystem.³¹ When ecologists reviewed a wide range of ecological data, they were unable to demonstrate that any ecosystems had an equilibrium state.³² Here in the Pacific Northwest, fire, floods, disease, wind storms, and human activities are viewed as major

agents of disturbance in our watershed ecosystems.³³ With major floods for instance, the streams and valleys are reshaped and reset to new trajectories.

The recent view does not see any end point. It sees only watersheds on a trajectory for a short period of time until a disturbance occurs which alters the state and trajectory of change within the ecosystem. Various disturbances can have a wide variety of effects on ecosystems. During some disturbances, the watersheds may be reset far from where they were just prior to the disturbance.

Back when ecosystems were seen as having a normative end point or telos, or an equilibrium point toward which they were heading, the notion of ecosystem health provided few conceptual difficulties. Ecosystems were seen as analogous to organisms. However, with the abandonment of any sort of end point or equilibrium, the concept of health became problematic.³⁴

The contemporary debate lies here. Two works which exemplify the currently prevalent views are Costanza, et al.³⁵ and O'Neill, et al.³⁶ Costanza, et al. recognized that ecosystem health is a complex topic and took a pluralistic systems approach to the question. However, it is noteworthy that not a single chapter author

in their book suggested or defended the organismic theory. O'Neill, et al. also took a systems approach to the topic of ecosystems. In both cases, the notion of ecosystem health is problematic. Where do we go from here? Do we abandon the whole notion of health? (There are many proponents of this position: e.g. Schindler, Rapport, Regier and Hutchinson.)³⁷ Or do we redefine the concept of ecosystem health? Or something else? It is into this current debate that this study is aimed. In the next two sections, I critique the perspectives of Costanza, et al. and O'Neill, et al.

A Critique of Robert Costanza, et al.'s View of Ecosystems as Systems

In 1990, Robert Costanza and others organized a workshop aimed at giving a clearer conception of the health of natural environments. They recognized that "health" in relation to ecosystems had never been adequately defined to be of practical use for policy decisions.³⁸ Toward the goal of adequately defining ecosystem health, two workshops were held, and the outcome was published in their 1992 book.

Costanza, et al. believe that some form of systems analysis toward ecosystems should be accepted. They adopt a minimalist

systems approach, interpreting ecosystems as hierarchically organized systems. Specifically for them, this entails an ecological theory without any teleological implications.³⁹ They focus on the multiple levels of organization and multiple space and time scales. In their view, more specific aspects of the characteristics of ecosystems should be considered a matter of experimentation and empirical determination. They specifically reject a full-blown organismic theory, such as that espoused by Clements.⁴⁰ In their view, organism is useful only as a metaphor. Specifically, they argue that,

In fact, open, nonliving systems-especially dissipative structures- provide a more robust conceptual analog than do either mechanisms or organism. Dissipative structures, from chemical clocks to complex ecosystems, maintain their system and structure through interchanges of energy with their environment. And while it is often useful to note the similarities between organized systems and organisms, use of these metaphors and analogies need not imply a literal assertion that on any given level ecosystems are alive.⁴¹

It was Costanza, et al.'s claim that open, nonliving dissipation structures (such as flames or thunderstorms) provide a more robust conceptual analog for ecosystems than either mechanisms or organisms that brought to mind Polanyi's argument against Crick and Commoner.⁴² Since all holistic inanimate systems (open, nonliving dissipation structures) were reducible to the laws of chemistry and physics, then it logically follows that there were no operational

principles to serve as the standard of rightness for these systems. Therefore, the concept of health does not apply to these systems. Costanza, et al.'s claim meant that ecosystems do not have dual control boundary conditions, and therefore they are explainable by the laws of chemistry and physics. The concept of health is also not logically coherent for these types of systems. It makes no sense to consider the dissipation of these holistic inanimate systems as failure. It is just the result of the laws of chemistry and physics. Costanza, et al., by preferring the open, dissipative systems model, such as a flame or thunderstorm as the best metaphor for ecosystems⁴³, have rendered health to be logical nonsense.

The result of their belief that open, dissipative structures are a more robust analog for ecosystems can be seen directly by examining their concept of health. They define health from a minimalist open systems approach,

...in terms of four major characteristics applicable to any complex system: sustainability, which is a function of activity, organization, and resilience. An ecosystem is healthy and free from 'distress syndrome' if it is stable and sustainable- that is, if it is active and maintains its organization and autonomy over time and is resilient to stress.⁴⁴

They claim that this definition is applicable to all complex systems.

Two key concepts within this definition are sustainability and

distress syndrome. Sustainability implies that the system can maintain its structure and function over time. A diseased system is one that is not sustainable. "Distress syndrome" refers to the irreversible process of system breakdown leading to the collapse of the system.

While most ecologists would not have a problem applying that definition to ecosystems, let us begin by applying this definition of health to a flame or a thunderstorm. A flame or a thunderstorm, which is a complex system, is healthy and free from "distress syndrome" as long as it is stable and sustainable- that is, if it is active and maintains its organization and autonomy over time and is resilient to stress. A thunderstorm or a flame is diseased if it is not sustainable. Such a definition of health is problematic because it fails to recognize that the concept of health does not even apply to holistic inanimate systems. To call an open, dissipating system diseased is nonsense. The notion of disease is not like the notion of dissipation. Dissipation is just obeying the laws of chemistry and physics. Disease refers to something other than just obedience to the laws of chemistry and physics. Their analysis is blind to the distinction between machines and organisms on one side, and holistic inanimate

systems on the other side. It is blind specifically because they have ruled out purpose, and without considering the purpose, they have ruled out the attribute upon which health is determined.

Costanza, et al.'s philosophical commitments made it difficult, if not impossible, for their method of systems analysis to detect the difference between open, dissipative, holistic, inanimate systems and machines and organisms. They were careful to rule out any consideration of purposes (teleology).⁴⁵ Yet a chemical and physical investigation of a machine without consideration of the purpose of the machine could never determine if the machine was functioning properly or not. In fact, such an analysis could not even identify the class of the object. A systems analysis without consideration of purpose cannot even identify the classes of machines.

The effect of Costanza, et al.'s minimalist systems analysis is to reduce ecosystems to open dissipative systems; however, do not misinterpret the concern here. Ecosystems may be open, dissipating systems. My concern is that this systems approach does not enable us to determine if ecosystems are open, dissipating systems. It fails to provide us with a method of analysis for determining if ecosystems are open dissipating systems. Rather, it just considers

ecosystems as if they were open, dissipative systems. This assumption is what causes health to become problematic in ecology. Health is problematic in Costanza, et al.'s view because they are trying to apply it to systems like thunderstorms. Health does not apply to these types of systems. Their methodology and possibly their prior philosophical commitments do not enable them to make a distinction between types of hierarchical systems. The outcome of this difficulty is making the concept of ecosystem health problematic.

The problem of not differentiating between the two types of systems, machines and biological organisms on one hand, and inanimate holistic systems on the other, has existed in ecology since at least Tansley. If Golley is correct in his assertion that Tansley's motivation for developing the ecosystem concept was to " ...maintain its (ecology's) connection to mechanistic, reductionistic science.... ", then the problem has existed from the beginning of the concept.⁴⁶ The philosophical assumption of the mechanistic reductionistic science is that all systems are reducible to their atomic parts.⁴⁷ Therefore, all systems have the same fundamental structure. This view assumes a continuous theory of nature. While Costanza, et al. are not Cartesians, nor do they necessarily hold the continuous

theory of nature, the outcome of their perspective is in agreement with the Cartesian perspective on this issue.

Secondly, Costanza, et al. a priori reject any purposive or teleological elements within their systems perspective. Such a view does not allow any consideration of operational principles which determine how a machine or organism functions to accomplish its purposes. Without a consideration of purpose, the analysis of this study has shown that the concept of health and identifying the classes of machines or organisms becomes problematic. If ecosystems do not have dual control boundary conditions, then health is problematic. The dual control boundary conditions are based on the purposes of the machine or organism.

Michael Polanyi claims that it is primarily the commitment to the Cartesian project which has made any sort of teleological explanation unscientific. It is the philosophical commitment to ultimately explaining all phenomena as matter in motion that has made teleology an empty concept in science. There is no other purpose than the motion of atoms which is relevant to an explanation of objects. However, this is not to say that teleology must be grounded in a deterministic overriding cosmic purpose.

Charles Pierce and William James offered a different concept of teleology based on forces. In their view, there is a tendency toward achievement of a more ordered or regulated relationships which is inexplicable unless some principle operative in that direction was imminent in all things.⁴⁸ If we do not investigate the purpose of machines or organs, we can never come to an understanding which would enable us to identify what the objects were, nor could we make health a coherent concept.

In the next section, I will critique O'Neill, et al., another major contemporary work which develops the notion of ecosystem and ecosystem health. O'Neill and others also utilize a general systems approach to ecosystems; however, they develop a system which differs considerably from Costanza, et al.'s perspective. Even so, there is much in common in their perspectives.

A Critique of O'Neill, et al.'s View of Ecosystems as "Hierarchical" Systems

O'Neill, et al. have also recently examined ecosystems as systems.⁴⁹ Their book is recognized as a foundational work in recent ecosystem science. In their view, a physical system generally consists of two or more components which interact in significant

ways to produce a behavior and may or may not as a system interact with the environment.⁵⁰ Components in a system may be tangible, such as planets in the solar system or a gear in a machine, or intangible, such as words in a language or ideas in a philosophical system. A system is a human construct, and as such it is somewhat arbitrarily defined. However, it is a general concept covering a broad range of phenomena. This perspective is based on general systems theory.

O'Neill, et al. believe that in ecology, little justification is needed to establish that a systems view of ecological phenomena is appropriate. A systems view of ecological systems may be inherent in human experience in general, because a wide variety of cultures have coined words to characterize the various local ecological systems: tugai, tundra, steppe, chaparral, veldi, tiaga, and mallee, etc.⁵¹ This is not to say that such a systems approach implies that these plant communities are organisms as Clements' theory would imply. O'Neill, et al. make it clear that they make no a priori assumptions about the nature of the systems of interest. In fact, they specifically do not assume that ecosystems are purposive, nor

that they are organisms. They also reject the organismic theory of ecology.⁵²

.....However, teleology is not implied in the definition, one simply observes the interactions as the causal explanatory principles of the system's behavior. Thus, the observation that phenomena occur at the ecosystem level and that we can consider the ecosystem as a system for the purpose of studying those phenomena does not imply any a priori commitment to the system as a supraorganism.

The systems view of ecosystems held by O'Neill, et al., also claims to make no a priori judgments about a theory of nature. They say,

Like any scientific theory, hierarchical theory will ultimately prove to be another limited approach. Thus, we will not argue that the natural world must be, in fact, hierarchically structured. To make use of the theory, it is sufficient that many observation sets permit ecological systems to be conceptualized as hierarchical.⁵³

So, their systems view of ecosystems does not necessarily entail assuming a hierarchical theory of nature. Nature might be continuously defined as the Cartesian's assume. O'Neill, et al. admit that given current evolutionary theory and the current concepts of the origin of life, it is logical to assume that biological systems are hierarchically structured.⁵⁴ However, the question immediately arises whether, O'Neill, et al. mean the same thing by hierarchy as we have defined it in this study.

For O'Neill et al, hierarchical theory is based on general systems theory. Its purpose is to decompose complex ecosystems dynamics

into simpler components.⁵⁵ A hierarchy is a system with a number of levels where each level is composed of subsystems on the levels below and is controlled by the level above.⁵⁶ The levels of a hierarchy are determined by examining the process rates of various functions. The hierarchical levels are isolated from each other because they operate at distinctly different rates, either in space or time.⁵⁷ Consider a tree. The lowest level of the hierarchy is the light striking the leaves. The rates fluctuate rapidly, often differing from minute to minute, while at a higher level, the tree is growing and laying down annual rings. The tree growth is a higher hierarchical level than the light striking the leaves of the tree because the rates of these processes are orders of magnitude slower than the lower levels. Tree growth rate is occurring on the time frame of a year.

Consider the example of the different geomorphic processes and rates forming the coastline of Oregon. At the lowest level, a biologist may be looking at the effects of biological organisms on the surface of a boulder. The space-time scale is a year, and the space scale is a m^2 . At the same time, another geomorphologist may be looking at the effects of wave action and tides. Here, the scale is a few kilometers of beach over a time scale of decades to a century.

Simultaneously, a third geomorphologist may be looking at plate tectonics. Here the scale is thousands of kilometers over thousands of years. The lowest level process is cycling much faster than each of the higher levels. Most processes are only relevant at a particular space-time scale. The effect of this type of hierarchy is to separate processes from one another. The older view was that everything is connected to everything else. While O'Neill, et al. would agree with that on one level, they actually emphasize that the effect of their hierarchical model is to disconnect parts from each other. The processes are occurring at different space-time scales. From their perspective, it is the relative disconnection that constitutes the organization of the system.⁵⁸

In this study, each level of a dual control hierarchy relies for its workings on the principles below it, even while it is not reducible to the lower principles. Each level also leaves open possible boundary conditions operating at a higher level. The issue is control, i.e., boundary conditions. As we saw in machines, the upper level harnesses the lower level, the physics and chemistry of the system, to accomplish its purposes. In living beings, there are numerous levels of control. The lowest level is the inanimate physical and

chemical laws. The next level up is the vegetative level, which sustains life. The level above that includes growth, reproduction, muscular coordination, and morphogenesis. A level above that enters the level of behaviors.

The view of hierarchies in this study is significantly different from O'Neill, et al.'s. Here, hierarchies are based on control (boundary conditions), while O'Neill, et al. define them based on the rates at which processes occur. Given our four levels of hierarchies identified for organisms, (preceding paragraph) it is clear that hierarchies in this study are not related to the rate of processes. While the lowest two levels do meet O'Neill, et al.'s criteria, the highest level does not. The vegetative function is occurring at a slower rate than the inanimate laws of chemistry and physics. However, the highest level is behaviors. This level does not have slower rates than the levels below it (growth, reproduction, muscular coordination, and morphogenesis).

O'Neill, et al.'s concept of a hierarchy is a case where the boundary conditions set the historical context, but they do not function as operational principles because O'Neill, et al. have a priori ruled out purpose. This type of boundary condition does not function

as rules of rightness for the ecosystem. Therefore, "ecosystem health" is logical nonsense in these cases, in the same way that it is incoherent to consider the failure of thunderstorms or flames. Therefore, if O'Neill, et al.'s concept of hierarchy is actually the boundary conditions of ecosystems, then ecosystem health is logically incoherent.

However, O'Neill, et al. acknowledge that a certain subset of hierarchical systems are cybernetic (purposive), and that viewing ecosystems as cybernetic in certain space-times scales will advance our understanding of ecosystems. However, they believe that this approach has limited application. Cybernetics for them is neither the fundamental nor sole organizing principle of ecosystems. They claim,

Some observation sets are usefully explained in terms of a system that organizes and controls itself. When one is dealing with such an observation set, the cybernetic paradigm is valuable. But this is a far cry from saying that ecosystems are cybernetic. This implies that there is some scale-independent "thing" out "there" that is self-controlled. It is not certainly true that ecosystems viewed at any of the possible scales of observation will always be self-controlled.⁵⁹

This response also makes ecosystem health logical nonsense. Like Costanza, et al.'s formulation of a system, O'Neill, et al.'s view also does not recognize the distinction between the two types of boundary conditions. This is not to imply that O'Neill, et al. do not

acknowledge that some systems are purposive. In fact, within general systems, they recognize that some systems are "organizations", and some organizations are self-regulating.⁶⁰ But their criteria for identifying the two types of systems is not correct. O'Neill, et al. use Denbigh's distinction between orderliness and organization.⁶¹ Orderliness is a measure of how well each specimen approximates an ideal, while organization is more difficult to define, and is not orderliness. Orderliness applies to crystals, while organization applies to organisms, societies of organisms, and nonliving structures created by organisms. Organization is difficult to define, but it does include the following properties: It is something that exists independently of specific components. For example, individual trees die, but a forest continues. Or, cells are replaced within an organism, but the organism continues. The components of an organization are interdependent. A social insect separated from its community does not live very long. An organ removed from an organism does not continue to function very long. Organizations seem to have a function.

Within the class of systems called organizations, some of them are self-regulating (cybernetic) in O'Neill, et al.'s view. When faced

with a changing environment, a cybernetic system will make certain changes within its structures or processes which are designed to maintain their overall structure and/or function. These organizations are self-regulating by means of feedback loops which exchange energy as well as information. For example, humans self-regulate their body temperature. One of the responses to getting cold is to initiate shivering. There is first an exchange of information. The body passes information that it is cold. Then shivering is initiated. This flexing of muscles generates heat which helps warm the body. This is an example of a feedback loop. Once the body returns to normal temperature, then the feedback system discontinues the shivering. The feedback loop maintains the body temperature in an environment where the temperature is changing.

By accepting Denbigh's distinction between orderliness, a measure of how well each specimen approximates an ideal and organization which is something else that ecosystems have, the possibility of a typological classification system is ruled out. According to Denbigh's classification, ecosystems either possess organization, or they have an ideal. They cannot have both. To deny that ecosystems have organization as well as an ideal, or are driving

to achieve an ideal, rules out the possibility of a typological classification scheme. The necessary result of ruling out the possibility of typological classification is that it makes ecosystem health incoherent because it rules out the possibility of dual control boundary conditions which serve as the ideal for the class.

To illustrate that this is the case, we will first consider the classification of crystals. Then we will examine the classification of biological organisms. My objection is that by making a distinction between orderliness and organization, the grounds necessary for making health a coherent concept are destroyed. The classification of crystals is a hierarchical system based on their ordered aggregates. The patterns are not observed in the separate atomic parts; however, the 230 possibilities are determined a priori from a geometrical analysis. The ideal classes are found by assuming that the approximate plane surfaces of crystals are geometrical planes which extend to the straight edges in which the planes must meet, thus binding the crystal on all sides.⁶² This formulation defines the possible theoretical shapes of the crystals. The theoretical classification scheme determines which features are regular, and it is required to fit the facts of experience; otherwise, no matter how

widely the particular crystal deviates from the theory, this will be put down as a shortcoming of the particular specimen and not the shortcoming of the theory. The theory can never be falsified by any particular individual specimen. Facts which are not described by the theory create no difficulty for the theory, for it regards them as irrelevant to it. They are imperfections in the particular crystal. The 230 classes are geometrical statements. They satisfy the axioms of the theory. The theory says nothing definite about experience, but it potentially bears on experience, for there is always the possibility that experience will present us models for a geometrical theory.

The relationship of the theory to experience in the case of classifying crystals is that, in some sense, the theory transcends the experience to which it applies. But as in every form of idealization, it renders the theory irrefutable by experience. This does not mean that there is no appraisal of the classification scheme. If the classification system is not found apposite, it will not be popular. The theory must be judged intellectually and not directly by experience.

Therefore, according to such a classification system, the 230 different possible shapes represent the possible ideals to which any

particular crystal is aiming to achieve. The classification system is used to establish which shape a particular crystal is becoming. Once a specimen is determined to be a member of a particular class of crystals, the ideal shape for that particular class becomes the ideal shape to which the particular specimen is aiming. Denbigh acknowledges that this is the nature of classifying crystals, and orderliness is the concept by which he captures this aspect of this classification system. I agree with this portion of Denbigh's analysis. My difficulty is that Denbigh then goes on to claim that organization is distinct from orderliness. By making this distinction, Denbigh is claiming that biological systems which have organization do not have orderliness. This rules out the possibility of typological classification and with it dual control boundary conditions and the notion of health.

The classification of living beings is similar to the classification of crystals. Organisms are classified according to their ideal shape; however, the distinction between the classification of crystals and the classification of organisms is that with crystals, the classification was an a priori axiomatic geometrical scheme. In biology, the shapes

are determined empirically. The classes are not deduced from an a priori geometrical system; they are determined piecemeal.⁶³

Polanyi outlined that new species are discovered first by an uneasy feeling that something is not right.⁶⁴ Once a new species is established, it is usually defined by the presence of certain distinctive key features; however, the initial uneasy feeling was not caused by the presence of key features. It was the realization that this specimen should be appraised according to another scale of merit appropriate to it. The key features came later. Whenever a specimen of a particular species is appraised by the taxonomist, the standards of normality are modified. At the same time, the individual is appraised to determine if it is normal or not. The typological classification scheme establishes the ideal shape for the particular species. Like the example of machines we examined in the last chapter, without considering the operational principles of machines, it is not possible to identify an individual machine as a member of a particular class of machines. Likewise, without considering the operational principles of biological organisms, i.e., what shape they are trying to achieve, it is not possible to identify a particular specimen as a member of a like group of organisms.

Without the typological classification scheme, how by experience alone would we ever decide that a particular specimen was a member of a particular group? How, in spite of differences, do we decide to judge some features to be the same and at the same time judge other features which are similar to be different?

Polanyi argues that we create a gestalt and that we have focal awareness of the comprehensive organisms, while we have subsidiary awareness of the parts. If organisms do not approximate an ideal gestalt, how is it that we can identify a particular organism as a particular species? For example, we all can determine with little or no difficulty whether a living being is a human being. How can we do that unless we have a gestalt and recognize that a particular specimen has some deficiencies, but it is nonetheless striving to achieve a human shape? We cannot devise a definition which would fit all examples, so how can we accomplish it? Polanyi argues that by experience alone, without a commitment to the belief that there is a human type, we could never identify an individual as a human being. It would be impossible. Yet we do it with little or no effort, and we can not specify how we do it. Acceptance of Denbigh's distinction between orderliness and organization and the claim that orderliness

applies to crystals and organization to organisms not only makes the concept of health problematic by ruling out typological classes, but it also destroys our ability to identify a specimen as a member of a species.

To clarify how acceptance of Denbigh's distinction destroys the possibility of the concept of ecosystem health, consider a human being as an ecosystem. A human being has a hierarchical structure and recognizable boundaries. It meets O'Neill, et al.'s conditions for being an ecosystem. It is a system with component subsystems. It has organization because it has processes occurring at different rates. Some of the processes are cybernetic, but others are not. Now following O'Neill, et al.'s line of reasoning, it is an error to argue that humans are cybernetic because only some processes are purposive (cybernetic) at particular space and time scales. It makes no sense from their perspective to argue that humans are cybernetic because the same processes may not be cybernetic at different space and time scales. To focus on one set of observations over another is biased.⁶⁵ We must not focus on one way of observing the natural world.⁶⁶ There is no end point, telos, or standard of health. We can only look at various processes and say that some processes are

cybernetic at some space and time scales. To do otherwise is to imply that there is some scale independent "thing" out "there" that is self-controlled.⁶⁷ The conclusion of our investigation of human beings as ecosystems is that some processes are cybernetic while others are not. Following O'Neill, et al.'s line of reasoning, it makes no sense to consider human beings as cybernetic. It is not the case that ecosystems viewed at any of the possible scales will always be self-controlled.

The implication of this last statement for O'Neill, et al. is that all of our descriptions are relative. They depend on a particular set of instruments and a particular way of looking at the system. Because we do not have all possible measuring instruments, we must never consider that any description is absolute or necessary.⁶⁸ When we consider human beings as ecosystems, it is easy to see that any standard of health is destroyed, and health becomes problematic. My objection is that this conceptual model makes ecosystem health problematic from the start. Therefore, without considering the purpose, and only considering the physical/chemical processes, it destroys our ability to identify the types. And without types, health is incoherent.

Many ecologists currently accept a hierarchical systems view of ecosystems along the lines of O'Neill, et al. In this view, there are a minimum of a priori assumptions made about the ecosystems and their make-up. The characteristics of a particular ecosystem need to be empirically determined by examination of the proper space and time scale. This view does not necessarily make ecosystem health coherent. While it is a hierarchical theory, it is not a hierarchical theory of the right kind to make ecosystem health coherent. Ecosystems must be viewed as having a type for health to be coherent. If types are a priori ruled out, then health is also a priori ruled out.

The current systems approach accepted by most ecologists, including O'Neill, et al. and Costanza, et al., conceptually rules out the possibility that ecosystem health is coherent. If ecosystems are reducible to inanimate holistic systems like thunderstorms and flames, then logically the concept of ecosystem health is incoherent. It is the perspective of this study that ecosystem health may be incoherent; however, the concept should be examined and critiqued, rather than rejected a priori. In the next section, the concept of ecosystem health will be examined using the framework developed

in this study. The goal is to attempt to determine if ecosystems are like organisms and machines.

Is Ecosystem Health a Coherent Concept in Ecology?

We are now in a position to address the central question in this study, "Is ecosystem health a coherent concept in ecology?" To resolve this debate over ecosystem health, philosophers and ecologists must explicate and critique the basic philosophical commitments which underlies ecology and correct the commitments which are in error. This is no easy task. The starting point for this study was Michael Polanyi's observation that machines and living organisms were not reducible to the inanimate laws of chemistry and physics. Therefore, a hierarchical theory of nature must be assumed. The implication is that objects can have a different number of hierarchical levels of control. Flames and thunderstorms only have one level of control, the laws of chemistry and physics, while machines and organisms have two or more levels of control. The concept of health only applies to objects which have two or more levels of control. The goal here is to begin to flesh out the nature of ecosystems. In this section of the study, I argue that ecosystems

have dual control boundary conditions, and the concept of health can be applied to these systems.

The line of reasoning in this section is to first establish that drainage basin ecosystems exist as entities within the landscape, then I address the question, how many levels of control these systems have? After establishing that ecosystems are in some sense entities on the landscape, the question is asked if ecosystems are organisms. If they are, then ecosystem health is a coherent concept analogous to human health. I argue that ecosystems are not organisms, but I also show that this does not rule out the possibility that the concept of health applies to ecosystems. Ecosystems are only required to share the vegetative level of control with other living systems to be dual control systems. Ecosystems do share this level of control with other living objects.

The first question is, do drainage basin ecosystems exist as entities? If they have recognizable boundaries, then they can in some sense be construed as entities. Drainage basins are portions of the landscape which can be delimited by their watersheds, i.e., the ridges which serve as boundaries for the routing of water and matter and energy through the basin. Therefore, they have recognizable

boundaries. They do exist, in some sense, as entities. But it might be argued that the boundaries are not sharply delimited because animals, for instance, can easily move back and forth across the boundaries. While it is true that animals can move across drainage basin boundaries and affect the amounts and movement of material, it is nonetheless true that the drainage basin "metabolism" is largely controlled by the movement of water through the drainage basin. Even in organisms, bacteria and viruses move back and forth into and out of the organism. So the argument that animals moving across drainage divides does not change the basic nature of these systems.

Are ecosystems organisms? If they are, then they will need to meet the necessary requirements for health to be a meaningful concept because the concept of health applies to organisms. Life is an achievement that can succeed or fail.⁶⁹ Death is failure. In the case of biological organisms, life is usually embodied in a finite set of individuals, set in space for a limited duration of time. Each individual has come into existence at a definite moment to remain alive for a certain period, after which it will die. The individual organism is a member of a class, based on a similar unspecifiable

shape and function. At its lowest level, we see achievement. Each part relies for its function and its meaning as a part of the organism, on the presence and proper functioning of a number of other parts.⁷⁰ This level is called the vegetative level. It is sometimes called an organic whole. The parts are mutually dependent for their existence on the whole functioning organism. The second level of biological achievement, morphogenesis, passes from the study of typical shapes to the science of their coming into being.⁷¹ The third level is deliberate action.⁷²

By this description of an organism, an ecosystem is not an organism. An ecosystem is not an individual in space for a limited duration of time. It is not something that comes into existence at a certain moment, lives for a certain length of time, and then dies. In order to include ecosystems into the class organisms, we would have to alter our usual understanding of the organism. Even more troubling is trying to apply the second and third level of the hierarchy of achievement to ecosystems. The notion of an ecosystem coming into being is difficult, but to attribute deliberative action to an ecosystem is clearly wrong.⁷³ Consequently, ecosystems are not organisms.

If ecosystems are not organisms, does this rule out the possibility of health being logically coherent? No. While the great majority of objects in biology have the form of individuals, all biological objects do not. For example, living objects which are not considered individuals would include viruses, germ plasm, tissues in cultures, and some plants which reproduce by budding. All these objects are living. They share with individuals the achievement of life. They share with individuals the lowest level of achievement, what Polanyi calls the vegetative level. Since all life can succeed or fail, these objects have dual control boundary conditions. They have operational principles which serve as rules of rightness. At the vegetative level, they have rules of rightness, specific for their type. It is on this level that health of the ecosystems would depend. Therefore, it is logically possible that even though ecosystems are not organisms, health can be a logically coherent concept. The crucial question is whether ecosystems exist as a hierarchical systems with dual control boundary conditions. Are they systems which can succeed or fail? If ecosystems are systems which can succeed or fail, then they are members of the class of objects that have operational

principles which harness the laws of chemistry and physics to accomplish their purposes.

Do drainage basin ecosystems share with other living objects the vegetative level of control? I believe that they do. At the vegetative level, each part relies for its function and for its very meaning as part of the entity on the presence and proper functioning of a number of other parts.⁷⁴ It is at this level that growth and maintenance of life are maintained. An entity is alive as long as the parts (organs) function in a joint operation. Life is a strategem, where each part must rely on other elements to support it and accomplish the overall purpose of the entity.⁷⁵ It is this aspect which makes life irreducible to the inanimate laws of chemistry and physics. While it has been successfully argued that biological organisms have operational principles like machines, I would argue the same is true for an ecosystem.

To begin, let us return to Costanza, et al.'s discussion of ecosystem health. They defined the concept of "health" in terms of sustainability, which is a function of activity, organization, and resilience. For them, an ecosystem is healthy and free from 'distress syndrome' if it is stable and sustainable. It must be active and

maintain its organization and autonomy over time and be resilient to stress. While this definition does seem applicable to ecosystems, when we substituted a flame or a thunderstorm for an ecosystem, the definition became problematic. The reason that the argument is accepted for ecosystems is because these systems can succeed or fail. Sustainability is an attribute of a system which has operational principles. They have rules of rightness. To consider the health and sustainability of a thunderstorm is problematic. These systems have no operational principles which sustain them. We cannot make sense of the notion of a thunderstorm being stressed and suffering from 'distress syndrome'. Thunderstorms are purely the product of the laws of chemistry and physics. Most ecologists and philosophers, unless they are heavily burdened with prior philosophical commitments, acknowledge that Costanza's, et al.'s definition of ecosystem health seems reasonable, but it is nonsense when thunderstorms are substituted for ecosystems. They argue that the definition should apply to any complex system. But it does not. Therefore, we recognize that ecosystems have operational principles like other forms of life.

One aspect that makes it difficult to accept that ecosystems have operational principles like machines or organisms is that success and failure do not seem so clear-cut with ecosystems as it is for machines or organisms. When a machine breaks, it is clear it has failed. When an organism dies, it is clear it has failed, but when an ecosystem fails it is not often so clear. Ecosystems, even severely degraded ones, still have communities of organisms. Although, the community of organisms may be reduced to bacterial species which can live in reduced, anoxic conditions, there are still biological organisms functioning as a community. There is still a functioning ecosystem, even though we would consider it unhealthy. It would be hard to imagine an ecosystem that we would consider dead. Only extreme possibilities like the blast zone of a nuclear explosion would possibly be totally devoid of life.

Another reason that makes it difficult to envision a dead ecosystem is as biologists, we have paid little attention to the purposes of such systems. We have been taught never to think of purpose. If we understood the purpose of an ecosystem it might be easier to conceptualize a failed system. It would be a study in its own right to address the question of the purpose of ecosystems. This

subject has recently received little attention, and it is beyond the scope of this study to investigate the purpose of ecosystems, but there are some things which we can say about it.

It is probable that failure of an ecosystem is different from failure of an organism. When some operational principles do not continue to operate, death of the individual occurs. Like machines, when the operational principle ceases, that level of control disappears and only the inanimate laws of chemistry and physics remain. In both of these cases, the result is simple and straightforward. However, in the case of biological organisms, it is not always the case that failure of the whole organism is at stake. We saw that organisms have the capacity to instantaneously change operational principles ("equipotential"). For example, when an insect loses a leg or when a cell is moved during early development it develops into a different organ depending on where it is located. Likewise with ecosystems, there may be a series of possible operational principles, somewhat analogous to the "equipotential" capacity of organisms to change their operational principles in the face of changing conditions. A healthy ecosystem would be a system which has a large number of possible operational principles that can

sustain its purpose at a high level. Such an idea is not unprecedented in ecology; Charles Warren viewed an ecosystem as having a capacity, i.e. the ability to develop into a number of specific performances depending on the environment.⁷⁶ In his framework, a healthy system maintains the potential to develop into a wide number of potential states depending on the environmental conditions. Such a conceptual view has some similarity to a large number of potential "equipotentials".

It is difficult to give a simple example where an operational principle ceases in an ecosystem and the system dies (fails). Such a simple straight-forward case may not exist for ecosystems because they do not exist as individuals. However, that does not preclude ecosystems from having dual control boundary conditions. But it does raise a serious question about whether ecosystems can have unique and specific operational principles. We saw in organisms that operational principles established rules of rightness for classes of organisms. It raises the issue whether or not ecosystems exist as type classes.

In the case of drainage basin ecosystems, do types exist? For example, in biology, the type could be like species of organisms, such

as coho salmon, chinook salmon, or cutthroat trout. It is membership in such a class that determines the rules of rightness or the standard of health for the particular individual. In typological classification, the individuals exist as kinds, i.e. all members of a particular class share the same features as other members. They all are just as much a member of the class as any other member. Do such classes exist in drainage basin ecosystems? The usual attempts at such a classification scheme for drainage basins takes the regional approach. Drainage basins are classified according to common climatic or vegetative patterns.⁷⁷ The regional classification may then be divided into a number of subclassifications. This is a hierarchical classification scheme modeled after biological classification. This is the kind of classification scheme which is necessary for our ecosystems to be hierarchical systems at the regional level. If such a scheme is viewed as false or inappropriate for these systems, then ecosystems may not exist as a set of specific types, like organisms. The classification of ecosystems might simply consist of a few very general types like humid or arid.

But how can we decide if our ecosystems have types?

According to Michael Polanyi, the only way is to be committed to the

belief that drainage basin ecosystems are characterized as having these types. If it turns out that such a classification scheme is not true, we will sooner or later give up the commitment that they are of such types. There is no empirical test that can be performed. Like the example of classifying crystals no empirical test can be devised to test the classification system because the imperfections are viewed as imperfections of the particular crystal and not an imperfection of the theory. According to Polanyi, it is the nature of a typological classification system that they are not directly testable. It is also the nature of health that it is one of the measures which makes the classification system as a whole directly untestable, because the standard for the class of organisms is used to measure the achievement of the particular individual in question. It is not only the classification system which is tested, but simultaneously an appraisal of the particular organism according to the standard for the class also occurs. The failure may be at the level of the particular individual. This is the nature of classifying systems which have dual control boundary conditions.

In the organismic theory of Clements early in the century, portions of the landscape were viewed as types. They were

vegetative types called formations. The first major challenge to the notion of types was Gleason's research which claimed that communities were only an assemblage of organisms, and they were not types of communities. Since Gleason's challenge, much of the focus of ecology has been away from the idea of communities or ecosystems as types. Since about the 1950's, ecology has generally shifted away from the notion that ecosystems exist as types but the idea is still alive in ecology. For example, Franklin and Dyrness in their *Vegetation of Washington and Oregon* divide the Northwest vegetation into a number of characteristic classes.⁷⁸ These classes are based on zones of characteristic vegetative communities within Oregon and Washington. If these vegetational classes in some sense exist as typological classes, then there would be a basis for typologically classifying drainage basin ecosystems. Franklin and Dyrness do not claim that these vegetative communities are typological classes, nor do they deny that they are typological classes. The question whether the vegetative communities in the region are typological classes or not is crucial for the debate about ecosystem health. If the vegetation or some other aspects of drainage basins are not typological, then this study would suggest that health applies

to ecosystems in only a very general way. But if typological classes exist, then more specific operational principles exist for the particular classes at the level of the specific class.

Another related endeavor within ecology which aims at a typological classification scheme is the attempt by the U.S. Environmental Protection Agency to divide the U.S. into ecoregions.⁷⁹ Again, if ecoregions exist as kinds, then there is a basis for health being a coherent concept in ecology because the ecoregions would represent the typological classes. With these typological classes, then the observer's judgment of rightness will operate simultaneously on two levels. On the higher level, the individual drainage basin can be recognized as a member of a class, and at the same time the ideal of the class can be refined, while on the lower level, the individual is appraised to see how well it achieves the ideal.⁸⁰

If some form of regional typological classification scheme such as the above two examples does not exist, then the necessary conclusion is that no specific ideal kinds of ecosystems exists within a region. If no kinds exist, then there is no standard of health for these ecosystems as a class. Ecologists cannot coherently claim ecosystems are not typological classes and yet claim there is a

standard of health other than the individual watershed. For example, Lichatowich, et al. propose such a strategy built on each watershed as an individual.⁸¹ However, I do not believe that any biologist would want to claim that watersheds in the Pacific Northwest do not have a set of common features, nor that in some sense there are common features within areas within the region. For example, if a series of photographic slides from throughout the region were projected and field biologists were asked to identify as close as possible the locations of the watersheds, I believe that biologists could easily describe the general locations of each slide within the region. This suggests that some sort of classification scheme is applicable to these watersheds at the regional level.

I do not believe that Polanyi is entirely correct that there is no empirical test which can help determine if a typological classification scheme is correct or not. I believe that classification of drainage basin ecosystems can be examined by checking the nature of the boundary conditions between proposed classes of ecosystems. If the boundaries are discontinuous then typological classes exist. If they are continuous then they do not. However, such a test is not easy to conduct. There are a number of difficulties. First, what criteria are

measured to determine the classes? Some criteria might be continuous like climate variables while others like plants community distributions might not be? How do we decide which are the right variables to use?

Even if we get the right variables, we have to determine a method of detecting whether the boundaries are continuous or not. As we previously discussed, it is not always easy to determine if some distributions are continuous or not. For example, Clements argued that the discontinuous boundaries of the prairie formations were statistical and based on quantitative measures. The appearances to the eye were deceptive. Only by careful measurements could the discontinuous boundaries be discovered. If we decided on a standard measure, we are "still not out of the woods" for the spatial frequency of sampling will affect whether a discontinuity appears to exist or not. If the sample spacing is wider apart it will tend to give more of the appearance of a discontinuity than if the sample spacing is narrower. We need to determine a reasonable sample spacing to determine if the boundaries are discontinuous or not.

If we could decide which criteria to measure and determine a reasonable sample spacing, that would still not necessarily definitively answer the question. While this study has shown that a continuous all-encompassing theory of nature is ruled out, there are two competing "hierarchical" theories left. In one of those "hierarchical" theories of nature, objects which differ by type, have unique features which no other class of object has. According to this theory, objects would be discontinuously distributed along quantitative axes. However, in the other all-encompassing "hierarchical" theory of nature, objects which differ by type, differ by amount of some key feature. Objects would appear to be distributed continuously along an axis or axes but at a certain point a threshold is crossed and a qualitative change occurs. Therefore, the outcome of the test might be dependent on the philosophical commitment of the ecologist to a particular theory of nature with regard to watersheds. In order for the test to be correct, the correct theory of nature would have to be held. It is outside the scope of this study to determine which all encompassing "hierarchical" theory of nature is correct. But such a determination would have to be made in order to resolve this issue. I do not believe that looking at even a

large number of drainage basin ecosystems will resolve this issue. So the question, "do drainage basin ecosystems exist as type classes?" cannot be answered by this study. While drainage basin ecosystems do appear to have dual control boundary conditions, specifying the specific operational principles is not possible because classification of the various basins into classes remains problematic. Without such a classification scheme, it is not possible to take the investigation any further. While a few schemes have been proposed, they do not have broad general support at this time. Further discussion of the specific operational principles must wait until classification of these ecosystems is accomplished.

To summarize the conclusions of this chapter so far: If we follow Tansley's lead and view ecosystems as physical systems, the concept of ecosystem health is incoherent. If we accept Golley's assessment that Tansley accepted the mechanistic, reductionistic Cartesian science as his philosophy of science, then ecosystem health is problematic. Tansley's concept does not recognize the possibility of dual control boundary conditions. An ecosystem is a physical and chemical system. Therefore, ecosystem health is nonsense if we accept Tansley's concept of an ecosystem. There are many ecologists

today who would not accept a Cartesian philosophy of science who would nonetheless agree with the Cartesians that we ought to view ecosystems as physical chemical systems with no teleological commitments. This study has shown that the fate of such a view is to also render ecosystem health incoherent. Costanza, et al. and O'Neill, et al. both fall in this category. As ecologists and philosophers, we cannot on one hand claim that ecosystems are open systems like thunderstorms and then turn around and claim that ecosystems are systems that can succeed or fail. One or the other commitment has to be given up. Either ecosystems can fail and health applies or they can not fail and health does not apply. Either ecosystems are like machines and organisms or they are like fire and thunderstorms. These are the two options. Many of the major players in the current debate reject the idea that drainage basins are "hierarchically" controlled systems, but they argue that in some sense they are systems that can succeed or fail. If the analysis of this study is correct, the position of a large number of the major players in this debate is incoherent.

I have argued that drainage basin ecosystems have dual control boundary conditions. They are systems which can succeed or

fail, and ecosystem health is a coherent concept. However, ecosystems are not organisms. They do not exist as individuals in time. The classification of ecosystems is problematic at this time so further discussion of the operational principles of drainage basin ecosystems must wait until the classification is completed.

This study also suggests that in order for a regional classification scheme to exist, a view of ecosystems similar to Clements' view of communities would need to be accepted for "ecosystem health" to be coherent at the regional level. Clements took a hierarchical approach and was attempting to regionally classify plant formations on a model based on biological taxonomy. If such a classification is real and not spurious, then there are typological classes of objects. This would make it possible to regionally develop operational principles. Clements' project was also centered around the notion that the succession of plant communities was deterministic and aimed at a final stage, called the climax community. From Clements' perspective, attainment of the climax stage was the achievement toward which the succession of plant communities was headed. Each regional kind of climax community had its own unique standard of achievement. Such a system can

succeed or fail. Such a system has dual control boundary conditions. Clements drew a parallel between the development of an individual of a species and the succession of a plant community. The framework of this study indicates that Clements' theory would also make ecosystem health coherent. However, our analysis indicates that Clements' plant communities are not individuals. They are not organisms. Clements was careful himself to claim that communities are not organisms. They are like organisms. This study suggests that Clements' organismic theory is not entirely wide of the mark; it is correct that ecosystems are the kind of systems that can succeed or fail.

This is not to say that this study is suggesting that we must accept his organismic theory or health is incoherent. Clearly, Clements' theory does not adequately consider disturbances and their role in structuring the landscape. Nor does Clements's theory really address the dynamics of ecosystems given that they are not individuals. What this study suggests is that several major components of his theory are consistent with what must be true for "ecosystem health" to be coherent at the regional level. If we sympathetically read Clements's theory to be saying that

vegetational community types are not individuals but they develop trajectories, then his theory is far more reasonable. He used the term complex organism to distinguish these types from organisms. I am not advocating that ecology go back to Clements's theory. I believe that much of his theory was incorrect; however, this study which began from a different perspective than Clements's theory suggests that major components of his view are the major premises necessary for "ecosystem health" to be coherent. If ecosystems can fail, if we can restore them, then these assumptions must necessarily be held. We can no longer claim that ecosystems are like thunderstorms or flames and talk about "health" and "restoring" ecosystems.

This study, which extended Michael Polanyi's framework to ecosystems, provides a powerful tool for investigating the question if ecosystem health is coherent or not. Given that few ecologists these days share Clements' philosophical commitments, it suggests that for most ecologists, ecosystem health is nonsense. Ecosystems are like fires and thunderstorms rather than machines and organisms. That means these ecologists must give up the notion of restoration and

health. Both these concepts are empty, given their philosophical commitments.

Clarification of Position of This Study with Regard to the Current
Debate in Conservation Biology

To help clarify the results of this study, I will now relate the conclusions to the current debate about ecosystem health. To accomplish that, I will return to the five objections raised by the opponents of ecosystem health and interact with them.

1. Ecosystem health has never been clearly defined. Therefore, we can not evaluate such a concept.

It is true that ecosystem health has not previously been adequately defined.⁸² It is certainly also true that a great deal of this debate has resulted from a lack of a clear understanding of the concept of health. It would certainly aid the debate to clearly define the major terms of the debate. This study, following Michael Polanyi's lead, suggests that health is the level of achievement judged from a standard of rightness established for that class of objects. The concept of health does not apply to open, dissipative

systems. Such a definition clarifies much of the ambiguity surrounding health.

2. The level of organization called an ecosystem does not exist. Only a random assemblage of organisms exists which are loosely organized into communities. Therefore, ecosystem health is an empty concept.

If the level of organization called the ecosystem does not exist, then clearly the concept of health of said system is incoherent. The key question is, is there a level of control above communities? If so, then ecosystems exist, and if they have dual control boundary conditions, then ecosystem health is logically coherent. It is not necessary that the ecosystem exists as an individual. It only has to exist at the vegetative level. If Gleason is right and only a random assemblage of organisms exists, then there is no level of control above populations. If that is true, then the concept of ecosystem health is incoherent above the level of populations. Since World War II, Gleason's general position has largely been accepted by many ecologists.⁸³ For those ecologists, it is not coherent to talk about success or failure at a level of biological organization higher than the

population. These ecologists cannot coherently consider restoration of drainage basins either.

This study contends that the level of organization called the ecosystem does exist as a level of control. There is a sense in which a level of control exists above the population level. Species are dependent on other species in a way that transcends the species level. For instance, "consumers" are dependent on primary producers in the ecosystem. The function of the group of species called primary producers is to produce the food for another whole functional set of species which are dependent on their food. In the next section, I will further argue that ecosystems have dual control boundary conditions. These boundary conditions are the level of control above the species level which harness the species interactions to accomplish the purpose of the ecosystem.

3. The level of biological organization called the ecosystem exists but not as an organism.

Here, the opponents accept a hierarchical theory of nature, but they have identified a discontinuity between ecosystems and organisms. The conclusion of this study is that this objector is correct in assuming that nature is hierarchically structured. They are also

correct in that there is a discontinuity between organism on one side and ecosystems on the other. Ecosystems are not organisms.

However, these premises do not necessarily result in the conclusion that the concept of health does not apply to ecosystems. This study is in complete agreement with a subset of these objectors who argue that ecosystems are not organisms, but the concept of health of ecosystems is nevertheless coherent. Ecosystems do not exist for only a period of time. They are not born, nor do they die like individuals. They also do not have intentions. They are not purposive like biological organisms above earthworms. However, this does not rule out the possibility that health applies to ecosystems similarly as health applies to humans or organisms. Ecosystems share the vegetative level, i.e., the achievement of life, so they also share the characteristic of health. In this view, the analogy between medicine and ecology applies only at the level of vegetative function. It does not apply at other levels. This suggests that the analogy between medicine and ecology is of limited utility.⁸⁴

4. The concept of health, in itself, is problematic in science. Health is a normative concept, which recognizes certain states as desirable. Therefore, the concept of health goes beyond description

and fact. Also, what scientific criteria can be used to establish the standard of health? Therefore, health is not a scientific term.

This is the likely response of a logical positivist or someone highly influenced by a positivistic philosophy of science. Michael Polanyi's whole project was primarily a rejection of this view. His critique would consist of much of his published work. Polanyi showed that health was incoherent from this perspective.

5. The concept of ecosystem health is dangerously related to teleology and contradicts Darwin's evolutionary theory. Therefore, the concept of ecosystem health is problematic.

Opponents are not exactly correct in claiming that ecosystem health as developed in this study is dangerously related to teleology. Their objection ought to be that ecosystem health is teleological. This study has shown that without teleology, not only is health incoherent, but we could not identify any objects which have dual control boundary conditions as members of a class. This includes biological organisms. Without a teleological perspective, biology and physiology as we know it are not possible. Biology requires acceptance of a hierarchical theory of nature. Polanyi argues that the main stumbling block to accepting a teleological view and a

hierarchical view in biology is not religion, but the Cartesian commitment to the reduction of biology to the laws of inanimate chemistry and physics.⁸⁵ It is largely the Cartesian commitment to their continuous theory of nature which creates the difficulty. The ultimate explanation must lie at the atomic level. Any claim to purposive action at another level is vitalistic. It is anathema to the Cartesian project. Francis Crick for example explicitly makes this argument.⁸⁶

The opponents of ecosystem health who argue that the concept of ecosystem health contradicts Darwin's theory of evolution are correct. It is true that one cannot coherently hold Darwin's theory of evolution and accept the existence of dual control boundary conditions in biology. Darwin's theory of evolution requires a continuous theory of nature. Darwin's theory of evolution can be viewed as the culmination of the Cartesian project by making biology compatible with Cartesian science. The major question that Darwin set out to answer was, "Why does it appear that animals are divided up into a large variety of kinds, and how did these kinds originate?" Darwin's solution was that organisms only appear to form kinds. Organisms actually differ from each other by degree. They are

actually only apparent kinds. Organisms originally are continuously distributed until natural selection removes some of the variations, then they appear to be discontinuously distributed; however, they are actually continuously distributed. Therefore, there are no typological classes of organisms, only a large variety of organisms continuously distributed in nature. According to Darwin's view, nature makes no jumps. Or said another way, there are no discontinuities in nature. Until Darwin proposed this solution, biologists had not been able to explain how species were divided into kinds, discontinuously distributed, if a continuous theory of nature was accepted. Therefore, Darwin's theory provided the explanation which allowed biology to rest on the philosophical foundations compatible with Cartesian science. This is Darwin's triumph.

To show more clearly Darwin's reasoning and his acceptance of a continuous theory of nature, examine Darwin's argument in The Origin of Species.⁸⁷ In his introduction, he lays out what he considers to be the core of his theory:

I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists entertain, and which I formerly entertained—namely that each species has been independently created—is erroneous. I am fully convinced that species are not immutable; but that those belonging to what are called the same genera are linear descendants

of some other and generally extinct species, in the same manner as the acknowledged varieties of any species are the descendants of that species. Furthermore, I am convinced that Natural Selection has been the main but not the exclusive means of modification.

Darwin begins his argument by examining variation within domestic species and expanding the discussion to variations under natural conditions. The discussion of variations under natural conditions paralleled the discussion of variation of domestic species. Darwin began his discussion of variation under natural conditions by claiming that he would not define a species, although he believed that every naturalist knew vaguely what one was. For Darwin, the distinction between species and variations is vague and arbitrary. Species are simply strongly marked and permanent varieties.⁸⁸ (It will subsequently be seen what Darwin means by permanent). Variations occur within species, and some variations have a better chance of surviving than other variations. Natural selection, commonly termed "survival of the fittest," is seen as a mechanism preserving the favorable variations and destroying the injurious variations. Therefore, varieties slowly develop into independent and permanent species. Nature makes no big leaps. As Darwin saw it, species arose from closely related but earlier species.

Darwin could have ended his argument here. He had accomplished all that he set out in the introduction to prove. The traditional view was that species were considered to be fixed and immutable. Darwin had shown that they were not.

In later chapters of his book, he argues that the same mechanism functions over greater time scales and is the driving force for the development of higher biological groupings such as classes.⁸⁹ Darwin claims that his theory is analogous to Lyell's uniformitarianism in geology: species slowly develop from variations by natural selection in the same way that the coast cut the valleys and cliffs.⁹⁰ Darwin's theory is grounded on the assumption that all differences in biological classes can ultimately be reduced to processes forming species.

Specifically, what Darwin has in mind is a biological analog of uniformitarianism, which claims that geologic processes operating at modern-day frequencies and magnitudes extrapolated can account for the landforms that we see. All we need is sufficient time. So, when Lyell looks out and sees the waves crashing onto the coastal cliffs of England, he can envision the present shape of the cliffs as the result of thousands of years of waves crashing into the shoreline at

their current strength. Analogously, Darwin sees that some variations have greater chances of surviving than other variations, and the species we see today are the result of natural selection operating over long periods of time. The belief serving the foundation of uniformitarianism is that nature is continuous, i.e. that processes we see operating at short time scales like a day or year can be extrapolated to much longer periods of time. These processes operating on the scale of a day or year can account for the shape of the coastline given enough time. If nature is not continuous, then the foundation of uniformitarianism and Darwin's theory of evolution is fallacious. If nature is hierarchical, then the assumption that modern-day processes may be extrapolated over long time scales is false.

Darwin's theory of evolution is a fallacious argument if nature is hierarchical, and processes are being extrapolated to different levels of the hierarchy. For Darwin has assumed that processes operating on the level of variations are the same as processes operating on species. The only difference is the time involved. Said another way, Darwin assumes that processes forming the variations within varieties are the same as the processes forming variations

within species. The only difference is the time involved. Therefore, the processes forming the variations of species reduce to the processes forming the variations of varieties. Darwin has assumed that processes forming biological classes reduce to the processes forming biological species. Again, Darwin's argument is sound, if nature is continuous. But if nature is hierarchical instead of continuous, then Darwin must prove that the processes at issue can be extrapolated at the various time scales. Darwin never gave that proof, nor did he believe he needed to because he assumed a continuous theory of nature.

We can also see why Darwin did not see a need to define a species or a variation or any other biological group. They are all continuously defined. They are arbitrary. All biological organisms differ from one another by degree. There are no true kinds in Darwin's view. There are no real typological classes. Therefore, classes only differ from each other by degree, so they are arbitrary. When Darwin called species "permanent" variations, he did not mean by permanent, unchangeable. If he meant unchangeable, then he would be agreeing with the traditional view that species were unchangeable. Since he has set out to refute that traditional view, he

means something else by permanent. The only thing that seems to make sense in this context is that Darwin is claiming that "permanent" means unchangeable for some period of time. All these definitions are arbitrary because they are continuously defined.

A biologist who understood this clearly was G.C. Williams. In his classic book, *Adaptation and Natural Selection*, he argued clearly against any hierarchical theory.⁹¹ His specific target was group selection as developed by Wynne-Edwards, which claimed that populations were "homeostatic systems" capable of regulating their size through complex negative feedback mechanisms analogous to the physiological mechanisms in individual organisms.⁹² Williams argued clearly that natural selection at the individual level was the not only the primary mechanism of evolution; it was the also a sufficient explanation for virtually all ecological phenomena.⁹³ Here we see explicitly the rejection of a hierarchical view. All selection is at the level of the individual. Williams understands clearly the implications of Darwin's theory.

This study shows that the continuous theory of nature which underpins the Cartesian project and Darwin's theory of evolution makes health problematic in biology. Darwin's theory also rules out

the possibility of typological classification, which is based on the existence of real kinds. This study rejects this aspect of Darwin's theory. A number of other biologists and ecologists also reject a continuous theory of nature. Eugene Odum, argued explicitly that his holistic perspective of an ecosystem is based on a 'whole-before-the-part' approach.⁹⁴ This contradiction has not caused Odum's ecosystem view to be rejected by biologists. Steven J. Gould also appears to reject a continuous view of nature. He believes that at times nature makes big leaps, and we cannot extrapolate current processes over long time scales to explain the evolution of species.⁹⁵ Gould argues for a hierarchical view of evolution involving three complimentary, but different modes of change at three levels: variation within populations, speciation, and patterns of macroevolution.⁹⁶ Gould and Eldredge claim that "Higher level selection is an addition to, not a refutation of, the conventional Darwinian view of selection upon organisms."⁹⁷ The problem here is how to make good on the claim to accept a continuous theory of nature as required by Darwinian Evolution and have higher level selection occurring. A large literature exists on this problem. A clear articulation of the problem is found in a paper by David Kellogg.⁹⁸

Anyone who proposes a hierarchical scheme is proposing a theory which is likely to be contradictory to the major philosophical framework of Darwinian evolution. If the proponent of a hierarchical theory also claims to be a Darwinian evolutionist, it is likely that the claim is not a coherent one. The point here is that although this study contradicts Darwin's theory of evolution, that ought not automatically disqualify it from ecology and biology. Although this study does reject portions of Darwin's theory, that move should not necessarily cause this study to be removed from serious consideration. To reject portions of Darwin's theory is in itself a perfectly reasonable option. After all, biology and ecology do not necessarily stand or fall with Darwin's theory.

It is also true that this study is teleological. For decades, the Cartesians have relentlessly maintained that all teleological elements must be removed from biology and ecology. Teleology is antithetical to the Cartesian project. Francis Crick explicitly makes this claim. This study has shown that the logical consequences of the Cartesian project is to make the concept of health incoherent and to destroy our ability to identify individuals as members of a class. Therefore, the Cartesian rejection of teleology should itself be rejected.

Notes

¹ A. Tansley, , 'The Use and Abuse of Vegetational Concepts and Terms.', *Ecology* 16(3) (1935), pp284-307, pp284-86; F. Golley, *A History of the Ecosystem Concept in Ecology*. (Yale University Press, New Haven, 1993), p. 15.

² R.V. O'Neill, D.L. DeAngelis, J.B. Waide, and T.F.H. Allen, *A Hierarchical Concept of Ecosystems* (Princeton University Press, Princeton, 1986), p. 21.

³ H.C. Cowles, 'The Ecological Relations of the Vegetation on the Sand Dunes of Lake Michigan'. *Botanical Gazette* 27 (1899), pp. 95-117, pp.167-202, pp. 281-308, pp. 361-391.

⁴ F.E. Clements, *Plant Succession: An Analysis of the Development of Vegetation* (Carnegie Institute Washington, D.C, Publication 242, Washington, D.C., 1961), pp. 3-7; and F.E. Clements, 'Nature and Structure of the Climax.' *Journal of Ecology* 24(1936), pp. 252-284.

⁵ F. Clements, *Plant Succession*, pp.3-7.

⁶ See R.C. Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955* (University of California Press, Berkeley, 1981),p. 83 . He presents a more comprehensive discussion of the roots of Clement's views.

⁷This theory of nature was discussed in the last chapter. It is a hierarchical theory held largely by dialectical materialists.

⁸J. Phillips, 'The Biotic Community' *Journal of Ecology* 19 (1931), pp. 1-24; J. Phillips, 'Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts. Part.1.' *Journal of Ecology* 22 (1934), pp. 554-71.; J. Phillips 1935a, 'Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts. Part.2. Development and the Climax.' *Journal of Ecology*, 23 (1935) pp. 210-43; J. Phillips, 'Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts, Part 3. The Complex Organism.' *Journal of Ecology* 23 (1934) pp. 488-508; J.

Phillips, 'A Tribute to Fredrick E. Clements and His Concepts in Ecology.' *Ecology* 35(2)(1954) pp.114-15.

⁹ F. Golley, 'A History of the Ecosystem Concept in Ecology', p.24.

¹⁰ F.Golley, *Ibid.*, p. 31.

¹¹ F. Golley, *Ibid.*, p. 16.

¹²The details of this discussion are from R.C. Tobey, *Saving the Prairies*, p. 176.

¹³ Tobey points out that Tansley was a personal friend of Bertrand Russell and quite likely Russell had considerable influence on Tansley's development of his ideas.

¹⁴ R.C.Tobey, *Saving the Prairies*, p. 15.

¹⁵ F. Golley, *A History of the Ecosystem Concept in Ecology*, p. 29

¹⁶ F. Golley, *Ibid.*, p. 34.

¹⁷ F. Golley, *Ibid.*, p. 34.

¹⁸ F. Golley, *Ibid.*, p. 34.

¹⁹ H.A. Gleason, 'Is the Synusia an Association?' *Ecology* 17(1936), pp. 444-451; H.A. Gleason, 'The Individualistic Concept of the Plant Association.' *American Midland Naturalist* 21(1939), pp. 92-110.

²⁰ It is quite likely that Gleason did not understand Clement's perspective. Clement's did argue that formations existed as types and had unique features, but as we have seen that Clement's theory of nature saw the types arising from continuous distinctions. To Gleason, this perspective probably appeared as nonsense.

²¹Trophic dynamics refers to the feeding dynamics within the ecosystem. Trophic categories include producers, consumers, and reducers.

- ²² E. Odum, *Fundamentals of Ecology* (Saunders, Philadelphia, 1971) p. 8.
- ²³ E. Odum, *Ibid.*, p. 8.
- ²⁴ E. Odum, *Ibid.*, p. 9.
- ²⁵ M. Polanyi, 'The Republic of Science: Its Political and Economic Theory.' In: M. Grene, *Knowing and Being* (The University of Chicago Press, Chicago, 1969). p. 49
- ²⁶ The biosphere includes the atmosphere and down into the earth to the depth of several miles. It is the zone of biological activity of the earth.
- ²⁷ Drainage basins are often called watersheds, however, this is technically incorrect. The watershed is the ridges which define the boundaries of the drainage basin.
- ²⁸ L. von Bertalanffy, *Perspectives on General Systems Theory* (Braziller, New York, 1975). Also see the journal, *General Systems Theory*.
- ²⁹ Tobey, *Saving the Prairies*, p. 179.
- ³⁰ O'Neill et al, *A Hierarchical Concept of Ecosystems*; also B.D. Haskell, B.G. Norton, and R. Costanza. 1992. 'What is Ecosystem Health and Why Should We Worry about It?' In: R. Costanza, B.D. Norton, and B.D. Haskell (editors), *Ecosystem Health* (Island Press, Washington, D.C., 1992) p. 16.
- ³¹ D. Botkin, *Discordant Harmonies: A New Ecology for the Twenty-first Century*, (Putnam, New York, 1990); also D. Ehrenfeld, 'Ecosystem Health and Ecological Theories.' In: R. Costanza, B.G. Norton, B.D. Haskell, *Ecosystem Health: New Goals for Environmental Management* (Island Press, Washington, D.C., 1992) p. 138.

- ³² J.H. Connell, and W. P. Sousa. 1983. 'On the Evidence Needed to Judge Ecological Stability or Persistence.' *American Naturalist* 121 (1983), pp. 789-824, pp. 789ff.
- ³³ J.C. Karr, C. Frissell, T.C. Dewberry and R. Doppelt, 'Part II. Recommendations' In B. Doppelt, M. Scurlock, C. Frissell, and J.C. Karr, *Entering the Watershed* (Island Press, Washington, D.C., 1992) p. 50.
- ³⁴ D. Ehrenfeld, D, 'Ecosystem Health and Ecological Theories', p. 138.
- ³⁵ R. Constanza, et al., *Ecosystem Health: New Goals for Environmental Management*.
- ³⁶ R.V. O'Neill, et al., *A Hierarchical Concept of Ecology*
- ³⁷ Rapport, D.J., H.A. Regier, and T.C. Hutchinson. 1985. 'Ecosystem Behavior under Stress.' *American Naturalist* 125-5 (1985), pp. 617-640.
- ³⁸ R. Costanza, *Ecosystem Health: New Goals for Environmental Management*, p. 3.
- ³⁹ R. Costanza, *Ibid.*, p. 16.
- ⁴⁰ R. Costanza, *Ibid.*, p. 15.
- ⁴¹ R. Costanza, *Ibid.*, p. 15.
- ⁴² Chapter II of this study.
- ⁴³ R. Costanza, *Ecosystem Health*, p. 31.
- ⁴⁴ R. Costanza, *Ibid.*, p. 9.
- ⁴⁵ R. Costanza, *Ibid.*, p. 16.
- ⁴⁶ F. Golley, *A History of the Ecosystem Concept in Ecology*, p. 15.

⁴⁷The term mechanistic reductionistic science is taken to be synonymous with Cartesian science in this paper.

⁴⁸ M. Polanyi, and H. Prosch, *Meaning*, pp. 162-163. It is beyond the scope of this study at this point to take this discussion further. We will return to it in a related discussion.

⁴⁹ R.V. O'Neill, et al., *A Hierarchical Concept of Ecology*, Chapter 3.

⁵⁰ R.V. O'Neill et al., *Ibid.*, p. 38.

⁵¹ R.V. O'Neill et al., *Ibid.*, p. 40.

⁵² R.V. O'Neill et al., *Ibid.*, p. 39.

⁵³ R.V. O'Neill et al., *Ibid.*, p. 73.

⁵⁴ R.V. O'Neill et al., *Ibid.*, p. 37.

⁵⁵ R.V. O'Neill et al., *Ibid.*, p. 55.

⁵⁶ R.V. O'Neill et al., *Ibid.* p. 61.

⁵⁷ R.V. O'Neill et al., *Ibid.*, p. 76.

⁵⁸ R.V. O'Neill et al., *Ibid.*, p. 86.

⁵⁹ R.V. O'Neill et al., *Ibid.*, pp. 52ff.

⁶⁰ R.V. O'Neill et al., *Ibid.*, pp. 44-45.

⁶¹ R.V. O'Neill et al., *Ibid.*, pp. 44-5.

⁶² M. Polanyi, 'Life Transcending Physics and Chemistry.' p56-7; also M. Polanyi, *Personal Knowledge*, pp. 43-48.

⁶³ M. Polanyi, *Personal Knowledge*, p. 349.

⁶⁴ In Chapter II of this study.

⁶⁵ R.V. O' Neill et al., A Hierarchical Concept of Ecosystems, p. 1.

⁶⁶ R.V. O'Neill et al., *Ibid.*, p. 6.

⁶⁷ R.V. O'Neill et al., *Ibid.*, p. 53.

⁶⁸ R.V. O'Neill et al., *Ibid.*, pp. 69-70.

⁶⁹ M. Polanyi, *Personal Knowledge*, p. 381.

⁷⁰ M. Polanyi, *Ibid.*, p. 363.

⁷¹ M. Polanyi, *Ibid.*, p. 355.

⁷² M. Polanyi, *Ibid.*, p. 359.

⁷³ I am not aware of anyone claiming that drainage basin ecosystems have intentions. In some versions of the Gaii hypothesis, the biosphere (the surface of the earth from about 10 miles down to 10 miles into the atmosphere) is seen as having intentions, but I am not aware of anyone claiming that these intentions exist at the level of drainage basin ecosystems.

⁷⁴ M. Polanyi, *Personal Knowledge*, p. 363.

⁷⁵ M. Polanyi, *Ibid.*, p. 323.

⁷⁶ C.E. Warren, 1979. *Toward Classification and Rrational for Watershed Management and Stream Protection* (U.S. Environmental Protection Agency, Corvallis, 1979). This is not to say that the view expressed here is the same as C. Warren's. It is not. He defines a community as the totality of serial stages. This view is rooted in Kantian philosophy. The view expressed here is not Kantian.

⁷⁷ Probably the best example of this type of work today is the ecoregion classification scheme of J.M. Omernik, 'Ecoregions of the conterminous United States', *Annual Association of American Geography* 77 (1987), pp. 118-25.

⁷⁸ J. Franklin and C.T. Dyrness, *Vegetation of Washington and Oregon* (Oregon State University Press, Corvallis, 1973) pp. 44-51.

⁷⁹ J.M. Omernik, and A.L.Gallant, 1986. *Ecoregions of the Pacific Northwest*. (Environmental Protection Agency, Environmental Research Laboratory. EPA/600/3-86/033, Corvallis) pp. 1ff.

⁸⁰ M. Polanyi, *Personal Knowledge*, p. 345.

⁸¹ J. Lichatowich, L. Mobrand, L. Lestelle, and T. Vogel, 'An Approach to the Diagnosis and Treatment of Depleted Pacific Salmon Populations in the Pacific Northwest Watersheds.' *Fisheries* 20-1 (1995), p. 14.

⁸² R. Costanza et al., *Ecosystem Health*, began their book with the observation that "health" had not adequately been defined to be useful for the task, p. 1.

⁸³ F. Golley, *A History of the Concept of Ecosystem in Ecology*, p. 30.

⁸⁴This topic will be taken up in greater detail in Chapter 5.

⁸⁵ M.Polanyi and Harry Prosch, *Meaning*, pp. 161ff.

⁸⁶ F. Crick, *Of Molecules and Men* (University of Washington Press, Seattle,1966), pp.10ff.

⁸⁷ C. Darwin, *On the Origin of Species* (Harvard University Press, Cambridge, 1964) p. 6.

⁸⁸Darwin Ibid p. 108.

⁸⁹Darwin,C. Ibid p. 320.

⁹⁰Darwin, C. Ibid p. 142.

⁹¹ G.C. Williams, *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought*, (Princeton University Press, Princeton, 1966)

⁹² V.C. Wynne-Edwards, *Animal Dispersion in Relation to Social Behavior* (Oliver & Boyd, Edinburgh, 1962), pp. 13-18.

⁹³ G.C. Williams, *Adaptation and Natural Selection*, pp. 3-4

⁹⁴ This is an explicit acceptance of a hierarchical view. In fact this is an acceptance of Aristotle's hierarchical view. E. Odum, *Fundamentals of Ecology*, (Saunders, Philadelphia, 1959) p. vi.

⁹⁵ S. J. Gould, 'Is a New and General Theory of Evolution Emerging?' *Paleobiology* 6(1980) pp. 119-130.

⁹⁶ S.J. Gould, *Ibid.*, p. 119.

⁹⁷ S.J. Gould and N. Eldridge, 'Punctuated Equilibria at the Third Stage', *Systematic Zoology* 35 (1986) pp. 143-148, pp. 146-147.

⁹⁸ D.E. Kellogg, ' "And then a Miracle Occurs"- Weak Link in The Chain of Argument from Punctuation to Hierarchy.' *Biology and Philosophy* 3(1988), pp. 3-28, p. 23.

CHAPTER IV

CAN WE DIAGNOSE THE HEALTH OF ECOSYSTEMS? Part II.

Introduction

The first part of this study established that the concept of health applies to ecosystems. This chapter investigates the question, how can we determine the state of health of an ecosystem? Because ecosystems are members of the class of objects to which health applies, then the analogy with medicine is to some extent appropriate. However, because ecosystems do not exist as individuals, the analogy breaks down where it becomes germane that ecosystems do not exist as organisms or individuals, like humans or horses.

This chapter analyzes how a medical doctor diagnoses the health of a patient. Emphasis is placed on examining the role of skills involved in the process. Bringing the role of skills into the current debate on watershed assessment and ecosystem health may be the most valuable portion of this study. In the following chapter it will

be shown that a weakness of current watershed assessments is that they underestimate the role of skills in the process of diagnosing the health of watersheds.

The discussion begins with an examination of skill of knowing in general and then moves to the skill of a medical doctor diagnosing the health of a patient. This discussion follows Michael Polanyi's analysis of the role of skills in scientific knowledge. One of Polanyi's paradigm examples is the skill of a diagnostician. He draws on his early experience as a medical doctor. This chapter outlines the specific points where a medical doctor diagnosing the health of a patient are similar to an ecologist or conservation biologist diagnosing the health of an ecosystem. This analysis will show, in this chapter and the following one, that much of the difficulty currently being experienced by ecosystem practitioners results from not following the medical example. Identifying where current ecosystem diagnosis differs from medical diagnosis points the way to better trained ecosystem diagnosticians. Also, paying attention to how medical diagnosticians are trained will speed the ability of ecologists and conservation biologists to become good diagnosticians.

The Nature of Skills and Their Role in the Acquisition of Scientific Knowledge

Michael Polanyi argues that skills play a crucial role in obtaining knowledge in general and knowledge of the health of an individual. His project is an inquiry into the nature and justification of scientific knowledge. He begins with the observation that we know more than we can tell.¹ For example, we can recognize a person that we know from a group of strangers, yet we usually cannot say which features we used to identify them. We vaguely know, but we are not really sure. It is possible to reflect carefully on the features that we are using; however, the features are not readily conscious to us at the time that we identified the person. To pick a person that we know from a crowd is a skill. One aspect of a skill is that it is something that we do, and we have knowledge of that skill beyond what we can explicitly explain. We do not precisely know how we identify a particular person from a crowd, but we know that we can do it.

To further the examination of the skill of identifying a person from a crowd, consider that in law enforcement they have devised a method to aid in communicating a face. It consists in a large collection of pictures showing a large variety of facial shape, noses,

mouths, eyebrows, hair, etc. People can identify the particular features of the face that they know. The pieces are then put together into a composite picture of the face. This suggests that we can at least partially make explicit our knowledge of the specific features by which we identify a face; however, this does not change the fact that at the time we knew more than we could tell. We knew the face, but we could not explicitly identify the specific features by which we recognized the face. Also, with regards to the law enforcement method, we can not tell how we match the features of the face that we remember with those in the collection. This ability itself is not explicitly known to us at the time that we do it. We are not able to specify the particulars by which we identify the face.

Polanyi claims that the unspecifiability of the particulars is related to the findings of Gestalt psychology; however, his perspective is radically different from theirs. Polanyi regards knowing as,

an active comprehension of the things known, an action that requires skill. Skillful knowing and doing is performed by subordinating a set of particulars, as clues or tools, to the shaping of a skillful achievement, whether practical or theoretical. We can then be said to become 'subsidiarily aware' of these particulars within our 'focal awareness' of the coherent entity that we achieve. Clues and tools are things used as such and are not observed in themselves. They are made to function as extensions of our bodily

equipment and this involves a certain change of our own being. Acts of comprehension are to this extent irreversible, and also non-critical. For we cannot possess any fixed framework within which the re-shaping of our hitherto fixed framework could be critically tested.

²

He has recast some of the findings of Gestalt psychology as the outcome of an active shaping of experience performed in the pursuit of knowledge.³ Comprehension is the process of grasping a disjointed set of particulars and molding them into a comprehensive whole.⁴

We cannot comprehend a whole without seeing its parts, but we can see the parts without seeing the whole. Often when we are trying to solve a problem, we are aware that we have all the pieces, but we just cannot get them together right. Suddenly, in an instant, we see how they all fit together. We have comprehended the whole. This process may occur tacitly and so quickly that we are not aware of it, or it may be take considerable time and effort on our part.

Comprehension is an active skill. It is also non-critical, because in our attempts to comprehend the particulars, we are constantly inarticulately adding to our body of experience that we are bringing to the problem. Our framework is never totally explicit, nor are we aware of much of the tacit changes which are occurring as we work on the problem.

Skillful knowing or doing is achieved by observing a set of rules which are not known as such to the person following them.⁵ A skill is also an achievement. It can succeed or fail to accomplish its goal. Related to this, everything that tends towards an achievement, including bodily motions or thoughts, is classified as an activity. An activity is also something that can go wrong. In the act of attempting to attain an achievement, a particular skill can succeed or fail. For example, riding a bicycle is such a activity which takes skill. The person riding cannot tell all the rules necessary to ride. He cannot tell how it is that he rides. He does not know how it is that he balances. He knows that if he is falling to the right he must turn the handlebars to the right. The result is that the course of the bicycle is now to the right. This results in a centrifugal force which pushes the rider to the left. However, the rider only vaguely knows what to do. He is not explicitly aware of the exact rules. Riding a bicycle is also an activity which can succeed or fail depending upon one's skill.

Engineers and physicists have determined that the specific rule for balancing a bicycle is that for a given angle of unbalance the curvature of each turn is inversely proportional to the square of the speed of the bicycle.⁶ However, no one follows this rule. We do not

explicitly adjust the curvature of our path in proportion to the ratio of our unbalance over the square of our speed. We are not explicitly aware of our speed nor our degree of unbalance. Even if we could adjust the curvature of our path in proportion to our degree of unbalance and speed, we would still fall off the bicycle, for there are a number of other factors involved which must be taken into account. The rules can be helpful but they do not determine the practice of the art. In fact, a physicist could know this explicit rule and not be able to balance a bicycle. The skill of riding a bicycle cannot be reduced to a set of explicit rules which are consciously followed. It would be extremely difficult, if not impossible, to articulate all the rules for riding a bicycle, and knowledge of the rules does not ensure the ability to perform the skill. However, anyone who knows how to ride a bicycle in some sense knows the rules, yet the rules are not explicitly nor consciously known.

Since, the particulars involved in skills like riding a bicycle are not explicitly specifiable, they cannot be transmitted entirely by prescription. Only a portion of the information necessary to perform the skill can be articulated. They cannot be learned directly from a book or lecture. Skills are best learned in the setting of a master and

an apprentice. The student must submit to the authority of the master and imitate his skill. It would follow then that any skill which was not used for a generation is altogether lost. As Polanyi illustrates,

It is pathetic to watch the endless efforts equipped with microscope and chemistry, with mathematics and electronics- to reproduce a single violin of the kind that the half-literate Stradivarius turned out as a matter of routine more than 200 years ago.⁷

Polanyi believes that what has been said about skills equally applies to connoisseurship, i.e. the ability to pass critical judgments in an art or science.⁸ The skills of testing and tasting are continuous with active muscular skills like riding a bicycle. These abilities are based on a similar logic to the skill of bicycle riding. Again, the connoisseur is not explicitly aware of the rules governing the doing. Connoisseurship can be communicated only by example; it cannot be taught by precept.

Consider the connoisseurship of identifying of what class an object is a member. Such a skill is foundational to diagnosing the health of ecosystems or organisms. We must know what class a particular biological entity is a member of to diagnose its health because health is defined by class membership. At universities which specialize in this taxonomy, a great deal of time is spent in

laboratory classes designed to teach the student how to identify particular classes of rocks, chemicals, plants, animals, diseases, and other ailments. Taxonomy cannot be taught by lecture. Wherever connoisseurship is found operating in science, it persists only because it cannot be replaced by a measurable grading scale.⁹ Measurement has the advantage of greater objectivity, i.e. measurement gives consistent results in the hands of different observers; however, in the case of identifying if something is a member of a certain taxonomic class, no scheme of measurement can replace the judgment of an expert. C.F.A. Pantin has described how a new species is discovered,

...by a peculiar feeling of discomfort that something is not quite right, followed by a sudden detection of the error and simultaneous realization that it is highly significant-"It is a *Rhynchodemus* all right, but it is not *bilineatus*- it is a new species."¹⁰

Patin called this means of identification "aesthetic recognition". Once the new species is identified, it usually can be defined by distinctive key features. However, these key features are themselves variable. For instance, in 1930, the Fifth International Botanical Congress was called, in part, to try to standardize the definitions used within botany. The problem was that the lanceolate shape of Linnaeus is very different from that of Lindsay; however, no two taxonomists at

the Congress drew the same form of lanceolate.¹¹ The knowledge of key features is a valuable maxim for the identification of specimens, but like all maxims, it is useful only to those who possess that art of applying it.¹² The members at the Congress could not apply a measurable standard to the taxonomic problem. Therefore, they could not replace their connoisseurship with an objective standard. The large amount of time that science students spend in laboratory, especially taxonomic classes, shows how greatly these sciences rely on the transmission of skills and connoisseurship from a master to an apprentice. It demonstrates the extent to which the art of knowing has remained unspecifiable at the very heart of science.¹³

Additionally, we can look to our legal system to find a common everyday example of this taxonomic problem. Our laws make the crime of murder punishable by death or a prison term. The law implicitly assumes that we can identify a human being. Reflection shows that we always can identify the presence of the human form, even if it is malformed, diseased, or mutilated. Since there is no case on record in which an accused pleaded innocent because he did not recognize the human shape, we can assume that this demand is not excessive.¹⁴ (I am not referring to a case of mistaken identity, like a

hunting accident, but a genuine case of murder where the murderer claimed the individual that was killed was not thought to be human). Yet it seems impossible to give an explicit definition which would unambiguously specify the domain of human shape. It is also the case that those of us who can identify human beings are not in possession of an explicit definition. We believe that we can identify a human even though we can not articulate the specific features by which we do it.

An important distinction that arises from this analysis of knowledge and skills is the distinction between focal and subsidiary awareness.

When we are relying on our awareness of (A) for attending to something else (B), we are only subsidiarily aware of (A). The thing (B) to which we are focally attending, is then the meaning of (A). The focal object of (B) is always identifiable, while the things like (A), of which we are subsidiarily aware may be unidentifiable.¹⁵

For example, when we use a hammer to drive a nail, we attend to both the nail and the hammer but in different ways. We watch the effect of our stroke on the nail while we try to swing the hammer to hit the nail. When we bring the hammer down, we do not feel the hammer handle in our hand. We feel the hammer hit the nail. We have subsidiary awareness of the feeling in our hand which merges

into focal awareness of driving the nail. Focal awareness and subsidiary awareness are mutually exclusive.

Similarly, think of how blind people learn to use a probe. Initially they sense the clumsy feel of the probe in their hand. They are focally aware of the feeling in their hand. As they learn to use the probe as a tool or instrument, they begin to feel the probe touch the sidewalk. Gaining skill with the instrument is learning to be focally aware of the sidewalk and subsidiarily aware of the probe in their hand. As they learn to use the probe with more and more skill, the probe becomes part of them. They no longer focus their attention on it. A skill is the capacity to carry out a large number of particular movements with a view to achieving a comprehensive result. This result is true for a skillful practical performance like hammering a nail, or as Polanyi argues, for a skilled performance of knowing, such as a skilled medical diagnostician has.

The Skill of the Medical Diagnostician and its Implications for
Ecosystem Health and Watershed Assessment

To become a skilled medical diagnostician a person must go through a long course of experience under the guidance of a master. The apprentice must learn to recognize certain symptoms which his

textbook has described. He must personally know that symptom and he can only learn it by repeatedly being given cases where the symptom is authoritatively present, side by side, with other cases in which it is absent. The apprentice, over a considerable period of time, does this until the expert is satisfied that the student has acquired the ability to judge.¹⁶ Eventually, the student may become a skilled diagnostician. Such a course of study must include large amounts of time spent in laboratory classes and residency. At first, the student may look at specimens which the authority claims have certain symptoms and not be able to see them at all. For instance, the master may claim that a certain X-ray shows a certain symptom. The student initially may see nothing. The student must come to see the clues and learn to see what the master sees. This learning is an activity which can succeed or fail and takes considerable skill. Whether a student has the skill can only be judged by his ability to achieve the successful outcome. By the very nature of it being a skill, the student cannot articulate how he has come to know it. He can point to some of the specifics but the activity as a whole is unspecifiable.

When a medical student is learning to come to an understanding of what a healthy individual of a particular kind looks like, two activities are carried out simultaneously. One is that the individual is appraised to determine what kind it is. With each new specimen of a particular type, the student gains a greater understanding of the ideal for that type. While at the same time, the individual organism is being appraised by the ideal for that particular type to ascertain how well it conforms to the standard for the type. This connoisseurship is also largely unspecifiable. It is a question of having the skill to carry out the activity.

A number of important implications for diagnosing the health of ecosystems follow from the description of medical doctors diagnosing the health of their patients because ecosystems are also the kind of systems which can succeed or fail. There are a number of aspects where diagnosing ecosystem health ought to be analogous to a medical doctor's diagnosing the health of his patients. Several of these points are crucial for the current debate. Where current conservation biology perspectives differs from medical perspectives, this study shows in this chapter and the following one that much of

the difficulties being experienced with ecosystem health result from not following the medical example.

In medicine there is no textbook which specifies what a healthy individual looks like. Identification of a healthy individual is a skill which medical practitioners recognize cannot be explicitly specified. Certain particulars can be specified on reflection, but in the end it can only be learned by experience. The skill cannot be replaced by more objective measures. Therefore, no "cookbook" or "recipe" can be written to replace the skill of the diagnostician. The same would be true of the health of ecosystems. No "cookbook" account can be written. The goal of writing an explicit description of a healthy ecosystem, such that any trained or untrained observer could make a correct diagnose, is misguided. Such a view fails to recognize the crucial role of skills in the diagnosis of ecosystem health.

Similarly, the connoisseurship involved in diagnosing the health of ecosystems also cannot be entirely specified. Upon reflection, certain elements can be identified but it is not possible to explicitly give an account of the particulars. Therefore, no criteria other than the fact that the individual can perform the skill correctly can be

given. As we saw in the case of riding a bicycle, a complete knowledge of the physics involved does not address the question. A physicist could know all the physical equations for riding a bicycle and still be unable to ride a bike. Analogously, an ecologist could know all the cycling of matter and energy within a watershed and this information would not tell us whether this individual could diagnose the health of an ecosystem.

A university education for diagnosing ecosystem health should parallel medical schools. The course of study should emphasize the development of connoisseurship with regard to ecosystem health. There are currently no masters who specifically teach apprentices to diagnose the health of drainage basins in similar terms. There are programs developing which are headed that way, but there are two factors which hamper this endeavor. One is that scientific education, especially at the graduate level, is usually highly specialized. If we contrast medical school with a graduate education in science we can see this difference. All M.D.s first complete a general course of study and become general practitioners. Their general education contains significant amounts of laboratory classes and practical experience, as well as lectures. After completing their general education, they may

specialize but all of them share their general education. In medicine, it is the general practitioner who diagnoses the health of the patient. It is the general practitioner who recommends the particular specialist that the patient should see. In medicine, it would make little sense for a patient who believed that he were ailing to call a team of specialists to give a general diagnosis. The specialists would have a great deal of difficulty setting aside their specialized training. They would have to rely on their general education because they must first determine what it is that they have before them and once it is identified it, they must evaluate it based on their experience with that class of object.

In the current education of ecosystem health practitioners, there are no general programs. A scientist does graduate work in a specialty. To illustrate, compare the recent FEMAT scheme for diagnosing ecosystem health with the medical profession. In the FEMAT scheme, a team of specialists are brought together and asked to try to set aside their specialties and to assess the general health of basins.¹⁷ They are asked to set aside their training for the purpose of assessing the general conditions of drainage basins. Compare that practice with the medical field. In medicine, we quickly see the

difficulty of convening a group of specialists to assess the general health of a patient. But at least medical doctors have their general medical education upon which to draw. Currently, the specialists in ecosystem studies generally lack more than the rudiments of a general ecosystem education. They do not have anything analogous to general medical school to draw on. Nor is a significant part of their schooling in the field gaining the skill of diagnosing the health of ecosystems. Ecosystem practitioner's specialize by the time they enter graduate school.

The second factor is that science is largely grounded in an empiricism that makes the central role of connoisseurship difficult to acknowledge. There is a strong desire to establish an objective measuring scheme which can be used to assess the health of ecosystems. There is a belief that an objective scheme based on measuring a number of criteria must be established to place diagnosing the health of an ecosystem on sound scientific grounds, otherwise, evaluations are only subjective. This belief is mistaken. An empirically evaluation based on measuring certain attributes has the advantage of being objective, but such a scheme cannot capture more than a few rudimentary features of the connoisseurship

involved. Medical doctor's do not have such an objective scheme to diagnose the health of a patient. Why is it assumed that ecosystems are different? The connoisseurship cannot be replaced by a graded measured scale. The skill of general practitioner cannot be replaced by a "cookbook" placed in the hands of an untrained individual.

As we saw in Chapter 3, in order to diagnose the health of an ecosystem, a practitioner must simultaneously classify the individual specimen into its proper class as well as ascertain the achievement of the particular individual as a member of a class. The diagnostician, in the act of making a diagnosis, will be simultaneously improving his understanding of the type specimen as well as ascertaining the health of the particular specimen. This activity should be the central connoisseurship in assessing ecosystem health. Currently, little attention is being given to this activity in conservation biology. As we have seen, many of the major players deny that drainage basin ecosystems can be typologically classified.

These are the major points where the analogy between diagnosing medical health ought to be analogous to diagnosing ecosystem health. Currently, diagnosing the health of ecosystems is considerably far from medicine in these regards. If the analysis of

this study is correct, the most productive advancements in diagnosing ecosystem health can be made by developing ecosystem science closer to the practice of medicine. Conservation biology must determine how drainage basin ecosystems should be classified, and begin to develop general practitioners in each of the classes.

The analogy between diagnosing medical health and ecosystem health breaks down wherever the fact that ecosystems do not exist as individuals in time comes into play. For human beings, there is one basic concept of health for individuals of a particular age. The concept of health shifts as the individual moves along the trajectory of youth through old age. In individuals, this trajectory is non-reversible. In the case of drainage basin ecosystems, a more complex conceptual picture is more applicable. There is no non-reversible trajectory from youth to old age. It is the case that the vegetation moves along a somewhat linear development from earlier vegetational succession to later successional stages; however, a number of factors such as floods, fires, windthrows, or human activities can reset the successional development to earlier stages or it may appear that a number of stages simultaneously occur in an area.¹⁸ It may also be the case that certain disturbances move the

ecosystem to a whole new trajectory of development. At each successional stage, a concept of a healthy system must be developed. As we have previously discussed, the general practitioner examines a system and simultaneously classifies the basin generally (as we saw in chapter 3, whether basins can be classified into types remains to be established), identifies where it is along the successional stage, and assesses the state of health of the basin. With each basin examined, the general practitioner gains additional experience and can fine-tune the conceptual picture of the system. The fact that an ecosystem is not an individual which goes through a predictable trajectory from birth to death makes diagnosing the health of ecosystems more difficult than diagnosing the health of a human or a horse.

Therefore, assessing the health of an ecosystem in several key ways is analogous to assessing the health of a human. Where current methods of watershed assessment widely diverge from the methods used by medical doctors, we ought to examine the step and articulate why it is that it is done this way rather than the way medicine does it. In many cases, there will be reasons why the methods need to be different. However, medicine has had centuries to develop its

methodology and we should learn from their experience. This may speed the development of our skill in diagnosing the health of watershed ecosystems.

Summary

Ecosystem health is a coherent concept in conservation biology and ecology. Ecosystems are the kind of systems which have rules of rightness or health. As members of this class of objects, they share common features with other members of this class such as humans and horses. Diagnosing the health of a particular object consists in simultaneously identifying its class membership while assessing the achievement of the particular case with the type specimen for that class. This activity is a skill which takes a significant amount of connoisseurship. No measurable graded scale can replace this skill. No "cookbook" can be written which will enable an individual to do it. A skill must be learned by doing and the best method of learning is under the guidance of a master.

Notes

- ¹ M. Polanyi, *The Tacit Dimension* (Doubleday, Garden City, 1966) p. 4.
- ² M. Polanyi, *Personal Knowledge* (Harper, New York, 1964), p. xiii.
- ³ Gestalt psychology assumes that perception of a physiognomy takes place through the spontaneous equilibration of its particulars impressed on the retina or the brain. Polanyi has recast its findings as the outcome of an active shaping of the particulars into the whole.
- ⁴ M. Polanyi, *The Study of Man* (The University of Chicago Press, Chicago, 1959) p. 28
- ⁵ M. Polanyi, *Personal Knowledge*, p. 49.
- ⁶ M. Polanyi, *Ibid.*, p. 50.
- ⁷ M. Polanyi, *Ibid.*, p. 53.
- ⁸ It is interesting to observe that the word connoisseur comes from the Latin cognoscere which means to know or to understand.
- ⁹ M. Polanyi, *Personal Knowledge*, p. 55.
- ¹⁰ In M. Polanyi, *Ibid.*, p. 351.
- ¹¹ Fifth International Botanical Congress, Aug 1930, Report of Proceedings, Cambridge, England. In: M. Polanyi, *Ibid.*, p. 542.
- ¹² M. Polanyi, *Ibid.*, p. 351
- ¹³ M. Polanyi, *Ibid.*, p. 55.
- ¹⁴ M. Polanyi, *Ibid.*, p. 348.
- ¹⁵ M. Polanyi, *Ibid.*, p. x.

¹⁶ M. Polanyi, *Ibid.*, p. 55.

¹⁷ Anonymous, *Ecosystem Analysis at the Watershed Scale: The Revised Federal Guide for Watershed Analysis, version 2.1* (Regional Ecosystem Office, Portland, 1995), p. 3.

¹⁸ Tansley for instance recognized an important distinction here. On one hand, some succession did appear developmental; he termed this autogenic succession, while in other cases, disturbances so disrupted the succession that no orderly transformation in the vegetation took place. He called this allogenic succession. (J.B. Hagen, *An Entangled Bank* (Rutgers University Press, New Brunswick, 1992), p. 83).

CHAPTER V

THE IMPORTANCE OF THIS STUDY FOR RESOURCE MANAGEMENT AND POLICY

Introduction

The nation's river systems are degrading at an alarming rate. Nowhere is this more evident than in the Pacific Northwest. In 1991, Nehlsen et al. documented that numerous populations of Pacific salmon (Genus: Oncorhynchus) were in danger of extinction.¹ In 1994, for the first time in history no coho (O. kisutch) fishing was allowed along the Oregon coast. Salmon fishing historically was the state's second largest industry and coho were the most numerous salmonid in the catch. The salmon crisis, along with the Spotted Owl (Strigidae: Strix occidentalis) listing under the Endangered Species Act, signaled a crisis in resource management in the region. Traditional management schemes which focused on single species had failed.²

What is emerging is a view of resource management which emphasizes sustainability and biodiversity.³ A common theme is that an ecosystem perspective should be the foundation of resource management.⁴ A major push towards ecosystem management in the region occurred when President Clinton selected Option 9 from the Forest Ecosystem Management Assessment Team (FEMAT) document to break the Northwest forest gridlock.⁵ In this option, called the Aquatic Conservation Strategy, watershed assessment was identified as the major tool for implementing ecosystem management as directed by the Northwest Forest Plan. According to the Record of Decision there are 9 objectives in the Aquatic Conservation Strategy. Three of them are directly relevant to this study⁶:

- 1) Maintain and restore the distribution, diversity, and complexity of watershed and landscape scale features to ensure protection of the aquatic systems to which species, populations, and communities are uniquely adapted.
- 2) Maintain and restore the sediment regime under which aquatic ecosystems evolved. Elements of the sediment regime include the timing, volume, rate, and character of sediment input, storage, and transport.
- 3) Maintain and restore the species composition and structural diversity of plant communities in riparian areas and wetlands to provide adequate summer and winter thermal regulation, nutrient filtering, appropriate rates of surface erosion, bank erosion, and channel migration and to supply amounts and distributions of coarse woody

debris sufficient to sustain physical complexity and stability.

The focus of this chapter is on the ability of watershed analysis to diagnose the health of watershed ecosystems from the perspective of Pacific salmon populations. Protection and restoration of Pacific salmon is a major focus of the Aquatic conservation strategy. Therefore, the focus will be primarily on the stream ecosystem portion of the watershed. This study was designed to evaluate the watershed assessment procedure. Watersheds were defined in this study as drainage basins from 20-200 mi². This is the same size as watersheds in the Federal document. The Federal process is critiqued by examining the concept of health and the nature of the hierarchies within the watershed analysis process. The Federal watershed analysis process has similar problems to O'Neill et al. and Costanza et al. discussed in Chapter 3. Lastly, an alternative to the Federal watershed assessment process is presented which is used to assess the health of a watershed.

Critique of the Current Federal Watershed Analysis Process

According to the Federal document, "watershed assessment is an evolving process to develop a scientifically based understanding

of the processes and interactions occurring in the basin that control or influence the values that society places on the basin.”⁷ A watershed is nested within a hierarchy of analytical scales. The highest analytical scale according to the scheme is the region. The second level is the river basin. It is a portion of the landscape from hundreds to thousands of square miles. A watershed is a subunit of a river basin. It is normally from 20-200 mi². A further subunit is the specific area. To be useful for assessing ecosystem health, the dominant ecosystem processes must be identified at the larger scales.

8

The analysis consists of six steps: 1) Characterization of the watershed; 2) Issues and Key Questions; 3) Current conditions; 4) Reference conditions; 5) Interpretation; and 6) Recommendations. These steps are sequentially followed, although it is acknowledged that there might be a need to return to earlier steps. The process is undertaken by an interdisciplinary team of specialists, and a complete documentation of each progressive step through the process is seen as essential for demonstrating the credibility of the analysis.⁹

Watershed analysis is a procedure which can do the job of assessing the health of watersheds. It is a giant step in the right direction. A review of a number of assessments in the last few months indicated that the process can work. For instance, the Cummins/Tenmile watershed assessment along the mid-Oregon coast is a good example of a successful analysis. It basically provides a good foundation for assessing the health of the river systems and the functional components that are affecting the aquatic portion of the watershed. There is some apprehension about watershed analysis, but the worry is not that the process cannot work. It has worked in a few cases. However, in a large number of cases it has been less than successful.

A major impetus for this study is to help clarify some of the philosophical issues that are the root of some of the apprehension about watershed analysis. There are a number of ambiguities in the watershed analysis document which make it difficult to understand what is intended. Two of the major ambiguities are the definition of ecosystem health and the nature of the hierarchical view within the document. This chapter begins by examining how the document defines hierarchies and ecosystem health. The view of hierarchies in

the document is the same as O'Neill et al. and Costanza et al. The hierarchy is based on space and time scales. The concept of health is ambiguously defined.

In the introduction of the document, in a section on analytic hierarchies, it states,

To be useful for assessing ecosystem health, the dominant ecosystem elements must be identified at the larger scale (e.g. region and river basin) in order for smaller scale analysis to be relevant.¹⁰

This quote occurs within a paragraph emphasizing that watershed analysis is nested within larger scale processes at the river basin level. Chapter 3 of this study concluded that the concept of ecosystem health was not coherent from a hierarchical systems theory based on space and time scales. The watershed document defines their hierarchical system using the same space and time scales as O' Neill et al. and Costanza et al. Such a methodology does not assume that ecosystems have at least two levels of control. This study has shown that ecosystem health as defined in this study is not compatible with hierarchies based on time and space scales. Therefore the concept of ecosystem health is at least ambiguous in the watershed analysis document. There is the possibility that

“ecosystem health” can be alternatively defined, but as we will see the document did not adequately define the term.

“Ecosystem Health” is Ambiguously Defined in the Document

A distinction discovered by Jim Karr helps illustrate that the watershed analysis process is ambiguous. Jim Karr made a distinction between “ecosystem integrity” and “ecosystem health.”¹¹ “Ecosystem integrity” refers to the conditions at sites with little or no influence from human actions. “Ecological integrity” refers to the capacity to support and maintain a balanced, integrated biological system having the full range of parts and processes expected in the natural habitat of a region.¹² As Karr sees it, this definition contains 3 messages: 1) biology acts over a variety of scales from individuals to landscapes; 2) biology includes items one can count and processes that generate and maintain them; 3) biology is embedded in dynamic evolutionary and biogeographic contexts.¹³

“Ecosystem health” describes the preferred state of sites modified by human activity (e.g. cultivated areas, plantation forests, industrial parks, and cities). These later sites can be considered “healthy” when the present use does not degrade them in ways that

preclude that use in the future and does not degrade areas beyond their borders.¹⁴ In the latter case, the sites do not have integrity from an evolutionary sense, but they are flourishing and performing all their vital functions normally and properly. A healthy organisms is resilient and able to recover from stress. A healthy organism or site requires minimal care.

Karr suggested a distinction that is important here. "Ecological integrity" is objective in a sense that it is an assessment of the ecosystem based on ecological criteria. While "ecological health" is more subjective, it is based not only on ecological grounds but also on the social desires of human culture.

However, there are some difficulties with his distinction. Karr was one of the authors in Costanza et al. If he accepts their perspective, then his view of ecosystem health and integrity can be criticized on the same grounds that was presented in chapter 3. Costanza et al. accept the concept of ecosystem health but their paradigms for an ecosystem are flames and thunderstorms. They accept a hierarchical system, not a hierarchy of control, but a hierarchy of rates of processes. The hierarchies are hierarchies of space and time scales. Chapter 3 showed that the concept of health

was not compatible with this perspective. Karr has not clearly articulated his distinction in terms which are clear and coherent. He has not clarified how he reconciles the apparent contradictions.

Karr has also not clarified what he means when he says that ecosystem integrity refers to sites with little or no human influence. For example, prior to European settlement here in the Pacific Northwest, the Native Americans used fire to manipulate the landscape. In the Willamette Valley, including what is now the city of Eugene, they usually burned the valley annually. This maintained most of the valley in grasses rather than old-growth conifers. Would this situation constitute little or no human influence or would this be a significant influence? It is ambiguous where he wants to draw the line. There is a problem here of what is considered natural and unnatural. Are human activities all natural or unnatural or are there further distinctions that need to be drawn? This question lies outside the scope of this study but in order for Karr's definition of integrity to be clear he needs to address this in some detail.

However, let's assume that Karr can somehow resolve those problems. Biological integrity refers to the system itself in a condition without human activity. "Ecosystem health" in Karr's

terminology refers to society's evaluation of what is valuable. It refers not to the biological or ecological conditions of the system, but what society values. A system is "healthy" if it is providing society with the products it desires.¹⁵ This sort of an understanding of "health" is accepted by a number of people. Karr rightly wants to clearly draw a distinction between these two concepts. Does watershed analysis mean by "ecosystem health" the concepts of "ecological integrity" or "ecosystem health" or both?

It would superficially appear that the process is aimed at "ecosystem health" and not "ecosystem integrity"; however, there is ambiguity. In step 1 part of the characterization of the basin is a discussion of the dominant watershed features and processes operating today in the basin. This characterization includes the physical, biological, and human aspects of the watershed.¹⁶ In step 2 it states that the purpose of this section is to identify key issues and questions which will be examined in watershed analysis. It says,

[The purpose of this section is] to focus the analysis on the key elements of the ecosystem that are most relevant to the management questions, human values, or resource conditions within the watershed.¹⁷

It is the purpose of this section to set the scope for the analysis. Only data germane to key issues will be collected and analyzed.¹⁸ It

would seem that an analysis of "ecosystem integrity" is not necessarily included, although I would acknowledge that it could be included. In step 4, the reference conditions, it says that the purpose of this section is to explain how the existing conditions have changed over time as a result of human influence and natural disturbances. In order to do this step, it would seem necessary to create some picture of how the system worked without human interaction. Without this picture, how would we begin to decide that the effects were simply the results of our activities? If we do not know how the system functions without human intervention, how can we be sure effects that we are seeing are detrimental and the effects of human actions? The focus here is what was sustainable in the past and what changes have occurred to affect sustainability.¹⁹ This would seem to correspond to Karr's "ecosystem health;" however, it continues that,

the full reference variability includes the full range of ecosystem conditions, processes, and values within the current climate period (the Holocene), it includes both presettlement and historical epochs, as well as current conditions.²⁰

For this step the historical is defined within the period of written record. Here it would seem that it is addressing "ecosystem integrity".

Step 5 is the place where information from the first four steps is interpreted. The goal of this section is to explain the spatial and temporal context of the watershed (i.e. the biological, physical, and social processes at force in the watershed) over the last few thousand years to the present time. Here we have again merged the social with the biological. This corresponds to Karr's distinction of "ecosystem health" but not "ecosystem integrity".

The document never defined "health" and it appeared to use it in non-compatible ways throughout the document. In the introduction "health" was understood to include values but later in the document "health" was understood as not including human values. Although the process of watershed analysis is assessing the health of the ecosystem, it is not clear how "ecosystem health" is understood in the process. For instance, does it include the idea of "ecological integrity" or does it not? What is the standard of health? Are Native American activities included as part of the system or are they part of the human impacts? I find it difficult to maintain that Native American impacts are part of the natural system while human activities by Europeans are not part of the system, but it appears that several assessments imply this distinction. The methodology

does not define natural or unnatural activities, nor does it adequately define an ecosystem or "ecosystem health."

In this study "ecosystem health" is defined as the rules of rightness for the ecosystem based on its membership in the class of objects which have operational principles. The operational principles determine how the parts are interrelated to accomplish the purpose of the system. A system which is in a condition with little or no human activities, for Karr, would have biological integrity. For this study, such a system would be considered "healthy". Here, "health" refers to the condition of the watershed in reference to its standard which is determined by its class membership, not on the desires of a culture.²¹ While, the distinction that Karr is trying to make is a good one, there is reluctance to give up the term "health" to describe the relative level of achievement of a system compared with its ideal. When we refer to human "health" we usually are referring to the relative functioning of an individual as compared with the standard for that particular species. Ecosystem "health" should be reserved as a similar descriptor.

The Concept of Hierarchical Structure is Ambiguous in the Document

The other major ambiguity in the watershed analysis document involves hierarchical processes. The notion of a hierarchy can be variously understood. The watershed analysis process includes a number of hierarchical components. First, four analytic scales were identified in FEMAT: the region, the river basin, the watershed, and the site. The processes at higher levels must be kept in perspective in order for the smaller scale analysis to be relevant. The figures provided are of the familiar sort. The X and Y-axes are time and space which are continuous variables, but the levels of the hierarchy are recognized as discrete categories with unique features. It would seem that the claim is that these levels of the hierarchy differ in kind from other levels, i.e. they have unique processes which are significant in them that are not relevant at other levels. Large scale processes cannot be reduced to lower level processes.²² For example, it is advisable when starting the process to begin at the river basin scale to view watershed analysis in its proper context. In this way "tunnel vision" is avoided.²³ The references to hierarchies throughout are the hierarchies of systems analysis we saw in chapter 3 in both O'Neill et al. and Costanza et al. I argued in chapter 3 that

this formulation of systems was inadequate for “ecosystem health” to be coherent. A dual control hierarchical theory of ecosystems is necessary for “ecosystem health” to be coherent. The concept of “ecosystem health” only applies to the systems where the whole is more than the sum of its parts. In certain sections of the document, the accepted hierarchical view seems to imply that the whole is greater than the parts. In these cases, there is no reduction to lower levels. But in other cases, reduction is not assumed.

Over and over it was emphasized in the document that a hierarchical approach was being taken, and that higher level processes set the context for lower scale processes.²⁴ Yet look at the process itself. It was recognized that differences would occur between specialists.

Experts in various fields have difficulties, or show a reluctance in conversing with experts in different fields. This difficulty is attributed to several factors, including differences in terminology and language, analytic tools and techniques, philosophical approaches, maturity of the discipline, and basic lack of interdisciplinary frameworks or approaches.²⁵

As a result modules of specialists are organized that share similar perspectives. The whole is broken into smaller parts. But hierarchical theory tells us that the whole is not reducible to parts, so

how is this resolved? According to the document, "The process of subdividing ecosystem subunits has some potential inherent dangers. Since a primary purpose of watershed analysis is to provide an assessment of an ecosystem, dividing the system presents the problem of how to reassemble the pieces in a logical and coherent fashion."²⁶ How is this problem resolved in the document? It is not! The next two sentences continue, "This drawback is not limited to domains however. The traditional approach to science is to subdivide the assessment of any system into incrementally smaller pieces."²⁷ This is not a satisfactory answer. The process reduces the problem to a lower level and leaves it there. Therefore, the view of hierarchy which seems to be accepted at this point is that the wholes are equal to the sum of the parts. If the system is not reducible, then this reductionistic methodology is flawed. But if ecosystems are reducible, then "health" does not apply to them.

To make matters worse, in the module on the human dimensions domain, it states, "Analysis of the watershed ecosystem cannot be accomplished entirely by one specialist. Even if it could, there would be a need for a separate and in-depth look at the ecosystem's pieces before bringing the pieces back together again."²⁸

The last sentence is clearly a claim to reduction. If "health" applies to ecosystems, then no amount of looking at the parts can give a sufficient answer at the level of the whole. How many specialists would agree with the sentence from the document? My guess is a great number. They believe that the whole is a sum of its parts and no more. Therefore, the hierarchical view that is articulated throughout the document is ambiguous at best. The accepted hierarchical view may make "ecosystem health" meaningless.

Since each module which has its own language and terms, tools, etc., is never synthesized with other modules, a number of incoherent positions will naturally arise in the document. For example, in chapter 3 of this study a real hierarchical view is necessary for "ecosystem health" to be coherent but such a view is not compatible with Darwin's theory of evolution. Many sections of the document have these sorts of ambiguities in it. For example, Darwin's theory is related to Charles Lyell's geology, not to John Powell's geology. Powell rejected Darwin's theory; he could not make it compatible with his assumptions in geology.²⁹ The problem is how do you reconcile perspectives which are grounded by mutually

exclusive philosophical views of nature? This method provides no solution. As a result, a number of ambiguities arise.

The root problem here is that watershed assessment can not be reduced to modules. The watershed analysis process has rejected the medical model. The next two sections of this chapter will show that the Federal watershed assessment process is reductionistic in two senses: 1) it reduces an ecosystem to its component parts; 2) it attempts to reduce the skill of diagnosis to a quantifiable measurement.

Watershed Analysis is Assumed to Best be Conducted by a Team of Specialists

The watershed assessment procedure assumes that analysis is best done by an interdisciplinary team of specialists. This is a rejection of the medical model. In medicine, all doctors are trained to be general practitioners, and then they may specialize. It is the general practitioner who sends the patient to the specialist as needed. Accepting the medical model would alleviate much of the problem with reconciling all the contradictory views.

Several years ago an interdisciplinary project was initiated in the Oregon Coast Range. The project began with each of the

specialists telling the technicians how to collect the data. A set of technicians were going to each collect part of the data. A hydrologist, a geomorphologist, and two stream ecologists were present. Each specialist in turn told the technicians how to collect the data. The technicians then asked a fairly routine question. It seemed ambiguous to them which was the right bank and which was the left bank. My recollection is that the geomorphologist made a remark about any competent river worker knowing that the right bank is the right bank as you face downstream. Geomorphologists always work downstream. This problem was not resolved in a few minutes. A couple other similar ambiguities were resolved in a like fashion and the specialists left, and each team of technicians was left to figure out for themselves how to collect the data. Each team sorted out the details as best it could.

This instance happened early in the development of interdisciplinary teams, but much of the same dynamics is inherent in the approach today. The watershed analysis method acknowledged this problem with interdisciplinary teams but did not provide a solution.

By not following the medical example, we are making it more difficult to adequately diagnose the health of watersheds. The watershed assessment process assumes that a team of specialists are necessary to make the diagnosis. Such reasoning is rooted in a reductionistic way of thinking, i.e. in the end the modules must each be carefully examined by a team of specialists and the quantitative test performed before the synthesis can occur.

This perspective is only true if the whole reduces to its parts. If the whole does not reduce to its parts, as was shown in this study, the additional information at a lower level may be irrelevant to questions at a higher level. A diagnosis of the health of an ecosystem occurs at the level of the ecosystem and not at the level of its component parts. The watershed analysis method clearly outlines the problems with a specialist doing the assessment, but does not address the question of a generalist doing the analysis. There may be good reasons for not considering this option but none were given.

Credibility is part of the reason. An expert in a specialty is considered the last word on a subject. A quick check of who is asked to testify at Congressional hearings and the grounds upon which their testimony is accepted substantiates the claim. Such a view would

necessitates a reductionistic view. If a specialist is ultimately the person who answers all questions, then all questions reduce to these specialties and the whole is no more than the sum of its parts.

In medicine, where the whole does not reduce to the sum of the parts, it is the general practitioner who diagnoses the health of the patient, not the medical research specialist. We recognize in medicine that the skills of a general practitioner are different from those of a medical researcher. If the question is asked, which would you rather have make a diagnosis of your state of health, a general practitioner or a medical researcher? I would pick a general practitioner.

This Method has a Tendency to Minimize the Role of Skills in the Process

This section shows that the Federal watershed analysis procedure does not follow the medical example by tending to remove skills from the analysis. There is a desire by many within the federal agencies, as well as outside the agencies, to reduce the procedure to a "cookbook". Reducing the assessment procedure to a quantifiable methodology will stifle development of the necessary

diagnostic skills. This section brings chapter 4 of this study into the discussion.

The method, although ambiguously, outlines how each of the first four steps will assemble the data sets. Interpretation does not begin until the fifth step. Also, each team goes out and assembles the data in its domain. What we are describing here is a “cookbook” approach to watershed assessment. Examine the following example drawn from the procedure, an aquatic scientist diagnoses the health of an ecosystem as a medical doctor diagnoses the health of a patient.

.³⁰ The claim is that the specialist is examining the data much like a doctor would diagnose a patient. There are several concerns here.

Does diagnosis reduce to making the patient fit within a set of predetermined parameters? Can diagnosing the health and maintaining the health of a patient be reduced to a set of quantitative parameters? If so, this is removing all medical skill from the process and replacing it with a measurable standard. As we saw in chapter 4, such an approach has great appeal. It is more objective. Why is it that a doctor can not take few lecture classes, get a set of instruments, and begin measuring? If the blood pressure is too high, we must lower it. If weight is too high we lower it, etc.

Health is defined here as the condition where the individual falls within the accepted range of values for a number of measurable criteria. Each of the measurable parameters is addressed independently. The document appears to address each measurable criteria independently from the others.

In medicine, we realize that all the parts are interrelated and health refers to the joint operation of all the parts within the whole individual. Medical diagnosis is a skill that cannot be reduced to a set of measurable standards. The quantitative measurements are important but they cannot replace the connoisseurship of the medical doctor.

One of the concerns here is that health of stream systems can easily get defined as conforming to a set of quantitative standards. This is a very different definition of health than the one developed in this study. An individual could fall within the range of acceptable quantitative variables and still not be healthy. There is a real danger in accepting the concept of health as conforming to a range of acceptable values for a number of quantitative variables. The worry can not be solved by looking at more measurable standards. It is a problem that must be solved at another level of analysis. For

instance, consider the number and size of large pieces of wood in a river system. The watershed analysis approach could easily be reduced to the following method. First go to old growth sites and determine the number and size of pieces of wood. This sets the standard of health. Next measure the number and size of pieces of wood in another stream. If the stream falls below the range for healthy streams, then wood must be added to the system. To make sure the wood stays in the reach, it will be cabled and glued so it does not move. This ensures that our "restoration" efforts are successful. Over the last 10 years or so I have reviewed a number of restoration projects for the U.S. Forest Service and for the Pacific Rivers Council, and this was the thinking in virtually every project. The only data which were in the reports were the number and size of pieces. I would argue that such an approach may yield few positive benefits for the stream system. In fact, in many cases I believe such projects have been detrimental to the health of the stream system.³¹

The concern is that the process of watershed analysis will be seen as a "cookbook" to be rigidly followed in detail. Such an approach removes all skills from the process. Such an approach

would set back our efforts for decades or more. Polanyi's words echo loudly,

It is pathetic to watch the endless efforts- equipped with microscope and chemistry, with mathematics and electronics- to reproduce a single violin of the kind the half-literate Stradivarius turned out as matter of routine more than 200 years ago. ³²

We cannot replace skills with more objective quantitative measures, nor can we replace skills with "cookbooks" that guarantee success. While there are a number of agency people who see the watershed analysis method as a rigid "cookbook" setting out a set of quantitative methods, they are by no means the only ones. Conservation colleagues and environmental lawyers are also clamoring for quantifiable standards. They believe that a major reason that our natural resources are in crisis is because there were not measurable standards to hold the agencies accountable. They believe that a set of measurable standards is the only way to ensure that the legislative desires are fulfilled. Measurable standards enable anyone to judge whether a law has been followed; however, as chapter 4 illustrated, measurable standards also have the deleterious effect of removing the need for skills. Getting the measurable standards is likely to set back significant restoration for

decades. It removes the last remnant of skills which could actually accomplish the goal of diagnosing the health of a system. Imagine if we decided to replace the skill of medical diagnosis with a “cookbook” that anyone could apply. The effect would be to remove the skill of diagnosis from medicine.

If watershed analysis is seen as a “cookbook” to be rigidly followed, then it is arguable whether it can succeed. Skills are important at every level of the analysis. How do you establish a paper trail documenting the use of a skill if the aim of a skillful performance is achieved by observing a set of rules which are not known as such to the person following them? ³³ If the method is rigidly followed, then the steps need to be sequential. The method does allow that you can go back to an earlier step, but you assess current conditions before you assemble the historical picture. Also, according to the method, no interpretation is occurring prior to step 5. How can you assess the current condition without a standard for assessment? How would you know what to look for? How could a medical doctor diagnose the health of a patient without knowing what a healthy patient looks like? As we saw in chapter 4, when we are diagnosing the health of an individual, be it a watershed or a

human, we are simultaneously identifying the individual as a member of a class and gaining a clearer picture of its ideal, and we are appraising the individual in question to see how well it meets the standard for its class. This process cannot be reduced to "wooded" steps. It is a matter of skill. How could a paper trail be established which documented each step of the thought process?

So does this methodology unambiguously spell out how the process is done? No. It does allow good assessments to be done, but the process could also be followed and the results could be less than satisfactory. If the watershed analysis guide is seen as just a guide to some of the kinds of information and some of the skills that need to be employed, it can and has worked. However, if it is seen as a "cookbook" which must rigidly be followed, it will set back watershed analysis and restoration several decades.

The next section outlines portion of an alternative watershed analysis which was done prior to designing a watershed restoration project which was implemented in 1992. The procedure does not reduce analysis to a set of modules, but examines watershed level processes. It identifies a fundamental process which is absent from the federal watershed assessments.

The Knowles Creek Watershed Assessment

The Knowles Creek basin is a 20 mi² basin of the Siuslaw River on the mid-Oregon Coast. The Knowles Creek basin represents a good case study of a whole basin analysis for restoration. Knowles Creek has most of the salmonids found along the mid-Oregon Coast: coho salmon, chinook salmon, steelhead, and cutthroat trout. Knowles Creek is a tidewater tributary, so mainstem activities have little effect on the salmon. The physical setting of Knowles Creek is characteristic of large areas of the mid-Oregon coast so we believe this analysis has broader applications than just the study basin itself. It remains to be determined if the Knowles Creek watershed can be seen as a member of a class of basins with similar geology and climate or whether it differs by degree from adjacent basins.

The land ownership and past activities are similar to many other basins in the area. Most of the ownership is U.S. Forest Service and private timber lands, in this case John Hancock Timber Management Group.³⁴ Much of the lower valley floor is occupied by a major highway and small family tracts. Timber harvest began with settlement in the late 19th century, but large scale road building and

timber harvest began after World War II. Currently, only a small portion of the headwaters of the basin has not been harvested.

The Physical Setting of the Knowles Creek Basin

The Knowles Creek basin is in the Siuslaw River basin on the mid-Oregon Coast near Mapleton. Knowles Creek enters the Siuslaw adjacent to the highway 126 bridge. Knowles Creek is a tidewater tributary of the Siuslaw.

The geology of the basin is predominantly Tyee Sandstone. The bedrock weathers to sand and gravel (Knowles produces exceptional "skipping rocks"). The tributary streams are high gradient with steep hillslopes. Debris torrents are the most important geomorphic process moving sediment into the valley floors. The mainstem of Knowles is low gradient (most less than 3%). The valley floor widths vary from a few meters to several hundred meters.

The climate is coastal marine with moderate temperatures and wet winters and dry summers. Typically, Knowles Creek receives approximately 75% of its annual precipitation between October and March. It is not unusual for the basin to receive 20" of rain in a week and for 24 hour totals to reach 4-5". During the winter of

1964, which probably is the storm of record, 53" of rain were recorded in December and January and the daily total which produced the peakflow was approximately 6". On the other extreme, July-September often receive less than an inch of rain per month. During 1992, the total rainfall for the three month period was 0.00.

The Conceptual Model

The rudiments of the conceptual framework for watershed analysis of Knowles Creek are as follows: Basically the drainage basin is divided into two components for the purpose of this discussion; one is the uplands and the other is the valley floor and stream channels. The aim of the conceptual model is to understand how the basin functions, i.e. how does it move water, nutrients, organics, and sediment through the basin over long periods of time and through this digestive process develop salmon habitat? Our focus is on identifying the key watershed processes involved. For example, what are the key mass erosional processes delivering materials to the valley floor? Often they are earthflows, debris torrents, or landslides. How much material do these processes deliver to the valley floors over time? We also want to understand how the

vegetation affects these rates. What are the dominant species and their age classes? In order to understand the vegetational dynamics, we need to understand the fire regime, and if it is applicable the harvest history. We conduct a similar inquiry for the surface erosional processes. How does the fire history, vegetation patterns, harvest, and livestock grazing affect the rate that material moves to the valley floors? On the valley floors and stream channels, we want to know how the material from the uplands is "digested" through the system. For example, how often do the hollows pulse sediment into the first order stream channels? This digestion creates the structural features of the valley floors and stream channels.

We want to understand how the basin functions from four different perspectives: an idealized view, a historical view, the current view and a future view.

The idealized view describes how the basin moves material and energy through the basin without human intervention. This view describes the operational principles for the basin, i.e. it discusses how the parts are interrelated to accomplish the purpose of the ecosystem. For the purposes of this chapter we will consider the purpose of the basin to produce and sustain salmonid populations

within the basin.³⁵ It also focuses attention on the question, how would this basin be structured? What would be the dominant processes moving sediment, water, and organics from the uplands to the valley floor and stream channels? How often do mass erosion events, such as debris torrents, repeat in tributary basins? How fast do earthflows move sediments into the valley floors? How does the valley floor/ stream channel "digest" this material from the uplands? What do the valley floors and stream channels look like? How are the various aquatic organisms keying into the structure and functional processes of the valley floors and stream channels? The time span of interest is on the order of decades to centuries. This kind of exercise is viewed by some as an esoteric exercise of little relevance; however, this perspective is necessary as it serves as the standard for evaluation of the other models. Our understanding of the basin is only as good as our idealized reconstruction. That is not to say that restoring the basin to this model is the goal. If that were true, the management strategy would be obvious; simply put a fence around it and walk away. Rather, this perspective gives us an idea of the capabilities of the basin.³⁶

The second perspective is a historical model. How did this basin develop from the end of the Pleistocene to the present? What role did Native Americans play in the basin? This perspective is primarily aimed at understanding how the Native Americans affected the landscape and where the basin was at the beginning of European settlement. This perspective gives us the context to understand what Native American, European settlement and our subsequent management has had on the basin. This perspective relies heavily on the original cadastral land surveys as a baseline. These early surveys can give us a clear picture of the vegetational history at that point in time. This greatly helps reconstruct how the various geomorphic processes were operating at that point in time.

The third perspective is the current model. This perspective tells us where we are. It assesses the current health of the basin.

The fourth perspective is possible scenarios for the future. It can consist of a series of options that suggest to us what the possible outcomes of management options are.

These first three perspectives combined are the major components of the conceptual model. These three perspectives are synthesized into an evaluation of health of the basin. It also serves

to help determine restoration and maintenance efforts which are necessary within the basin. For instance, after a fire, what is the role of the burned snags and downed wood in affecting sediment, organic, nutrient regimes delivering material to the valley floors? Or after a flood, what should be done about the large log jams which are created? We believe that such a conceptual model can help identify the critical basin processes and integrate them into a single interconnected framework.

The Idealized Functional View of the Knowles Creek Basin

In the idealized view the goal is to understand how the Knowles Creek basin functions without human intervention, i.e. how the geomorphic processes move sediment and materials and form the salmonid habitat in the basin. The time scale of interest is centuries. At the end of the Pleistocene era, sediment inputs from the uplands were much higher than today. The amounts of sediments moving through the system have generally declined to the present.

The Knowles Creek Basin is in the Douglas-Fir vegetation zone of Western Oregon. The fire recurrence interval or time between stand replacement fires is about 150 years on the uplands; however,

the riparian zones have much longer recurrence intervals between stand replacement fires.³⁷ After the fires, wood in all sizes would immediately substantially increase and the increased amounts of wood to the valley floors and stream channels would remain for decades.

The inputs of sediment and organic material to the stream channel and valley floor is pulsed. After fires, as the roots weaken on the dead trees, there is probably an increased rate of movement of sediment into hollows and tributary stream channels. In severe cases, the hollow or stream channels might be filled quickly, leading to triggering of a debris torrent during a winter storm in the next few seasons. In other cases, the increased sediment input to the hollow or tributary channel simply continues for a decade or so and then diminishes back to its normal rate. In the Knowles basin, there are approximately 5,200 hollows in the basin and they pulse their sediment approximately every 6,000 years.³⁸ This gives an estimate of the frequency of debris torrents to the valley floors.

The debris torrents are triggered in the hollows and tributary channels during major storms. The torrent, comprised of large boulders and logs as well as soil and gravel, moves rapidly down the

tributary to the mainstem and the valley floor. The debris torrent often stops in the tributary junction or soon after entering the mainstem.³⁹ The valley floor gradient was not steep enough to keep them moving. Also the debris torrents would stop when they ran into the mature conifers on the valley floor. The debris torrent deposits would create a temporary dam in the stream channel where they set up for minutes to decades, depending on its size and location. Certain reaches of streams were often the site of debris torrent deposits. We named these areas the geomorphic control points. Above these areas, terraces would form, building the valley floor. These flats are the highly productive sites within the basin. They are very retentive of organic matter and sediment. It is in these areas that the major "digestion" takes place in the stream system.

We view these geomorphic control points and their associated flats like a series of beads on a string. Viewed together, they form the digestive capacity of the stream system. Each bead has its own "life history." The cycle begins when a debris torrent sets up in the geomorphic control point. The immediate effect is to dam up the stream channel. The slowed stream flow results in sediment and

organic material beginning to deposit. The debris torrent deposit may remain in place for seconds to decades depending on its size, composition, and the amount of stream flow to remove it. If the deposit remains in place through the initial storm, it will go through a period of a jam forming and pulsing out during later storms. But over time, as the channel above it builds, lowering the storage capacity of the flat, and as the big key pieces of wood weaken, sooner or later a storm is able to remove the jam from the control point. The control point then cuts to bedrock and as it does the stored material in the flat moves downstream. As the bead or flat goes through a cycle, it begins with a period of high retention of material and during that time it has high digestive capacity. Then it goes through a period where it holds material for a while before pulsing part of it downstream. Finally, the jam is removed and it exports most of the material from the flat downstream to the next flat. The flat then is in "transport mode" with very little storage and digestion taking place until the next debris torrent starts the process all over again.

Backing up, we can view the digestive process of the stream system by watching the dynamics of the beads. Some beads are in

their initial stages of the cycle and are very retentive and have high digestive capacity. Others hold and pulse in short cycles. While other beads only transport material through them. At any given point in time we will find a portion of flats in all the various stages of development. What is crucial is the dynamic nature of the beads. It is the dynamics which are required for digestion to occur in the stream channel. Material is held and then pulsed downstream to the next bead where it can be held, and so on. The key pieces of large wood and the large mature trees on the valley floor play a key role in determining the retentive capabilities of the flats. Without these key pieces of material, the control points cannot form jams which will survive storm events. When this happens, little material is stored and "digested" in the flats. Material just passes through the system.

The life history of the salmonids is tied to the digestion within the flats. It is during periods when the flats are storing material and have high digestive capabilities that they are the areas of high biological production. This is especially true for coho. It is during this time that the stream channel is highly "connected" to its valley floor, i.e. small increases in streamflow result in the water flowing

out over large portions of the valley floor. There are numerous off-channel pools and beaver ponds and many high flow channels. Flats which are only moving material through are not areas of high productivity. Over a period of decades or centuries, different flats will be the areas of high "digestion" and productivity of salmonids. In the idealized stream condition, a number of flats will always be "hot spots" of biological production; however, the particular flats which are most productive will change.

The Historical View of the Knowles Creek Basin

In the late 1870's, when the first European settlers ferried their possessions up the Siuslaw from Florence, the Knowles Creek basin was mostly young stands of Douglas Fir and patches of salal and salmonberry. Large sections of the Knowles Creek basin had burned in the fire or series of fires during the mid-1860's. The only areas which largely escaped the fires were the valley floors. Here mature conifers, predominantly cedar, were centuries old.⁴⁰

After the 1850-1860's fires, there probably was an increase in debris torrents as the weakened roots from the burned timber gave way and allowed larger amounts of sediment to enter hollows and

tributary stream channels. Years later, during large storm events, the accumulated materials in the hollows and tributaries would torrent down the tributary streams to the valley floor. The material in these debris torrents contained a number of large boulders and large logs, as well as soil and gravels. It is likely that when the settlers arrived, the Knowles basin was delivering large amounts of sediment to the valley floors.

The majority of torrents would stop at the junction of the mainstem or soon after they entered the mainstem. As the debris torrents hit the large (some approaching 2 m diameter) conifers on the valley floor, they were stopped. Also, if the debris torrent made the corner at a tributary junction, it would not be long before it hit large spanning pieces of wood which would begin to slow it and finally stop it as the spanner hit a mature conifer tree. At this time, Knowles Creek had a high capability to stop, hold, and process material.

It is likely that when the settlers arrived that some of the valley floors were grasses maintained by burning by the Native Americans. In the lower Siuslaw several prairies were evident in the early surveys that were maintained by burning; however, all along

the stream channels of the lower Siuslaw and Knowles Creek were the most magnificent stand of Cedars to be found along the coast.⁴¹ When the first settlers arrived the many of the flats in Knowles Creek would have been highly "connected" to their valley floors. For instance, when the first European settlers came to Knowles, it overflowed the valley floor in June during a light storm. (Today that same valley floor would not be flooded until approximately a 50 year storm event).

From 1880-1900, much of the lower 6 miles of the valley floor of Knowles Creek were settled. The valley floors were burned and the logs cleared from the valley floor and stream channels. The logs were cleared from the channels to minimize flooding and to clear the stream so the cedar could be floated down to the mainstream. During this period, some timber harvest took place, but fishing was the main money resource prior to 1900.⁴² As a result of this clearing, the stream began to rapidly downcut. By 1900, the stream had formed a single channel which had cut to bedrock along much of the lower 5 miles. Some of the banks were 5-6 m high. Also, splash-damming did occur early to move some of the logs down Knowles Creek to the Siuslaw River. While splash-damming did speed the

downcutting of the channel, the result was inevitable when the logs and conifers were removed from the valley floor. The large wood served as the key pieces of jams which created and maintained the channel "connectivity" with the floodplain. During this early phase of settlement, a great deal of the salmon productivity of the stream was lost. This is especially true for coho.

From 1900-1950, human activity declined from the initial settlement period. While timber harvest continued, it occurred at a rather slow rate. During this time, the amount of large wood in the stream channel and valley floor began to decline a bit. The rates of input from the uplands declined after the 1860's fires. It was now primarily windthrow on the valley floors and debris torrents which brought large wood into the stream channels.

The early 1950's to mid-1980's were an intense period of road building and logging. The majority of the basin was harvested during this period. With logging and road building, a large number of debris torrents occurred. During this period, debris torrents probably moved a volume of material equal to what naturally would have taken over 200 years to move into the channel.⁴³ This, combined with timber harvest of all the mature conifers on the

valley floor and stream cleaning to allow fish passage, resulted in destroying the digestive capabilities of the stream channel and valley floors. The Knowles Creek Basin no longer had the capability of "digesting" even normal amounts of materials from the uplands. The result was most of the flats were only exporting material. By the 1980's, the lower 12 miles of Knowles Creek had basically cut to bedrock. The only areas which retained gravels were above areas with remnant boulders from earlier debris torrents. The fish populations crashed. In 1982, only an estimated 1660 coho smolts went to the ocean.⁴⁴ Historically, approximately 75,000-100,000 coho smolts probably migrated to the ocean each year from the Knowles Creek basin.⁴⁵

The 1990's Picture of the Knowles Creek Watershed

After the period of intense road building and logging, the Knowles Creek basin began to stabilize. The amount of debris torrents declined. Most of the hollows had pulsed their material and began to refill. It will take decades or centuries to refill them. Also, largely due to a court suit, many of roads were fixed. Many road side-casts were moved and most of the culverts were replaced within the basin. The valley floor and stream channels have little or

no "digestive" capacity. Only two flats had any storage capability. The main channel of Knowles Creek has cut a channel to bedrock.

The headwaters of Knowles Creek and most of the Skate Creek watershed are the only areas which are intact and which maintain any significant "digestive" function. In these areas, the vegetation on the uplands is now approximately 130 year old conifers, primarily Douglas fir, hemlock, and western red cedar. A few older trees remain on the valley floor, but many of the large trees were killed in this section during the 1850's fires. The digestive capacity of this intact reach is probably near a low in its natural cycle. After the fires, the valley floors and stream channels were inundated with large wood. But since that time, there are few inputs. Most of the trees were killed. Also, the wood has had over 100 years to decay. The result is that the intact areas are also at a low in their capabilities to digest.⁴⁶

The rest of the Knowles Creek Basin is largely in timber production. The stands are currently mostly less than 50 years old. The riparian zones are predominantly alder; however, a significant number of cedars are growing through the alder canopy. (These

cedars were naturally reseeded, and they are growing mostly on down wood).

The stream channels are largely cut to bedrock. The alluvial formations are largely dewatered. The water tables are low. Therefore, the low flows during the summer are significantly less than they were historically. One reason is that most of the trees on the hillslopes are less than 50 years old and they are very productive. This production draws tremendous amounts of water from the soils. Also, the alluvial aquifer was dewatered when the stream channel cut to bedrock. During the drought of 1992, streamflow in the mainstem of Knowles was about 5 liters per minute for about six weeks. During low flow periods, areas of the stream become anaerobic. Water temperatures are also a problem on portions of Knowles Creek. While most biologists have emphasized the tree canopy as a critical component of this problem, this is not the most critical component on Knowles. On Knowles, the stream is dewatered. If there were larger base-flows, temperature would not be nearly as serious of a problem. Also, if the stream channel were not cut to bedrock, then a portion of the water would travel subsurface through gravels. This would also help keep the

water cool. In the intact areas, the surface waters of some pools may get to 75 degrees F during July and August; however, the subsurface component flowing through the gravels keeps the lower portion of the pools below 60 degrees. Many of these pools in intact areas have no surface flow entering or leaving them for several months. Also, just having the water flow over bedrock will heat it. If the water flowed through gravels it would stay cooler.

Comments on the Knowles Creek Analysis

The Knowles Creek analysis includes much of the same general information that is found in most assessments, including the federal procedure. Montgomery et al. outline a set of general questions that should be addressed in a watershed assessment.⁴⁷ These are basically the same questions which were asked in the Knowles Creek analysis. Some of these questions include: 1) How does this landscape work? 2) What has happened in the past? 3) What are the current conditions? 4) What are the trends in the watershed? These questions were also similar to the watershed analysis procedure from the Regional Interagency Executive Committee.

However, in the Knowles Creek analysis, the answers were given from the perspective of a generalist, not a specialist. In some

cases, it would aid the diagnosis to have information from the specialists, but in the majority of instances, it probably would not significantly change the diagnosis. Like the case with medicine, sometimes the specialists can significantly aid the general practitioner in the diagnosis, sometimes they can not.

One difference stands out. While many watershed analysis documents identify "hot spots" of fish production, I am not aware of a single one that examines the dynamics of these "hot spots" from the perspective of watershed digestion. Other analyses looked at a different space and time scale. Usually the perspective is a reach scale, such as riffles, pools, and glides, and not at the level above that, the flats. A major reason for this is that considerable data is available at the reach level. But that is not likely to be the only reason. These productive fish producing flats were not seen in the context of watershed function. They were not seen as dynamic, i.e. coming into a productive phase and passing through to a less productive, transport phase.

From the perspective of watershed digestion of sediment and organic materials, the key questions are: How are the inputs from the uplands routed through the valley floor and stream channels? How

often do various flats go through a complete cycle? How many beads are in what stage of their succession? How much capacity do they have at various points in time? These questions were never asked in the federal assessment. Watershed digestion is a critical basin function. It builds and provides the salmon habitat in a basin. If these questions were addressed to a specialist, it is unlikely they could provide an answer in a short period of time. However, a generalist can answer those questions quite easily from a watershed perspective. The specialist is likely to view the generalist's answers as inadequate. While, if the generalist waited until the specialists did get answers, the generalist would acknowledge that the specialists answers were very precise, but that level of precision is probably not necessary for the purpose of diagnosis. The claim is not that the specialist and precise data do not have a place in diagnosing the health of a basin, but what is their relationship to a watershed level diagnosis. In watershed assessment, the role of the specialist is very different than the role of the specialist in medicine. It is doubtful that the diagnosis of patients would improve significantly if specialists played a greater role in medicine. In the watershed analysis document the assumption is that an in-depth study by a

specialist is necessary to make a diagnosis. The Knowles Creek analysis was aimed at answering the questions at making a diagnosis at the watershed level, and not at a lower scale.

Watershed digestion is difficult to replace by measurable standards. It is a key process at the watershed level. An attempt might be made to measure it in a flat, but several components would be difficult. For instance, how much organic matter was excavated and transported, as opposed to stored, in the sediments and on the valley floor during a major storm? This is an essential calculation that would need to be carefully examined. It might be possible to examine it in some detail in a flat or two, but it would take an army of researchers to assess just this part of the process at the watershed level. But the point here is not primarily a point of logistics. Even if we got this data at the level of flats, digestion is more than the sum of change in organic matter storage of the flats. Digestion is the integration of a number of key elements and it is more than the sum of its parts, because it is a process that can succeed or fail. It has at least two levels of control. Even if we could accurately measure all the component parts, we still would not be able to capture watershed digestion. As long as the focus remains on collecting particular data

sets, the whole will be missed. The point is not that data are unimportant, data are essential. If our focal awareness is on the particulars, we can not see the whole. That was a major point in chapter 4. The specialist is trained to focus on the particulars, while the generalist is focused on the whole. An interdisciplinary team of medical specialists would have a great deal of difficulty making a diagnosis. Each specialists would have their component, but how would the information be integrated? The same problem exists in watershed assessment. Only the problem is much more severe in watershed assessment, because at least in medicine, all doctors are required to complete a multi-year program in general medicine before they can specialize. The medical profession has centuries of experience, and the structure of the medical community reflects this crucial point. The diagnosis needs to be done by a general practitioner with the aid of the specialists. Medicine could not work with only specialists and neither can watershed assessment.

Summary

This chapter examined the new revision of the watershed assessment document just released by the Regional Interagency Committee. This document will be used at least for the next year, to

guide watershed assessment on federal lands. This study raised several concerns about the document. In this document, watershed health was never defined. It is not clear if "health" is an intrinsic characteristic of a living system, or if it is an evaluation of watersheds from the demands of culture. Both were found in the watershed analysis procedure. They are significantly different perspectives. This study defined "ecosystem health" as an intrinsic characteristic of a watershed itself. The demands of the culture are important and are a fundamental part of the decision making process, but this study maintains that these are different questions. In medicine, these questions are also best separated.

The concept of hierarchy was accepted on almost every page of the document. However, it was ambiguously defined. Careful examination revealed that hierarchies were generally defined as processes occurring at different space and time scales. This was the definition of hierarchy used by O'Neill et al. and Costanza et al. This view of hierarchies does not recognize that health applies to some objects and not to others. It has the effect of reducing the object to the sum of its parts. Thunderstorms are just the integration of the laws of chemistry and physics. This method reduces machines, living

objects, and ecosystems to the same logical structure. This perspective destroys the notion of an operational principle. It reduces the dual control system to a single control system.

Lastly, and the biggest worry is how this guide will be used. There is a desire by many people in the agencies, as well as conservation activists and lawyers to set quantitative standards. If this document is used as a "cookbook" and if measurable standards are the focus, then this study concludes that watershed assessment may be set back decades. To set measurable standards will remove the skill involved in diagnosing the health of basins. If a standard is set for the number of large pieces of wood in a reach we will get it. And each piece will be cabled and glued to see that it stays. This has largely been the agency perspective for a decade. This accomplishes almost nothing. What counts is how the pieces of wood are functioning in the stream. How are the pieces of wood affecting watershed digestion. This is not easily quantifiable. Pieces of wood cabled in place play an insignificant role in watershed digestion. Watershed assessment is a skill which can succeed or fail. To remove the skill necessary to accomplish the task will result in restoration efforts which have little or no chance of succeeding.

Notes

- ¹ W. Nehlsen, J.E. Williams, and J.A. Lichatowich, 'Pacific Salmon at the Crossroads: Stocks at Risk from California, Oregon, Idaho, and Washington.' *Fisheries* 16-2 (1991), pp. 4-21, p. 4.
- ² J.L. Lichatowich, L. Mobrand, L. Lestelle, and T. Vogel, 'An Approach to the Diagnosis and Treatment of Depleted Pacific Salmon Populations in the Pacific Northwest Watershed.' *Fisheries* 20-1(1995),pp. 10-18, p. 10.
- ³ J. Lichatowich et al., *Ibid.*, p. 10.
- ⁴ B. Doppelt, M. Scurlock, C. Frissell, J. Karr, *Entering the Watershed* (Island Press, Washington, D.C, 1993) p. 33.
- ⁵ Anonymous, *Forest Ecosystem Management: An Ecological, Economic, and Social Assessment*. (Regional Ecosystem Office, Portland, July 1993) p. 1.
- ⁶ Anonymous, *Ecosystem Analysis at the Watershed Scale. Version 2.1* (A revised federal guide for watershed Analysis). (Regional Ecosystem Office, Portland, March 24, 1995), p. 77.
- ⁷ Anonymous, *Ibid.*, p. 14.
- ⁸ Anonymous, *Ibid.*, p. 5.
- ⁹ Anonymous, *Ibid.*, p. 3.
- ¹⁰ Anonymous, *Ibid.*, p. 5.
- ¹¹ J. Karr, 'Ecological Integrity and Ecological Health are Not the Same Thing.' In: P. Schulze, (editor). *Engineering within Ecological Constraints*. (National Academy of Engineering, Washington, D.C.,1994)
- ¹² J. Karr, *Ibid.*, p. 5.

¹³ J. Karr, *Ibid.*, p. 5.

¹⁴ J. Karr, *Ibid.*, p. 1.

¹⁵ Remember that Karr also said that a healthy system must also be sustainable and not degrade areas outside the boundaries of the system either.

¹⁶ Anonymous, *Ecosystem Analysis at the Watershed Scale Version 2.1*, p. 11.

¹⁷ Anonymous, *Ibid.*, p. 14.

¹⁸ Anonymous, *Ibid.*, p. 10.

¹⁹ Anonymous, *Ibid.*, p. 25.

²⁰ Anonymous, *Ibid.*, p. 25.

²¹ However, there is a possible ambiguity in this study as well. The operational principles describe how the various parts are interrelated to accomplish the purpose of the ecosystem. It is outside the scope of this study to investigate the purpose of ecosystems, but it is conceivable that an understanding of purpose might include something about providing various products for humans. If that was part of the purpose of ecosystems then this study might include both Karr's distinction of ecological integrity and ecosystem health. However, that topic is outside the scope of this study.

²² Anonymous, *Ibid.*, p. 5.

²³ Anonymous, *Ibid.*, p. 10.

²⁴ Notice that this statement is ambiguous. The higher level sets the context for the lower level but it does not say if it is a hierarchy of control or not. The hierarchical levels might be reducible or they might not be reducible.

²⁵ Anonymous, *Ibid.*, p. 57.

²⁶ Anonymous, *Ibid.*, p. 56.

²⁷ Anonymous, *Ibid.*, pp. 56-7.

²⁸ Anonymous, *Ibid.*, p. 67.

²⁹ Neither did Clements accept Darwin's theory because it necessitated holding a continuous view of nature which reduces wholes to parts. Both Powell and Clements held a neo-Lamarckian position.

³⁰ Anonymous, *Ibid.*, p. 86.

³¹ In a later section of this chapter I will return to this topic and discuss it in greater detail.

³² M. Polanyi, *Personal Knowledge*, p. 53.

³³ M. Polanyi, *Ibid.*, p. 49.

³⁴ John Hancock Timber Management Group purchased the private timber lands from Champion International.

³⁵ This certainly is not the only purpose or a complete description of the purpose of the basin, but production of salmonids would definitely be included as part of the purpose.

³⁶ The notion of capabilities is closely related to published and unpublished work by Charles Warren; however, the philosophic roots of this idea are significantly different from Charles Warren's.

³⁷ J.K. Agee, *Fire Ecology of Pacific Northwest Forests* (Island Press, Washington, D.C., 1993), p. 13.

³⁸ L. Benda, 'The Influence of Debris Flows on Channels and Valley Floors in the Oregon Coast Range', *Earth Surface Processes and Landforms* 15 (1990), pp. 457-466, p. 462.

³⁹ See L. Benda, *Ibid.*, for more details, pp460ff.

⁴⁰ Today the large cedar stumps can be seen throughout the valley floor. (The materials used for the historical reconstruction will appear in an appendix to the report).

⁴¹In a quote from Scoffield's journal in 1853. He had walked most of the major streams from the Klamath to the Alsea looking for a river route into the Willamette Valley.

⁴² Prior to 1900 there were 3 salmon canneries in Florence which processed salmon from the Siuslaw. The average catch was 89,000 coho and the run has been estimated at over 230, 000 fish. J.R. Sedell, and K.J. Luccessa, 'Using the Historical Record as an Aid to Salmonid Habitat Enhancement.' In: N.B. Armantrout (editor), *Proceedings of a Symposium on Acquisition and Utilization of Aquatic Habitat Inventory Information, 28-30 Oct 1981, Portland, OR* (The Western Division of the American Fisheries Society, Bethesda, 1992), p. 211.

⁴³ This information is largely drawn from Lee Benda, published (*Ibid.*) and nonpublished work. Although, at points, I have drawn different conclusions.

⁴⁴ Jeff Rogers ran a trap on lower Knowles for a season.

⁴⁵ This assumes that Knowles Creek has retained about the same capacity to produce salmon as the Siuslaw system as a whole. Currently, coho numbers in the Siuslaw as a whole are approximately 1-2% of their historical numbers.

⁴⁶ The same observation was reported for the Basalt Block Streams along the mid-Oregon Coast. T.C. Dewberry, *Historical reconstruction of Watersheds from Cape Perpetua to Heceta Head on the mid-Oregon Coast* (U.S. Forest Service, Siuslaw National Forest, Corvallis, 1994), Final Report, Project 43-04To-4-8616, p. 21.

⁴⁷ D.R. Montgomery, Grant, G.E., and Sullivan, K. 1995. 'Watershed analysis as a framework for implementing ecosystem management.' *Water Resources Bulletin* (in press), pp. 1-33, pp. 11-13.

BIBLIOGRAPHY

Agee, J.K., *Fire Ecology of Pacific Northwest Forests* (Island Press, Washington, D.C., 1993)

Anonymous, *Forest Ecosystem Management: An Ecological, Economic, and Social Assessment Report of the Forest Ecosystem Management Assessment Team (FEMAT)* (U.S. Government Printing Office, Washington, D.C., 1993) 1993-793-071

Anonymous, *Ecosystem Analysis at the Watershed Scale: The Revised Federal Guide for Watershed Analysis, Version 2.1* (Regional Ecosystem Office, Portland, 1995)

Ayala, F.J., and T. Dobzhansky (editors), *Studies in the Philosophy of Biology* (MacMillan, London, 1974)

Benda, L., 'The Influence of Debris Flows on Channels and Valley Floors in the Oregon Coast Range', *Earth Surface Processes and Landforms* 15 (1990), pp. 457-466.

Bertalanffy, L. von, *Perspectives on General Systems Theory* (Braziller, New York, 1975)

Botlin, D.B., *Discordant Harmonies: A New Ecology for the Twenty-first Century* (Oxford University Press, New York, 1990)

Clements, F.E., 'Nature and Structure of the Climax', *Journal of Ecology* 24(1936), pp. 252-284.

Clements, F.E., *Plant Succession: An Analysis of the Development of Vegetation* (Carnegie Institute Washington, Washington, D.C. Publication 242, 1961)

- Connell, J.H. and W.P. Sousa, 'On Evidence Needed to Judge Ecological Stability or Persistence' *American Naturalist* 121 (1983), pp. 789-824.
- Cowles, H.C., 'The Ecological Relations of the Vegetation on the Sand Dunes of Lake Michigan', *Botanical Gazette* 27 (1899), pp. 95-117.
- Crick, F., *Of Molecules and Men* (University of Washington Press, Seattle, 1966)
- Darwin, C., *On the Origin of Species* (Harvard University Press, Cambridge, 1964)
- Dewberry, T.C., 'Protecting the Biodiversity of Riverine and Riparian Ecosystems: The National River Public Land Policy Development Project', *Transactions of the 57th North American Wildlife and Natural Resources Conference (1992), Special Session #6*, pp.424-32.
- Dewberry, T.C., *Historical Reconstruction of Watersheds from Cape Perpetua to Heceta Head on the mid-Oregon Coast*, (U.S. Forest Service, Siuslaw National Forest, Corvallis) Final Report Project 43-04To-4-8616 (1994)
- Doppelt, B, M. Scurlock, C. Frissell, J. Karr, *Entering the Watershed* (Island Press, Washington, D.C., 1993)
- Edwards, P, (editor), *The Encyclopdia of Philosophy, Volume One* (MacMillan Publishing Co, New York, 1967)
- Ehrenfeld, D., 'Ecosystem Health and Ecological Theories', In: R. Costanza, et al (editors), *Ecosystem Health: New Goals for Environmental Management* (Island Press, Washington,D.C.,1992)
- Franklin, J. and C.T. Dyrness, *Vegetation of Washington and Oregon* (Oregon State University Press, Corvallis, 1973)

- Gatlin, L., *Information Theory and the Living System* (Columbia University Press, New York, 1972)
- Gleason, H.A., 'Is the Synusia an Association?', *Ecology* 17(1936), pp.444-451.
- Gleason, H.A., 'The Individualistic Concept of the Plant Association', *American Midland Naturalist* 21 (1939), pp. 92-110.
- Golley, F., *A History of the Ecosystem Concept in Ecology* (Yale University Press, New Haven, 1993)
- Gould, S.J, 'Is a New and General Theory of Evolution Emerging?' *Paleobiology* 6(1980)
- Gould, S.J. and N. Eldridge, 'Punctuated Equilibria at the Third Stage', *Systematic Zoology* 35 (1986), pp. 143-48.
- Hagen, J.B., *An Entangled Bank* (Rutgers University Press, New Brunswick, 1992)
- Haskell, B. D., B.G. Norton, and R. Costanza, 'What is Ecosystem Health and Why Should We Worry about It?' In: R, Costanza et al. (editors), *Ecosystem Health: New Goals for Environmental Management* (Island Press, Washington, D.C., 1992)
- Karr, J., 'Ecological Integrity and Ecological Health are Not the Same Thing' In: P. Schultz (editor), *Engineering within Ecological Constraints* (National Academy of Engineering, Washington, D.C., 1994)
- Kellogg, D.E., ' "And Then a Miracle Occurs"- Weak Link in the Chain of Argument from Punctuation to Hierarchy', *Biology and Philosophy* 3(1988), pp. 3-28.
- Kutchenberg, T, 'Measuring the Health of Ecosystems', *Environment* 27-2 (1985): pp. 32-37.

- Lackey, R.T., 'Ecological Risk Assessment', *Fisheries* 19-9 (1994), pp14-18.
- Lenoir, T., *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology*, (Reidel, Boston, 1982)
- Lichatowich, J., L. Mobrand, L. Lestelle, and T. Vogel, 'An Approach to the Diagnosis and Treatment of Depleted Pacific Salmon Populations in the Pacific Northwest Watersheds', *Fisheries* 20-1 (1995)
- Montgomery, D.R., G.E. Grant, and K. Sullivan, 'Watershed Analysis as a Framework for Implementing Ecosystem Management. *Water Resources Bulletin* (in press) 1995.
- Nehlsen, W., J.E. Williams, and J.A. Lichatowich, 'Pacific Salmon at the Crossroads: Stocks at Risk from California, Oregon, Idaho, and Washington', *Fisheries* 16-2 (1991), pp.4-21.
- Odum, E., *Fundamentals of Ecology* (Saunders, Philadelphia, PA, 1971)
- Omernik, J.M. and A. L. Gallant, *Ecoregions of the Pacific Northwest* (Environmental Protection Agency, Environmental Research Laboratory, Corvallis, 1986) EPA/600/3-86/033
- Omernik, J.M., 'Ecoregions of the conterminous United States', *Annual Association of American Geography* 77(1987), pp.118-25.
- O'Neill, R.V., D.L. DeAngelis, J.B. Waide, and T. F. H. Allen, *A Hierarchical Concept of Ecosystems* (Princeton University Press, Princeton, 1986)
- Phillips, J., 'The Biotic Community,' *Journal of Ecology* 19(1931), pp. 1-24

- Phillips, J., 'Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts. Part 1.' *Journal of Ecology* 22(1934), pp. 554-71
- Phillips, J., 'Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts. Part 2. Development and the Climax', *Journal of Ecology* 23(1935), pp. 210-43
- Phillips, J., 'Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts. Part 3. The Complex Organism', *Journal of Ecology* 23(1935), pp. 488-508
- Phillips, J., 'A Tribute to Fredrick E. Clements and His Concepts in Ecology', *Journal of Ecology* 35-2(1954), pp. 114-115
- Polanyi, M., *The Tacit Dimension* (Doubleday, Garden City, 1966)
- Polanyi, M., 'Life Transcending Physics and Chemistry', *Chemical and Engineering News* (August 21, 1967), pp.54-64.
- Polanyi, M., 'Life's Irreducible Structure', *Science* 160 (1968), pp. 1308-1313.
- Polanyi, M., 'The Republic of Science: Its Political and Economic Theory' In: M. Grene, *Knowing and Being* (The University of Chicago Press, Chicago, 1969)
- Polanyi, M., *Personal Knowledge* (The University of Chicago Press, Chicago, 1958)
- Polanyi, M., *The Study of Man* (The Univeristy of Chicago Press, Chicago, 1958)
- Polanyi, M. and H. Prosch, *Meaning* (The University of Chicago Press, Chicago, 1975)
- Rapport, D.J., H.A. Regier, T.C. Hutchinson, 'Ecosystem Behavior under Stress', *American Naturalist* 125-5 (1985), pp. 617-640.

- Ruse, M, *Philosophy of Biology Today* (State University of New York Press, Albany, 1988)
- Schindler, D.W., 'Detecting Ecosystem Responses to Anthropogenic Stress', *Canadian Journal of Fisheries and Aquatic Science* 44 (1987) Supplement, pp. 6-25.
- Sedell, J.R. and K.J. Luccessa, 'Using the Historical Record as an Aid to Salmonid Habitat Enhancement', In: Armantrout, N (editor), *Proceedings of a Symposium on Acquisition and Utilization of Aquatic Habitat Inventory Information, 28-30 October 1981, Portland, OR* (The Western Division of the American Fisheries Society, Bethesda, 1992)
- Tansley, A., 'The Use and Abuse of Vegetational Concepts and Terms', *Ecology* 16-3 (1935), pp284-307.
- Tobey, R.C., *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955* (University of California Press, Berkeley, 1981)
- Warren, C.E., *Toward Classification and Rational for Watershed Management and Stream Protection* (U.S. Environmental Protection Agency, Corvallis, 1979)
- Williams, G.C, *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought* (Princeton University Press, Princeton, 1966)
- Wynne-Edwards, V.C., *Animal Dispersion in Relation to Social Behavior* (Oliver & Boyd, Edinburgh, 1962)