TOWARD A GENERAL PHILOSOPHY OF ECOLOGY

by

Kevin Leo de Laplante

Graduate Program in Philosophy

Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

Faculty of Graduate Studies
The University of Western Ontario
London, Ontario
September, 1998

0-612-31151-1

ABSTRACT

This dissertation is a work in the philosophy of ecology and environmental philosophy. The central aims of the dissertation are to examine the role that ecological concepts and theories play in environmental philosophy, and to defend a conception of ecological science that is broad enough to address the philosophical and scientific concerns of environmental philosophers. As stated, these aims are consistent with the dominant tradition in contemporary environmental philosophy, but the dissertation is highly critical of the way the ecology-environmental philosophy relationship is conceived and theorized in contemporary environmental philosophy. Rather than view ecology as a conceptual and scientific resource that is relevant to environmental philosophy only insofar as it provides support for the ethical, social and political aims of environmentalism, I argue that the core problems of environmental philosophy are essentially problems for a general science and philosophy of ecology, which I define as "the philosophical and scientific study of system-environment relationships". This definition of ecology is broad, but it is not vacuous. A central aim of the dissertation is to defend the robustness of a conception of ecology that is sufficiently broad to encompass "ecological psychology", "ecological economics", and "ecological anthropology", as well as traditional ecological science.

The dissertation is divided into three parts, with three chapters in Part One, four chapters in Part Two, and two chapters in Part Three. Part One is a survey and critique of the role of ecology in environmental philosophy. Part Two develops a conceptual framework for a general philosophy of ecology based on developments in complex systems approaches in theoretical ecology and ecological psychology. Complexity and complex systems theories play a large role in the argument of the dissertation, and Part Three explores in greater detail

certain issues in the foundations of the complex systems sciences that are relevant to a conception of ecological phenomena as complex systems phenomena.

Keywords: ascendency theory, complex systems theories, complexity, ecology, ecological psychology, ecosystem ecology, environmental ethics, environmental philosophy, environ theory, network theory, thermodynamics, J. J. Gibson, R. Shaw, M. Turvey, S. Jørgensen, B. C. Patten, R. Ulanowicz.

DEDICATION

For Logan and Barn

ACKNOWLEDGMENTS

Writing this dissertation has proven to be the most challenging and rewarding of my experiences as a graduate student at Western. Many people contributed to its final realization.

First, I would like to express my deep thanks to my supervisor Kathleen Okruhlik, whose intellectual midwifery was crucial to the success of the thesis. Without her faith in my ability to complete the project I outlined to her over two years ago, and her critical feedback and guidance, the current dissertation would not have been possible.

To my advisory committee — John Bell, Wayne Myrvold, and John Nicholas — that read and commented on earlier, sometimes painfully undeveloped, drafts of the thesis, I give my thanks. I thank John Nicholas in particular for his extensive feedback and enthusiasm for the dissertation. Thanks also to my examining committee — Bill Harper, John Nicholas, Bob Bailey and Bill Wimsatt — who read the entirety of the final draft, and whose comments have provoked new directions in my thinking.

I was introduced to the literature on theoretical systems ecology by James Kay, of the Department of Environment and Resource Studies at the University of Waterloo. I participated in a course James taught on the epistemology of systems thinking during the fall of 1994, and during that period he made all of his research materials available to me, to examine at my leisure. I am deeply endebted to his generosity over this period, and our stimulating intellectual discussions.

For their enthusiastic support for my project, I would like to thank Bernie Patten and Bob Ulanowicz, the two network ecologists whose work figures most prominently in the thesis. Bernie and Bob do not see eye-to-eye on all issues concerning the philosophical interpretation of ecosystem theories, but their

discussions between themselves and with me have been marked with a gentle civility and respect that has made a lasting impression.

I would also like to thank Cliff Hooker, John Collier and Mark Bickhard for taking time out of their busy schedules to encourage and discuss my work.

It was a pleasant surprise to meet fellow philosophers of ecology Greg Cooper, Greg Mikkelson and David Castle (for a short while I thought I was the only one), with whom I have enjoyed informative conversations on methodological and philosophical issues in ecological science.

Western has a justly deserved reputation for excellence in its graduate program. I am grateful for having had the opportunity to be instructed in the philosophy of science and mathematics by such people as John Bell, Rob Clifton, Rob DiSalle, Bill Harper, Wayne Myrvold, John Nicholas, Kathleen Okruhlik, Kent Peacock, Itamar Pitowsky, Ram Valuri and Alison Wylie; in epistemology and the philosophy of mind by Bruce Freed and Ausonio Marras; in ethics and value theory by Barry Hoffmaster and Glenn Pearce; and in the history of philosophy by Patrick Maynard, John Thorp and Irving Block. To the many other faculty at Western with whom I did not take courses, but who have influenced and enriched my philosophical education in a myriad of ways, I also give my thanks.

I would like to thank all the student collegues who helped make my graduate school experience so enjoyable. Special thanks go to those of my "generation" (give or take a couple of years) with whom I shared anxieties over term papers, comprehensive exams, job interviews, as well as much stimulating conversation — Gerry Callaghan, Darcy Cutler, Francisco Flores, Lisa Gannett, Jason Holt, Moira Howes, Tim Kenyon, Elaine Landry, Tony Larivière, Rhonda Martens, Andrew Reynolds, Bill Vanderburgh, and Brad Wray. I could always count on Sammy Jakubowicz, Travis Hreno and Marc Ramsay to bring a smile to my face. I blame Sona Ghosh and Sean Mulligan for consistently keeping me up

way past my bedtime. There are many others that I have not mentioned here but who I will remember with great fondness; my thanks to you all.

I grew up in a family environment where education was valued and no goals were unattainable. I owe special thanks to my parents, Leo and Sallie, who encouraged and supported my studies in more ways than they know.

Finally, I owe my deepest thanks to my wife Brenda, who supported my work while putting up with the late nights, extended absences, and financial sacrifices that are familiar to partners of graduate students. I promise I'll make it up to you.

TABLE OF CONTENTS

Introduction

It may be useful to introduce the ideas and motivations behind this dissertation by reviewing the development of my interest in the foundations of ecology. One of my first jobs as a teaching assistant was as a grader for an undergraduate course in environmental philosophy. In becoming familiar with the environmental philosophy literature I was struck by the extent to which ecological science played a foundational role, not only in the presentation of environmental facts relevant to human survival (overpopulation, resource depletion, etc.), but in the motivation and articulation of a wide array of ethical, political and metaphysical theses. Within this literature, ecology was conceived at once as a *natural science* and a *worldview*, a body of knowledge concerning organism-environment relationships, and a model for a new "postmodern" metaphysics and epistemology, with profound implications for conceptions of human nature and our relationship to the natural environment. I assumed that such strong claims for the philosophical significance of ecology would have motivated philosophers of science to examine ecological science, and offer some commentary on its potential and limitations to function as a foundation for environmental philosophy. I quickly discovered, however, that philosophers of science have, until very recently, ignored ecology. There are few "philosophers of ecology", and even fewer who study the particular branch of ecology — ecosystem ecology — that, I believe, is most often appealed to in the writings of environmental philosophers. Thus I arrived at a proposal for a dissertation: to investigate the conceptual foundations of theoretical ecosystem ecology, and assess its proper relationship to the various metaphysical, ethical and political aims of environmental philosophy.

My initial expectation for the thesis was that it would be a predominantly critical enterprise. My plan was to reveal the superficial understanding of ecological science by environmental philosophers, highlight various areas where argumentation was poor or important philosophical issues avoided, and generally argue for a strong separation between ecological science and environmental philosophy. Yet as my work progressed I began to reconsider this initial assessment. For the most part, environmental philosophers do have a superficial understanding of ecological science, and they often do overlook important philosophical issues in their argumentation, but I have come to believe that a case can be made for ecology as both a science and a general perspective on a wide array of scientific and philosophical problems, and that the philosophical projects of environmental philosophers may, to a certain extent, be legitimated when viewed against the broader background of this ecological perspective. The current dissertation is an attempt to make this case.

Structure of the Dissertation

The dissertation is divided into three parts, with three chapters in Part One, four chapters in Part Two, and two chapters in Part Three.

Part One: *Ecology and Environmental Philosophy*

In Part One I argue that, contrary to the current self-conception of the discipline, the core philosophical challenges of environmental philosophy are not problems of ethics or socio-political philosophy, but rather are scientific and philosophical problems associated with understanding human nature, and human relationships with the natural environment, in ecological terms. I argue that environmental philosophy (and philosophy generally) would benefit from reconceiving itself as a **general philosophy of ecology**, and sketch the outlines of such a discipline, one whose central aim is to understand the nature of ecological phenomena.

Chapter 1 is a survey of theoretical positions in environmental philosophy. This chapter serves as an introduction to the problems that occupy contemporary environmental philosophers.

Chapter 2 tries to make the case that the core philosophical problems of environmental philosophy, which are typically conceived as ethical, social and political problems, are really problems for an expanded ecological science and philosophy, one whose subject matter is the study of ecological phenomena in a wide range of natural and social systems.

Chapter 3 offers an argument for the desirability, both for environmental philosophy and for philosophy generally, of reconceiving environmental philosophy as a **general philosophy of ecology**. The vision of ecological science that is articulated in Chapter 3 is one that identifies ecology not with any particular set of theories or methodologies for the study of natural, nonhuman ecological communities (what I call "traditional ecology"), but rather with the study of system-environment relationships wherever these may be relevant for a complete understanding of a given phenomenon. On this more expansive account of ecological science, for example, cellular metabolism may be conceived as an ecological phenomenon, one that cannot be understood without reference to the difference that environmental situatedness (both intra- and extra-cellular) makes to the dynamics of cellular functioning. Similarly, perception and action in biological organisms may be conceived as ecological phenomena, insofar as the relationship of an organism to its biotic and abiotic environment is an essential feature of the phenomena of perception and action.

An important point that I emphasize in Chapter 3 is that there *already* exist a variety of research traditions in fields outside of traditional ecology, such as psychology, anthropology and economics, that conceive the various phenomena within their domain of study as ecological phenomena, and that bring many of the conceptual resources of ecological science to bear on the study of these phenomena. I argue that a more expansive conception of ecology would see these research traditions as engaged in a common scientific pursuit.

At the end of Chapter 3 I describe three different forms that a philosophy of ecology might take. The first form is a philosophy of ecology modelled on contemporary philosophy of biology and physics, where the special science in question is identified with traditional ecological science (population and community ecology, biogeochemistry, etc.). There is already a small but growing number of philosophers of science who write on conceptual and methodological issues in traditional ecological science. The second form is a broader investigation of organism-environment relations as these relate to the phenomena of perception, cognition, action, and evaluation. This form of philosophy of ecology, as I conceive it, would overlap with areas of epistemology, value theory, and the philosophies of mind, psychology, language, and biology that employ a so-called "ecological approach". The third form is a philosophy of ecology conceived as a general perspective on scientific and philosophical issues, one that examines any subject matter through the lens of ecological concepts and theories. The analogy I have in mind is with feminist theory, whose subject matter is conceived broadly enough to be applicable to fields as diverse as epistemology, ethics, history, science, literary theory, film theory, and politics.

Part Two: *Elements of a Unified Ecology*

As described in Chapter 3, the domain of ecological phenomena ranges over physical, biological, artificial, social and conceptual systems, and hence is not the domain of any traditional natural science. In the chapters of Part Two I consider to what extent one *could* have a unified science of ecological phenomena that is broad enough to address ecological phenomena in all these areas.

A reasonable place to start looking for unifying ecological concepts and theories is traditional ecology, the natural science of ecological systems that is taught in university departments of biology and ecology. In Chapter 4 I argue that traditional ecology, as it is currently conceived and practiced, is a fragmented discipline, broken into ecological subdisciplines that lack a shared conceptual and theoretical foundation. More specifically, I show that the study of "demographic" and "evolutionary" ecological phenomena is theoretically and professionally segregated from the study of "physiological" and "systems-level" ecological phenomena. I suggest that a unified ecology requires that both types of phenomena be integrated within a common conceptual framework, one that reveals the mutual influences and dependencies between the two broad categories of ecological process.

In the same chapter I argue that a plausible candidate for such a conceptual framework may be found by conceiving ecological science as a **complex systems science** within which the concept of a the **ecological niche** plays a central role. This suggestion is inspired by work being conducted by theorists in philosophy, theoretical biology, ecosystem ecology, and the complex systems sciences. A complex systems framework offers a means for relating evolutionary and ecological phenomena, while the niche concept allows for a more fine-grained, contextual analysis of the relationship of organisms and species to the ecological environment. A goal for a unified ecological science, I argue, is to develop a complex systems approach to the ecological niche.

The following two chapters are devoted, respectively, to complex systems approaches and the history and use of the niche concept, in ecological theory. Chapter 5 is an introduction to complex systems approaches in ecosystem ecology. The idea of a "complex systems theory" is a relatively new notion for science (and the philosophy of science), but there are already several different varieties of such theory circulating in the scientific literature. In this chapter I argue that there is a distinctive tradition of complexity theory that has its roots in theoretical ecosystem ecology, and that this tradition has certain virtues that make it particularly suitable for the conceptual framework that I envision for a unified ecological science.

Chapter 6 is devoted to the niche concept in ecology. I survey the classical niche concepts, and show how a systems-oriented conception of the niche may be represented within a network-theory formalism derived from complex systems approaches in ecosystem theory (specifically, from the work of ecosystem theorist Bernard C. Patten).

Chapter 7 is an important one for the overarching argument of the dissertation. The broad aim of the thesis is to defend the plausibility of a unified ecological science that is broad enough to serve the needs and interests of workers in philosophy, and other areas of science outside of traditional ecology. In this chapter I draw on the theoretical framework of **ecological psychology**, a branch of cognitive science that conceives the phenomena of perception and action in ecological terms, to help buttress this claim. I argue that ecological psychology, the theoretical brain-child of perceptual psychologist J. J. Gibson, ought to be viewed as a legitimate branch of ecological science, and that traditional ecological science may be well-served by encorporating some of the theoretical concepts and experimental methodologies of ecological psychology into the corpus of traditional ecology.

In Chapter 7 I show that the contribution of ecological psychology to the problems of traditional ecology is grounded in its novel conceptualizations of the two notions that are the dominant unifying themes of Part Two: the niche concept and complexity theory. Ecological psychology offers a novel conception of the niche in terms of the "affordance structure" of an ecological environment, and in so-called "neo-Gibsonian" ecological psychology, encorporates this niche concept within a novel complex systems framework for modelling biological and ecological phenomena based on the notion of a "perception-action" cycle. I show also that the Gibsonian conception of the niche shows remarkable affinities with the systems-oriented niche theory of Bernard Patten, outlined in Chapter 6. I regard such conceptual convergences in otherwise separated and independent ecological subdisciplines as evidence for the unifying potential of these concepts.

Because ecological psychology is a science of animal and human *cognition* and *behaviour*, the resulting synthetic ecological science offers a framework for philosophical studies into the ecological dimensions of perception, action, and evaluation, bringing it into contact with a host of traditional philosophical problems. Thus, my positive candidate for a unifying theoretical framework for ecological science, and for the philosophy of ecology, is a synthetic, complex systems theory of system-environment relations that exploits the Gibsonian concepts of "affordance" and "ecological information", and the neo-Gibsonian, complex systems notion of a "perception-action" cycle.

In a concluding discussion to Part Two, I take some time to address the relevance and potential of a unified ecological science, particularly one that encorporates Gibsonian concepts, for the traditional normative problems of environmental philosophy.

Part Three: *Understanding Complex Systems Theories*

In the dissertation I propose that a general ecological science ought to be viewed as a *complex systems* science, a science that studies phenomena that are realized or instantiated within a broad class of physical, biological and social systems. The nature of such phenomena, and of the theories that aim to describe them, is a new, emerging field of study in science and the philosophy of science. In the chapters of Part Three I consider general questions of the structure of theories and the nature of explanation in the complex systems sciences.

Chapter 8 was written as a commentary on an article published in 1994 by James Franklin entitled "The Formal Sciences Discover the Philosopher's Stone". This chapter has been accepted for publication in *Studies in the History and Philosophy of Science*, and appears here in mostly unmodified form. In his 1994 article, Franklin argued that the complex systems sciences, or as he calls them, the "formal" sciences, ought to be understood under the model of *applied mathematics*, as sciences of formal mathematical structures. This would explain the seeming "domain-independence" of the formal sciences, since mathematical structures are abstract relational structures, and are in no way dependent for their character on the material composition of systems that instantiate them. Franklin also argued that the proposition that the formal sciences are mathematical in character, in conjunction with that the proposition that the formal sciences describe (structural) properties of real physical systems, entails that the empirical knowledge generated by the formal sciences has the deductive certainty of mathematical knowledge. This is a very strong claim, since it runs counter to widely held intuitions that, to paraphrase Einstein, insofar as mathematical propositions refer to physical reality, they are uncertain, and insofar as they are certain, they do not refer to physical reality. I argue against both the claim that the formal sciences should be understood as (in general) purely mathematical sciences, and the claim that (in general) the knowledge generated by the formal sciences has the deductive certainty of mathematical knowledge. What I suggest, instead, is that the phenomena represented and studied by the complex systems sciences are generated through a complex interaction of formal (necessary) and physical (contingent) constraints, and that the domain-independence exhibited by these sciences is thus both formal and physical in character; formal in the mathematical sense described by Franklin, and physical in the sense that the physical constraints involved are extremely weak and thus easily satisfied by a broad range of natural systems.

The type of physical constraint I have mind is reminiscent of Einstein's "principle"/"constructive" theory distinction. **Principle** theories, according to Einstein, are theories that impose general physical constraints on all physical phenomena. **Constructive** theories are those that posit hypothetical constituents of natural systems (such as the postulate of molecules as rigid spheres in the kinetic theory of gases), which, when suitably constrained by a principle theory (such as Newton's laws of motion), allow one to explain, via deduction, a **phenomenological regularity** (such as the ideal gas law).

In Chapter 9 I extend Einstein's distinction to complex systems theories in general, using complex systems theories in ecology as my model. I suggest that for complex systems approaches in ecology, the principle theory is **thermodynamics**, while the constructive theory is some form of **network** or **systems theory**, and the phenomenological regularities to be explained are the phenomena of **development** and **self-organization** observed in complex systems. These distinctions are used to argue against the position, widely shared among complex systems ecologists, that these complex systems phenomena are properly conceived as manifestations of a new, "fourth" law of thermodynamics.

In this dissertation I propose a new way of doing environmental philosophy, a way that would transform and broaden the discipline to such an extent that the traditional designation, with all of its entrenched associations with environmentalism, may no longer seem appropriate. We come closer to the conception that is defended here when we use the term "philosophy of environment", interpreting this expression to mean the philosophical study of the significance of the concepts of environment and environmental situatedness in any given field of inquiry, be it ethics, epistemology, or physics. Yet the study of environment is also, necessarily, the study of that which is environed. "Ecology" is more suggestive of a field that studies system-environment relationships, and the term already carries with it an established network of conceptual connections to the problems of environmental philosophy and ecological science that, I have found, constitute a set of useful resources for articulating the theoretical positions advanced in this dissertation. For these reasons, I have found the expression "general philosophy of ecology" to be both accurate and suggestive of the particular vision of science and philosophy presented here.

Chapter 1

What Environmental Philosophy Is Today: A Survey of Theoretical Positions

Introduction

Environmental philosophy, as it is currently practiced can be divided into two related but distinct theoretical projects, "environmental ethics", and "radical environmental philosophy" (also known as "radical ecology" or "political ecology" by some practitioners). Very broadly, environmental ethics is concerned with the *moral* dimensions of the relationship between human beings and nonhuman natural entities (animals, plants, rivers, forests, etc.). One can distinguish two competing approaches to environmental ethics, "anthropocentric" (human-centred) and "nonanthropocentric" (nonhuman-centred). Radical environmental philosophy, by contrast, focuses on the historical, cultural, religious and political roots of contemporary environmental attitudes and practices, typically with a view to *changing* those attitudes and practices. What follows is a survey of the various theoretical positions within these three broad categories — anthropocentric environmental ethics, nonanthropocentric environmental ethics, and radical environmental philosophy. The aim of this survey is to familiarize the reader with the role that ecological concepts play in environmental philosophy, as background for the discussions of chapters two and three.

1. ENVIRONMENTAL ETHICS

It is typical to distinguish environmental ethics from traditional moral philosophy by saying that traditional moral philosophy employs a human-centred or "anthropocentric" conception of value, viz. the view that only human welfare and interests have intrinsic moral worth or value, with the consequence that the value of the nonhuman world is conceived only in relation to human welfare, interests and values. The intuition upon which an environmental ethic is based, it is said, is the intuition that this conception of value is false, that humanity is not the sole bearer of intrinsic value in the world, that the nonhuman world possesses value in and for itself independent of human needs and interests. On this view, the challenge of environmental ethics is to come up with a non-anthropocentric theory of value upon which a properly environmental ethical theory can be based, i.e. one that countenances direct moral obligations to the nonhuman world[1].

I am inclined to define the field of environmental ethics somewhat more broadly, as *that field whose primary concerns are with the moral or normative dimensions of human-nonhuman relationships*. This definition makes no commitment to a particular form of value theory, and may include anthropocentic approaches such as one finds in economics or traditional moral and political theory. It also includes the sorts of investigations which fall under the heading of "environmental policy". We can therefore distinguish between *anthropocentric* and *nonanthropocentric* approaches to environmental ethics.

Anthropocentric Environmental Ethics

There are a variety of ways of defending ethical obligations with respect to the nonhuman world, such as obligations to preserve wilderness, reduce population growth rates, etc., which make no appeal to the intrinsic value or direct moral considerability of the nonhuman world. I identify four broad categories of approach within anthropocentric environmental ethics. The first two focus on applications of traditional moral and political philosophy in environmental ethics, while the second two are concerned with the relationship between economic theory and the environment.

---

[1] See, for example, Rolston 1993.

Ecologically-Informed Rational Self-Interest

One way of motivating environmental concern is simply to inform people of the diverse ways in which the welfare of human beings is dependent on a healthy, sustainable relationship with the natural environment, and how human activities threaten this relationship. Environmental writers will often preface their discussions with a survey of environmental "problems", such as overpopulation, greenhouse warming, species extinction and biodiversity loss, resource scarcity and overconsumption. Prior to any considerations of the intrinsic value of the natural world and our obligations to respect that value, such discussions serve to stimulate our sense of self-concern and concern for family, friends and local community, if not for humanity at large. This form of argumentation is closely tied to empirical and scientific issues concerning the reality and seriousness of environmental problems. As a species of moral argument it assumes nothing more than a healthy commitment to one's own welfare and the welfare of those near and dear.

Social Justice

A traditional focus on fairness and justice issues can be applied to problems concerning human-human interactions with respect to the natural environment. For example, one can discuss exploitative First World-Third World relations and their connection to environmental degradation, or conflicts between individual property rights and state regulation of resource usage, or discrimination against minority groups in the selection of areas in which to establish toxic waste dumps.

One can also employ traditional theories of social justice to generate general moral imperatives with respect to environmental issues, though applications of such theories typically require some modification of their original formulations. For example, proponents of a Rawlsian approach to social justice may modify the characteristics of the rational individual situated behind the "veil of ignorance" to include ignorance not only of physical and mental capacities, sex, social class and race, but also of the *time period* in which one is born; one would not know whether one will be be born in the year 1990, 1940, or 2150. The rational individual, the Rawlsian might argue, would not agree to the terms of a social contract that made it possible for one generation to destroy the resource base upon which future generations depend. Such a modification of Rawlsian social contract theory can be used to defend policy initiatives which promote reduction of human population growth rates, wilderness preservation and conservation of biodiversity, reduction of resource consumption and pollution rates, sustainable development, and so on[2].

Environmental Economics

Traditional neo-classical economic theory is grounded in a vigorously anthropocentric value theory. What is good-in-itself is variously described as human happiness or the satisfaction of human preferences (or the positive inner quality of feeling which attends the satisfaction of preferences); all other things are valuable only insofar as they contribute to the satisfaction of human preferences. In neo-classical economic theory the exchange value of goods and services in a market economy is regarded as a reliable measure of the degree of satisfaction experienced by the participants in an economic exchange. Hence, measures such as GNP, which track the total amount of money changing hands in the economy, are regarded as useful measures of the overall welfare of individuals within society.

Environmental economics is a branch of neo-classical economics that studies the costs and benefits of natural resource use in ways that accurately reflect individual and social preferences regarding the use those resources. The aim is to make visible the real individual and social costs of resource production and environmental degradation, and to develop regulative principles and/or

---

[2] See Thero 1995 for a survey of Rawlsian approaches to environmental ethics. For other examples see Scriven 1997, who develops an environmental ethic based on libertarian principles, and de Shalit 1995 for a communitarian approach.

market-based incentives that promote environmentally-friendly government and business practices[3].

Ecological Economics

Ecological economics differs from neo-classical environmental economics in several respects. First, it conceives and models economic systems as real biophysical systems subject to biophysical constraints (such as the first and second laws of thermodynamics), not as systems of abstract flows of exchange value. Second, it emphasizes limits to resource consumption and economic growth and seeks models of economic development that do not dependent on continually increasing consumption and production. Third, it does not shy away from ethical and political considerations that are not reducible to individual human preferences measured by monetary flows (Costanza et al. 1996).

Ecological economists model the "value" of natural resources in different ways (for example, in terms of thermodynamic work potential), but in the final analysis they are all forms of instrumental value accounting. Though there is no commitment to nonanthropocentric values built into the framework of ecological economics, ecological economics has been used to defend social and environmental policy initiatives that conform with the views of many nonanthropocentric environmental philosophers (e. g. Daly 1996).

Nonanthropocentric Environmental Ethics

For many environmental philosophers, anthropocentric approaches to environmental ethics express an indefensible "speciesism" with respect to the objects that they deem worthy of moral respect. Nonanthropocentric approaches to environmental ethics aim at justifying the extension of the domain of morally considerable entities beyond the boundaries of the human community. The project of defending a nonanthropocentric theory of moral value is characterized

---

[3] See, for example, Baumol and Oates 1988.

as a search for a set of criteria for moral considerability. What sorts of entities are entitled to consideration in our moral deliberations, and what is it about those entities, what specific morally relevant properties do they possess, that warrants such consideration?

Among nonanthropocentric approaches there is a theoretical progression which is sometimes called the "expanding circle" of moral considerability. Alternate theories are typically presented in order of increasing inclusiveness, from humans to sentient animals, to all organisms and species, to whole ecosystems, and finally to the global ecosystem, or "Gaia". I follow this convention in the presentation below.

Sentience-Based Approaches

Sentience-based approaches to environmental ethics regard the capacity to feel pain or pleasure as a morally significant natural property, and hence regard any creature capable of feeling pain or pleasure as having a welfare that matters morally. On this approach, pain and suffering are regarded as intrinsically bad (all other things being equal), and since many animals are capable of experiencing the same sorts of pain and suffering as humans, it follows that we have an obligation to take the interests of such creatures into account in our moral deliberations. Precisely which organisms are capable of experiencing pain and pleasure is a contested issue, but there is general agreement that all mammals fall into this category.

Arguments for the reduction of pain and suffering among animals are used to support prohibitions against the use of factory farming practices, certain kinds of animal experimentation, sport hunting, and meat eating. Peter Singer 1990 and Tom Regan 1983 are the canonical formulations for a sentience-based environmental ethic[4].

---

[4] The brief outline given here reflects Singer's utilitarian-inspired treatment more than Regan's, who defends a rights-based account in opposition to Singer's. But

Life-Based Approaches

Life-based approaches to environmental ethics seek to expand the circle of moral considerability to include all living organisms, not just the sentient organisms. Sentience may be a sufficient criterion for moral considerability, but why should it be necessary? Life-based approaches regard behaviours directed at self-preservation, self-maintenance, and the perpetuation of internal organization and structure, as morally relevant properties, for these, it is argued, allow one to talk about the "goods" or "interests" of an organism quite independently of an organism's capacity to be consciously aware of these goods or interests. Don't all living organisms strive to maintain themselves in the face of environmental disturbances? Don't they act in such a way as to maintain their internal organization and structure, avoiding things that might harm them and pursuing things that benefit them? Is it not legitimate to say that organisms value their own lives intrinsically? Life-based approaches to nonanthropocentric environmental ethics attempt to ground the notion of a morally relevant interest on properties characteristic of all living organisms (Goodpaster 1978; Taylor 1981; Johnson 1991).

Of course, from the premise that organisms value their own lives, it does not follow immediately that they possess the sort of value that should figure into human moral deliberations. A substantive theory of nonanthropocentric value which is intended to ground human obligations towards organisms must justify the inference from the claim that organisms "intrinsically value" their own welfare (value as verb), to the claim that this welfare possesses "intrinsic value" which is worthy of our moral respect (value as noun). This is difficult to argue given certain widely held conventions of moral theorizing, such as the

---

both hold the view that the notion of a morally relevant interest is grounded in the capacity to have experiences, and for our purposes this is the important feature of their views.

prohibition against inferring an "ought" from an "is" (the naturalistic fallacy). A not uncommon strategy among environmental philosophers for dealing with this problem is to appeal to ethical traditions that do not regard the naturalistic fallacy as a fallacy. A popular choice is some form of Aristotelian ethical naturalism, a tradition that essentially reduces talk of intrinsic moral value to talk of natural purpose, function, or goal-directedness (e.g. Johnson 1991).

Among life-based approaches to nonanthropocentric environmental ethics one can identify approaches that make a special case for the value of *species*, above and beyond the value of their member organisms. The killing of a blue whale may be a bad thing, but the killing of the *last* blue whale is, for many, a significantly worse thing; it is the termination not only of an individual organism, but of a *type* of organism. As Holmes Rolston has put it, species extinction marks not only the end of life, but, for the type of organism in question, the end of *birth* as well (Rolston 1993).

Nonanthropocentrists who share these moral intuitions must ask what it is about a species that makes its extinction such a bad thing. Answers to this question inevitably make reference to debates within the biological literature regarding the metaphysical status of the "species" category; i.e. whether a species is a merely theoretical construction within biological science, a natural kind, a class, or an individual. Proponents of the intrinsic value of species require that species be entities with sufficient objectivity and individuality to function as bearers of value independent of human valuations and interests. The usual strategy is to claim that species exhibit some of the properties mentioned above as morally relevant within the life-based approach, such as self-preservation, self-maintenance, or perpetuation of internal organization or structure over time[5].

---

[5] See, for example, Johnson 1991 and Rolston 1993. Johnson, like many environmental philosophers who seek to defend the moral considerability of species, makes appeal to the conception of species as *individuals* (as opposed to

The policy implications of a life-based ethic are, of course, broader than those associated with a sentience-based ethic. Once a life-based conception of value is established, one can develop arguments defending biodiversity, habitat and wilderness preservation policies by appealing directly to the intrinsic value of organisms and species.

Ecosystem-Based Approaches

Life-based approaches to environmental ethics make no attempt to argue for the intrinsic value of the natural environment as such. Soil, water, air, nutrients, and sunlight are viewed as instrumentally necessary for the existence and maintenance of life, but they have no "good" of their own; intrinsic value inheres only in the natures and activities of individual organisms. Yet some environmental philosophers want to extend the concept of intrinsic value to ecological entities such as lakes, forests, and deserts. Such "ecosystem-based" approaches require a holistic conception of value applicable to the entire network of biotic and abiotic components which constitute functioning ecosystems.

I identify three distinct types of ecosystem-based approach which are not often distinguished within the environmental philosophy literature. I call these the "process-functional", "population-community" and "natural history" approaches to ecosystem-based environmental ethics.

i) Process-Functional Approaches

A holistic conception of nonanthropocentric value can be generated from considerations similar to those that motivate a life-based value theory. Properties of self-regulation and self-maintenance are certainly characteristic of organisms, but why, it is asked, should they be restricted to organisms? Is it not possible that *whole ecosystems* may exhibit such properties?

---

classes or natural kinds) developed by Michael Ghiselin and David Hull (Ghiselin 1987; Hull 1978, 1987).

There is a tradition of theoretical ecosystem ecology, sometimes called the "process-functional" tradition in the ecological literature, which argues that ecosystems exhibit a number of system-level properties that are similar (if not identical) to the properties that generate morally valuable goal states for life-based theorists. Such properties may include homeostasis, homeorhesis, stability against perturbations, and self-organization. The argument for the intrinsic value of certain states of natural ecosystems is structurally identical to the argument for the intrinsic value of certain states of organisms[6] (e.g. Johnson 1991).

ii) Population-Community Approaches

It is a debated empirical question whether ecosystems actually do exhibit the properties suggested above. The tradition of "population-community ecology" is more sceptical of claims concerning goal-directed behaviours at the ecosystem level of organization. This tradition of ecological theorizing views ecosystems as communities of species populations whose composition and dynamics are determined more by historical contingency and probabilistic effects at the level of interacting populations, than by ahistorical and deterministic factors operating at the ecosystem level (Simberloff 1980). Environmental philosophers who are skeptical that ecosystems actually exhibit the necessary cohesion, systemic integrity or individuality necessary to support properties such as self-organization or goal-directedness must offer a different kind of argument for valuing whole ecosystems (Callicott 1996).

One approach is to value the ecosystem as the necessary biogeochemical *context* in which life processes occur. Species populations both adapt to and modify their biotic and abiotic environments, in such a way that the process of evolutionary adaptation and change cannot be separated from the network of

---

[6] See O'Neill et al. 1986 for a discussion of "process-functional" and "population-community" approaches in ecology.

biotic and abiotic interactions which contitute ecosystems. Thus, to value life processes is to value the whole tangled web of organism-environment interactions which participate in and generate those processes (Rolston 1993). Another approach is to adopt a form of life-based communitarianism, by analogy with communitarian approaches in traditional moral and political philosophy (Leopold 1949; Callicott 1996; Katz 1997). All such approaches I classify as "population-community" approaches to nonanthropocentric environmental ethics.

iii) Natural History Approaches

The process-functional and population-community approaches discussed above may be used to justify valuing ecosystems or ecological communities as wholes, but some environmental philosophers feel that by themselves, these approaches fail to account for (what is for them) a strong moral intuition regarding the respective value of "wild" nature, untouched by human hands, and "tamed" or "cultivated" nature, which bears the mark of human use and activity. Consider an ecosystem that has developed over hundreds of thousands, perhaps millions of years, free from human influence. An ecosystem-based ethic will confer a certain value to this ecosystem. Now imagine the ecosystem is burned to the ground, and a process of human-engineered ecological restoration is initiated such that over a (relatively) short period of time the ecosystem regains the same species composition and diversity that it had prior to being burned. The question is, does the restored ecosystem have the same value as the original ecosystem?

A major ontological difference between the two ecosystems is that they have radically different natural histories. One is the product of a continuous process of biological evolution spanning millenia, the other is a recent product of human invention, more a human cultural artifact than a "natural" object. Some environmental philosophers want to attribute a different and greater value to the natural ecosystem than to the restored ecosystem (e.g. Elliot 1982; Katz 1997).

They want their value theory to reflect the specific value of "wild" nature, the intuition that natural history contributes to the metaphysical identity of a natural system, and hence to the kind of value which it possesses. One might, for example, ground the normativity of natural history by analogy with arguments for the nonsubstitutability of persons or other objects whose value seems to depend on their history. Most of us would not, for example, be indifferent to the choice between living with a loved one and living with a molecule-by-molecule replica of a loved one who was vaporized moments ago, or to the choice between owning a family heirloom and owning a molecule-by-molecule replica of that hierloom. Given arguments that would justify such moral intuitions, one might construct analogous arguments which justify valuing "wild" nature over human-engineered nature[7].

I call this approach to ecosystem-based environmental ethics the "natural history" approach because ecologists in the natural history tradition of ecological science specialize in accumulating detailed historical knowledge of the ecology of specific natural communities. Such ecologists are less concerned than process-functional or population-community ecologists with developing general theories of ecosystem behaviour. Thus, the term captures this focus on the priority of natural history over ahistorical considerations in evaluating the value of ecosystems.

iv) Gaian Approaches

It is a short step from considerations of the value of regional, local ecosystems to consideration of the value of the global ecosystem (or "Gaia", named after the Greek goddess of the Earth). The so-called "Gaia Hypothesis", the brainchild of scientist James Lovelock, is the claim that global biogeochemical cycles,

---

[7] Neither Elliot (1982) nor Katz (1997) ground the argument for the *value* of natural ecosystems by explicit analogy with the nonsubstitutability of persons or

dynamical interactions between biotic organisms and the physical and chemical environment of the earth's surface and atmosphere, function to maintain conditions on Earth favourable to the continuing existence of life. Lovelock argues that Gaia exhibits goal-directed behaviour analogous to certain homeostatic mechanisms observed in organisms (Lovelock 1988).

If one assumes the truth of the Gaia Hypothesis then one can apply the same arguments used in life-based approaches to talk about the value of entities that exhibit goal-directed behaviour, and conclude that Gaia has intrinsic value, a "good" of its own. But from the perspective of nonanthropocentric value theory, the ethical and policy implications of taking Gaia seriously are unclear. Beyond an injunction against blowing up or razing the Earth, it is difficult to derive ethical implications relevant to the spatial and temporal scale of human activities. Gaia is "interested" in the continuation of *life* on Earth, not any specific *species* of life.

For this reason, most of the applications of the Gaia concept in environmental philosophy have not been in the area of nonanthropocentric value theory. Gaia has been more influential in helping defend and articulate various religious, spiritual, and mystically oriented eco-philosophies, including certain forms of Goddess worship and "new age" forms of Deep Ecology (e.g. Badiner 1990 and Russell 1991).

Even if Lovelock's hypothesis turns out to be false or scientifically unverifiable, the Gaia concept has inspired the development of a global earth science perspective on environmental issues, which most regard as a welcome contribution to contemporary environmental science.

---

heirlooms; I use these examples to motivate what I think is the underlying logic that is distinctive of the natural history approach.

2. RADICAL ENVIRONMENTAL PHILOSOPHY

Radical environmental philosophers offer broad-based analyses and critiques of entrenched economic, social, scientific, religious and intellectual traditions which they believe are responsible for modern society's "dysfunctional" relationship with the natural environment. The different schools of radical environmental philosophy are distinguished by which tradition is viewed as the primary bearer of responsibility for environmental degradation. The main positions are known as Deep Ecology, Social Ecology, Socialist Ecology and Ecological Feminism.

Deep Ecology

The term "Deep Ecology" was coined by Norwegian philosopher Arne Naess in his seminal 1973 paper "The Shallow and The Deep, Long-Range Ecology Movement". Deep Ecology is broadly associated with a commitment to the existence of nonanthropocentric value in the nonhuman world, and the belief that the primary cause of human degradation of the natural environment is the inability of individuals within societies governed by anthropocentric value systems to *experience* the nonhuman world *as* intrinsically valuable. The conceptual and value systems of modern Western cultures reinforce the view that human beings are separate from and above the natural world, that nonhuman nature is merely a resource for human use. This conceptual framework makes it difficult for individuals to establish meaningful relationships with nonhuman nature, to relate to nature in any way other than as a resource or instrument for human use.

One might expect Deep Ecologists to be most concerned with elaborating and defending the sorts of nonanthropocentric value theories described above, but this is not the theoretical focus of Deep Ecology. Deep Ecology is more concerned with moral psychology and the phenomenology of moral experience than with abstract moral theory and argumentation. Deep Ecologists want to encourage the development of capacities to experience the world as intrinsically valuable, by analogy with the way that humans experience their own welfare as intrinsically valuable. We don't, after all, need moral arguments to persuade people to value their *own* welfare. The Deep Ecology strategy is to promote experiences of identification and meaningful relationship with nonhuman nature, so that as a matter of *psychological habit* rather than rational compulsion, we will come to value the welfare of nature as we do our own welfare. We are encouraged to expand our identifications, our sense of "self", to encompass elements of reality beyond the boundaries of our skin, and in so doing we will automatically alter our attitudes and practices towards those elements of reality.

Philosophically, Deep Ecology is not commited to any particular ethical or metaphysical foundations. In this respect Deep Ecology is a deeply pragmatic philosophy; any conceptual system will do, as long as it promotes the right sorts of identifications with the natural environment. This explains the propensity of Deep Ecologists to mine different intellectual and religious traditions for conceptual resources to help articulate the Deep Ecological perspective. On the side of the angels are various forms of Eastern philosophy (Zen Buddism, Hindu Vendanta, Taoism), Native American and other aboriginal worldviews, various process-oriented, monistic philosophies in the Western tradition (Heraclitus, Spinoza, Whitehead, Heidegger), and twentieth century developments in physics and ecology (cosmology, quantum mechanics, relativity theory, systems theory, ecosystem theory) (Devall 1980).

The common theme running through these diverse philosophical and intellectual traditions (as interpreted by the Deep Ecologist) is an opposition to principled metaphysical dualisms between self and non-self, subject and object, mind and matter, and fact and value. By coming to see ourselves and the world as interdependent, interpenetrating and interdefining, we enable the expansion of our sense of self and our identifications with the nonhuman world.

Social Ecology

"Social Ecology" is identified most closely with the writings of Murray Bookchin (1982, 1990). Social Ecologists are skeptical of the efficacy of mere conceptual change to effect broad changes in human practices towards nature. For the Social Ecologist, human oppression of nature is not a product of anthropocentric conceptual frameworks, but a manifestation of the same forces which are responsible for the oppression of humans by humans. Social Ecology sees human oppression, defined broadly as the suppression of individual freedom and self-development, as a structural feature of institutional relations of domination and hierarchy. Both human freedoms and the freedoms of nonhuman organisms are suppressed within social systems which are grounded in relations of power and domination. The key to a sustainable relationship with the natural world is, according to the Social Ecologist, the realization of a decentralized political environment where semi-autonomous communities are free to construct ecologically friendly modes of living which reflect the diversity of authentic human values and bioregional contexts. Not surprisingly, Social Ecology is sometimes referred to as "Ecological Anarchism".

Social Ecology draws on and synthesizes a number of political and philosophical traditions, most notably a form of naturalized Hegelian dialectic, the 19th century anarcho-communism and evolutionary theory of Peter Kropotkin, and the social theory of Lewis Mumford.

Socialist Ecology

Socialist Ecologists, or "Eco-Socialists", share with Social Ecologists a focus on human social and political systems as key determinants of environmental attitudes and practices, but Socialist Ecologists remain wedded to a broadly materialist conception of history, and of the origins of inequality and oppression in economic class divisions. The "red green" critique focuses on structural features of capitalism which necessitate the economic exploitation of human beings, and the need for greater and greater levels of natural resource consumption. Of particular interest to Eco-Socialists are the dynamics of global capitalism, economic exploitation of Third World populations and resources by

First World countries, and international development issues (Miller 1978, O'Connor 1991).

Ecological Socialism differs from classical socialism in several respects. It replaces the rigid anthropocentrism of classical Marxism, where the socialist state was envisioned as completing the transformation and mastery of nature begun by capitalism, with an ecologically informed theory of the relations of social production to human labour and the natural environment. And it replaces the classical ideal of a centrally managed state political structure with commitments to democracy, internationalism, and ways to overcome the dualism of local versus state control and administration. Eco-Socialists are more aligned with 20th century Marxist humanism and Frankfurt school Critical Theory than traditional Marxism-Leninism.

Ecological Feminism

Ecological Feminism, or "Ecofeminism", is the most complex and varied of the radical environmental philosophies. Ecofeminist views span the range of feminist perspectives and traditions, and Ecofeminists will often identify themselves as Socialist Ecofeminists, Social Ecofeminists, Spiritual Ecofeminists, and so forth. The central theme which unites all forms of Ecofeminism is a commitment to the notion that the oppression of women and the oppression of nature are historically and conceptually linked (e.g "Mother Nature"), that patriarchal conceptual frameworks based on conceptual dualisms and hierarchical value thinking have functioned to maintain male dominance by identifying and de-valuing the "feminine" and the "natural".

Karen Warren offers the following description of a "patriarchal conceptual framework" (Warren 1990). Patriarchal conceptual frameworks define concepts in terms of mutually exclusive, oppositional pairs — reason/emotion, mind/body, logic/intuition, science/art, objective/subjective, culture/nature, male/female, etc. — and in a fashion such that the left-hand category is invariably valued more than the right-hand category. Within patriarchy, these categories are grouped by association, so that reason, the mind, logic, science, fact, objectivity and culture are valorized and associated with the male, while emotion, the body, intuition, art, subjectivity and nature are devalued and associated with the female.

Different Ecofeminists will analyze the relationship between patriarchy and environmental degradation differently, but they all agree that it is impossible to move towards a less destructive, more harmonious relationship with the natural environment without at the same time moving away from patriarchal social, institutional and conceptual frameworks.

Conclusion

This completes our survey of the conceptual landscape of contemporary environmental philosophy. Of course there are fields of study and theoretical approaches within environmental philosophy which have not been discussed here, a listing of which might include such fields as "environmental pragmatism", "ecotheology", "postmodern environmental philosophy", and "environmental aesthetics". But to reiterate, the purpose of this survey is not to be exhaustive, but to put the reader in a position to reflect on the core problems of environmental philosophy, and in particular on the role that ecological concepts and theories play in articulating these problems.

Chapter 2

Ecology And The Problems Of Environmental Philosophy

Introduction

Environmental philosophy is commonly regarded as a branch of moral, social and political philosophy that focuses on the normative dimensions of the relationship between human beings and the natural environment. The core philosophical questions that occupy environmental philosophers are:

1) Do we have moral obligations to protect or preserve the natural environment? If so, what are they, and to whom, or what, are they owed?
2) What are the root causes of contemporary attitudes and practices towards the environment, and how can we change them?

These two questions are not independent (e.g. one of the causes of contemporary industrial culture's exploitative treatment of the natural world may be the presence of widely held beliefs that we have no direct moral obligations toward the environment) but they are logically distinct, and they line up along traditional disciplinary boundaries. Philosophers specializing in ethics and value theory are more likely to contribute to the literature on Question 1, while philosophers specializing in social and political theory are more likely to contribute to Question 2. But there is a third question, not often made explicit, that occupies environmental philosophers of all stripes:

3) How are we to understand the ecological dimensions of human nature and human activity, and what are the implications of such understanding for questions 1 and 2?

Question 3 is concerned with the following sorts of issues:

i) the nature and severity of the environmental crisis,
ii) the nature of ecological limits to growth and resource consumption,
iii) the similarities and differences between humans and nonhuman organisms,

iv) the variety of ways in which human welfare may be dependent on the maintenance of certain relationships with plants, animals and the environment,

v) the role that ecological relationships — between humans and the environment and between competing groups of humans — have played in the organization and development of human societies,

vi) the scientific and philosophical status of ecological and evolutionary science, and

vii) the extension and application of ecological concepts and theories in areas of human thought and experience well outside the boundaries of traditional ecological practice (such as metaphysics, epistemology, cognition, value theory, etc.).

Environmental philosophers will admit that these issues are of primary importance in motivating, articulating and developing various positions within environmental philosophy, i.e. positions which claim to offer answers to questions 1 and 2. But here we see a problem that, I believe, has vexed environmental philosophy since its birth in the early 1970s — namely, that philosophers who are concerned about the environment, who believe that contemporary attitudes and practices with respect to the environment are seriously undesirable, are much more likely to have their theorizing informed by ethical, social and political theory *than by the scientific and philosophical disciplines best suited to addressing the issues raised by Question 3.*

The purpose of this chapter is to substantiate the claim that issues concerning the ecological dimensions of human nature and human activity are of primary importance in motivating, articulating and developing various positions within environmental philosophy. I will develop and discuss a series of examples drawn from the environmental philosophy literature to illustrate this claim. The aim is not to offer detailed criticisms of the various views we will examine (that would require much greater resources than are available here), but to show how deeply philosophy, science and ecology are interwoven in environmental philosophy.

Note that in what follows I assume and make reference to the survey of positions in environmental philosophy given in Chapter 1.

1. Anthropocentric Environmental Ethics

Anthropocentric environmental ethics is concerned with the role that environmental factors play in determining moral obligations to other humans rather than to the environment itself. But what is the relationship between theorizing about "environmental factors" and theorizing about the sorts of alternative economic, moral and political philosophies described in Chapter 1? Roughly, it is one of *constraint*. Environmental threats (greenhouse warming, ozone depletion, biodiversity loss), resource scarcity and exponential growth trends in population and resource consumption are thought to impose novel and severe constraints on viable solutions to the traditional problems of economic, social and political justice. Traditional normative theories were developed in a cultural context which did not recognize natural limits to growth or the multiple ways in which human welfare is dependent on healthy, sustainable relationship with the natural environment. For anthropocentric environmental philosophers the challenge is to expand or modify those traditional normative theories in ways that prescribe the appropriate sorts of changes in attitude and behaviour that are required of us by a recognition of those natural limits and dependencies. Perceived physical, biological and ecological constraints are what drive the normative project.

But is there not a set of prior issues on which any restructuring of normative ethical theories must necessarily depend? Shouldn't we first consider the scientific and philosophical status of claims concerning physical, biological and ecological constraints? The question of how many people *should* live on the Earth is constrained by beliefs concerning how many people *can* live on the Earth, but do we have a good answer to the latter question? Environmental philosophers are aware that there is dispute between members of the scientific community over the nature and severity of many so-called "environmental problems", such as greenhouse warming, ozone depletion, biodiversity loss, and overpopulation, but what are the issues involved in assessing competing claims about the existence or severity of an environmental problem? How should decision-making be conducted under conditions of high empirical uncertainty and high value stakes? These sorts of questions require greater familiarity with the relevant substantive scientific issues and with general issues concerning scientific confirmation and the role that values play in scientific methodology. Yet they have not been given a high priority in environmental philosophy.

The Debate Over Limits to Growth

General philosophical problems concerning the nature and status of ecological concepts and their application to human beings are also important issues in anthropocentric environmental ethics. Consider for example the debate between "cornucopian technocentric optimists" and "ecocentric pessimists" over the existence of natural limits to human growth and prosperity. Environmentalists typically insist that there are natural limits to human population growth and resource consumption. The Earth has a finite supply of usable material resources, it is said, and just like a population of reproducing bacteria in a petri dish, at some point per capita resource consumption of the population will exceed the per capita resource or "carrying" capacity of the environment. Economic growth models which emphasize the material dimensions of resource utilization and carrying capacity will hence often impose rigid limits on growth and resource consumption. For example, the computer model of the "world" made famous in the *Limits to Growth* report (Meadows et al. 1972) assumed that exponential economic and population growth entails exponential resource decline. Because resources are assumed to be finite in the model, when population overshoots Earth's carrying capacity, the population curve plummets due to mass starvation.

The late economist and population theorist Julian Simon was a vociferous defender of free-market economics and infamous denier of the existence of an impending environmental crisis (Simon 1981, 1996). Simon believed that despite — indeed, because of — the continuing exponential growth of human populations, humanity was on the verge of an era of unprecedented economic growth and prosperity. Simon upheld a classical liberal (bordering on libertarian) humanist political ideology, and many people might think that the main disagreement between cornucopian theorists like Simon and more pessimistic environmentalists is over political convictions, but these were not the central feature of his dispute with environmentalists. Rather, the core of his objection to environmentalism involved a disagreement over *what it means to view human beings in ecological terms*.

The environmentalist assumption is that carrying capacity is a measure of material resource availability, but what, Simon asked, is "material" about the concept of a "resource"? If you look out the window and ask yourself "how many resources do I see?", what should you say? The question is difficult to answer because upon reflection one realizes that resources are only resources *to* someone or something *for* some purpose. A thing *becomes* a resource only in relation to some actual or potential use of that thing by an agent. Resources aren't simply "out there" in the world, waiting to be used. Simon argued further that human beings are unique in our ability to *create* resources at will, through the development of new technologies. For example, one would not think of the uranium lying in the ground as a valuable human resource prior to the development of nuclear technology. For Simon, it seemed more appropriate to say that after the Manhattan Project a new resource was created where none had existed before.

The very notion of a natural limit to resource consumption assumes that carrying capacity is fixed for a given population, but according to Simon, environmentalists are simply wrong about this. Human beings are capable of *increasing* the carrying capacity of the Earth through the creation of new resources. Further, since a resource is not a material thing, but a *mode of use of a thing for a given end,* one can create *substitutes* for a given material resource that perform the same function (generating energy, for example) as the original resource. These two notions, of the essential immateriality and infinite substitutability of resources, are used by Simon and other technocentric optimists to argue against the basic environmentalist assumptions concerning resource scarcity and limits to growth.

This example illustrates how a conceptual or interpretive issue (what is the ontological status of the concept of a "resource", and how does this concept relate to human activity?) can function to structure debate on a topic which is of fundamental concern to environmental philosophers, namely the status and severity of the environmental constraints upon which subsequent normative theorizing is based.

2. Nonanthropocentric Environmental Ethics

In anthropocentric environmental ethics, normative theorizing is distinguished from nonnormative theorizing in a fairly straightforward way. I characterized the relationship as one of "constraint"; nonnormative issues (in particular scientific and ecological issues) impose constraints on viable solutions to the traditional problems of ethical and political justice. But the value theory at work in anthropocentric environmental ethics is not itself closely bound up with scientific and ecological issues — the fact-value distinction is clearly visible, at least with respect to facts concerning the nature of ecological theories or the severity of the environmental crisis. This is not the case in nonanthropocentric environmental ethics. Here we observe a rich interplay between value theories, normative ethical theories, and issues in physical, biological and ecological science.

In Chapter 1, I gave a few examples of the ways in which concepts or theories in the physical and biological sciences have been used to support a nonanthropocentric ethic. Here I want to extend that discussion with a closer examination of the positions of two influential nonanthropocentric environmental philosophers, Holmes Rolston III and J. Baird Callicott. Both Rolston and Callicott reject sentience-based and life-based approaches as insufficient for generating a complete environmental ethic that does justice to widely held (among environmentalists, at any rate) intuitions concerning the value of natural entities like species and ecosystems. The value theories of sentience- and life-based approaches are *individualistic* — only individual organisms are regarded as deserving of moral consideration. The challenge for Rolston and Callicott (and all ethical holists) is to justify a value-holism which would extend moral consideration to ecological wholes.

We saw in Chapter 1 how a holistic process-functional approach to ecosystem-based environmental ethics can be modelled on a life-based approach in a natural way. The natural property which is said to warrant moral consideration on a life-based approach is the self-maintaining, self-organizing, goal-directed activity of living systems (since such activity allows one to talk about the "goods" or "interests" of living systems), but process-functional approaches to ecosystem-based environmental ethics assert that ecosystems exhibit similar properties, and are therefore candidates for moral consideration.

The form of this kind of argument is quite general. We can use a typical animal welfare argument to illustrate. The argument begins with the following sort of question: what is the morally relevant property that human beings possess and animals lack which makes it morally acceptable to use animals for food, sport hunting, entertainment, and scientific experimentation, but not humans? Theory 1 presents X as the candidate property (say, rationality, or self-awareness and autonomy, etc.). Theory 2 responds by noting that we do not treat human beings who may lack the property X the same way we do animals (consider infants, children, the mentally challenged, people in comas, and so on). People who hold Theory 1 are charged with *inconsistency* if they do not either a) treat humans who lack the property X as they would animals, or b) extend to animals the moral consideration given to humans who lack property X.

Adopting either a) or b) would resolve the inconsistency, but few people are willing to accept a), so b) appears as the only reasonable alternative[8].

Arguments of this form are used to motivate a weakening of the criterion for moral considerability (say, from rationality to sentience, or sentience to self-preservation and self-organization), with the consequence that the new criterion applies to more sorts of entities than the previous criterion, and hence entails a widening of the domain of morally considerable entities. This much is achieved solely in the name of rational consistency between actions (with respect to the treatment of humans and animals) and beliefs (with respect to the value of humans and animals). But the argument is also used to motivate a reconsideration of the naturalistic foundations of one's value theory. In bringing to one's attention the fact that a rationality-based value theory would entail that young infants and the severely mentally handicapped are not worthy of moral consideration, one is challenged to reconsider just what it is that one values in human life. So although the above argument form does not entail any particular naturalistic foundation for value, arguments that fit the form are often the basis and inspiration for the adoption of a new naturalistic foundation for value.

This kind of argument can be seen in the life-based approach of Kenneth Goodpaster and Lawrence Johnson's extension of it to ecosystems. Goodpaster quotes the cybernetician Kenneth Sayre:

> The typifying mark of a living system . . . appears to be its persistent state of low entropy, sustained by metabolic processes for accumulating energy, and maintained in equilibrium with its environment by homeostatic feedback processes. (Sayre 1976, 91)

Goodpaster goes on to say that

> [g]ranting the need for certain qualifications, a definition such as this strikes me as not only plausible in its own right, but ethically illuminating, since it suggests that the core of moral concern lies in respect for self-sustaining organization and integration in the face

[8] See, for example, the argument structure in Goodpaster 1978, Taylor 1981, Singer 1990, and Johnson 1991.

> of pressures toward high entropy. (Goodpaster 1978, reprinted in Zimmerman et al. 1998, 68)

The idea is that the activity of striving to maintain one's internal organization and structure effectively defines the "goal" or "end" towards which all other organism activities are directed; it is, for the organism, an "end-in-itself", that state which the organism pursues for its own sake. Such activity allows one to talk about the "interests" or "welfare" of an organism, without any requirement that the organism be consciously aware of these interests.

Lawrence Johnson argues in a similar vein concerning the moral considerability of ecosystems:

> [W]e think of an ecosystem as an ongoing process taking place through a complex system of interrelationships between organisms, and between organisms and their non-living environment. The organisms change, and the interrelationships may vary somewhat, but there is a continuity to the ecosystem, and a center of homeostasis around which the states of the ecosystem fluctuate, which defines its self-identity. Normally, an ecosystem maintains its stability through an intricately complex feedback system . . . . However, an ecosystem can suffer stress and be impaired. It can be degraded to lower levels of stability and interconnected complexity. It can have its self-identity ruptured. In short, an ecosystem has well-being interests — and therefore has moral significance. (Johnson 1991, 216-17)

This type of argumentation is characteristic of the process-functional approach to ecosystem-based environmental ethics.

Rolston and Callicott are unwilling to use this type of argument to justify valuing ecosystems as wholes as they question whether ecosystems have the requisite structure, individuality and continuity over time to function as bearers of "interests". As Callicott puts it, "[b]iocentrism . . . represents the end point of this simple line of argument. It stretches this familiar pattern of moral reasoning to its limit." (Callicott 1998, in Zimmerman et al., 13). Note how normative questions within environmental philosophy can be quite sensitively dependent on issues regarding the empirical status of biological and ecological theories. We shall see more of this as we look more closely at Rolston and Callicott's approaches to environmental ethics.

Rolston's Environmental Ethic

A nonanthropocentric environmental ethic has two jobs — one, to give us a reason for extending moral consideration to nonhuman entities *at all*, and two, to tell us *how much* consideration we ought to give (how to weight and adjudicate between competing values). Up to now we have only looked at the first part of the job, but the second is equally important if environmental ethics is to play any useful critical or practical function. A defensible environmental ethic of the sort Rolston and Callicott desire must accord with certain widely held intuitions; that all other things being equal, the value of the life of an ant is less than that of a dog, which is less than the life of a (competent adult) human being; that the death of the last member of a species is a worse and different sort of loss than the death of any individual member of that species; and that ecological communities and ecosystems have a value *as such*, which is not reducible to the aggregate values of the individual member organisms. The history of nonanthropocentric environmental ethics is a history of attempts to justify one or more of these intuitions, but a greater challenge is to justify them all within a coherent and defensible philosophical system.

Callicott gives a useful overview of Rolston's environmental ethic which we will use to frame our discussion. According to Callicott, Rolston's approach is to start with a life-based value theory as the basic infrastructure of his ethic, then augment and extend this value theory in various ways:

> To the equal baseline intrinsic value of living things, each with a good of its own, Rolston adds a value premium, so to speak, for sentience and an additional value premium for self-consciousness. Thus sentient animals possess more intrinsic value than plants and insentient animals; and we self-conscious rational animals possess the most intrinsic value of all individual natural entities. . . . Rolston then awards a value dividend, as it were, to species, the perpetuation of which is the reproductive end of specimens, and to

> ecosystems as the matrix in which baseline intrinsically valuable living things evolved and on which they remain dependent for their flourishing. In Rolston's essentially biocentric system, like the moon that shines by a borrowed light, natural wholes, such as species and ecosystems, possess an intrinsic value derived from the baseline intrinsic value of living organisms and thus enjoy only derivative moral consideration. (Callicott 1998, in Zimmerman et al., 14-15)

Callicott's description is a faithful summary of the bare structure of Rolston's environmental ethic, but it misleads in several places. The impression is that the value of holistic entities is derived from the more fundamental and primary intrinsic value of individual organisms, but such an interpretation fails to appreciate Rolston's basic theory of value. For Rolston, value is to be identified with *creativity*, *spontaneity* and *freedom*. A rock tossed through the air follows a path which it has no power to alter; its behaviour is completely determined by external forces acting upon it. An organism can act to resist local potential energy gradients (it can go uphill where a rock must roll downhill) and the disordering effects of increasing entropy: "Organisms suck order out of their environment, stage an energetic fight uphill in a world that overall moves thermodynamically downhill. They pump out disorder" (Rolston 1988, 97). But the autonomy and creativity of organismic behaviour is not a property of organisms in isolation, but is dependent upon, and constituted by, the ongoing activity of the organism as the momentary expression of an historical and ecological entity. The organism develops and maintains itself over time in conformity with its genetic constitution, a property which is itself the product of an evolutionary history. The genotype stores information about the evolutionary past of the species, those genotypic variations which survived the ongoing test of local adaptation and survival. But this testing is an ecological phenomenon, requiring essential reference to the network of biotic and abiotic interactions which form the selective environment of the organism. The intrinsic value of the organism, then, is always embedded in a larger whole, both spatially and temporally. Ecosystems are not sufficiently centralized and organized to be thought of as *acting* (and hence cannot be bearers of intrinsic value), but their role is even more fundamental, for they are the necessary context within which all value is generated:

> Organisms defend only their selves, with individuals defending their continuing survival and species increasing the numbers of kinds. But the evolutionary ecosystem spins a bigger story, limiting each kind, locking it into the welfare of others, promoting new arrivals, bringing forth kinds and the integration of kinds. Species *increase their kind*; but ecosystems *increase kinds*, superimposing the latter increase onto the former . . . . Though it has value *in* itself, the system does not have any value *for* itself. Though a value producer, it is not a value owner. We are no longer confronting instrumental value, as though the system were of value instrumentally as a fountain of life. Nor is the question one of intrinsic value, as though the system defended some unified form of life for itself. We have reached something for which we need a third term: *systemic value*. (1998, 140-41).

Rather than systemic value being "derived from" the intrinsic value of organisms, it is more accurate to say that in Rolston's environmental ethic it is the system that is of ultimate value in nature. It is valuable for its fertility and creativity in generating values, and all other values, intrinsic and instrumental, are embedded within it.

What is the role of human beings in Rolston's environmental ethic? Human beings are distinctive in their capacity for self-awareness, which has enabled our thought and action to be guided by a conception of the good. This faculty of rational self-awareness and moral conscience allows humans a perspective which transcends the self-absorbed, niche-focused perception of other organisms. In our ability to transcend ourselves and nature, we humans can become aware of the objective, intrinsic values that are independent of us. In our capacity as moral beings, we can respect these values and live appropriately to our surroundings.

More can be said about Rolston's environmental ethic but this should suffice for our purposes. Rolston's ethic is an example of what I called in Chapter 1 a "population-community approach to ecosystem-based environmental ethics", though it also has elements of a "natural history approach". The ecosystem is valued as the necessary productive matrix within which all life evolves and is embedded, but it is not itself a "self" which is the owner of a "good-of-its-own". The naturalistic flavour of the ethic is another characteristic feature, and this ethic is intimately tied up with details of the physical, biological and ecological processes which make up the natural world:

> In environmental ethics one's beliefs about nature, which are based upon but exceed science, have everything to do with beliefs about duty. The way the world *is* informs the way it *ought* to be. (Rolston 1998, 143)

Callicott's Environmental Ethic

Callicott's environmental ethic is inspired by his interpretation of the influential writings of Aldo Leopold (Callicott 1989). Leopold was a forester by training and a wildlife ecologist by profession. His collection of essays, *A Sand County Almanac* (Leopold 1949), is a seminal document for environmentalists and the first influential statement of a holistic environmental ethic. Leopold's famous moral maxim states that

> "[a] thing is right when it tends to preserve the integrity, stability, and beauty of the biotic community. It is wrong when it tends otherwise."

Callicott argues that Leopold's ethical theory is influenced by Darwin's writings on the evolutionary origins of altruistic sentiment. Consider:

> An ethic, ecologically, is a limitation on freedom of action in the struggle for existence. An ethic, philosophically, is a differentiation of social from anti-social conduct. These are two definitions of one thing. The thing has its origins in the tendency of interdependent individuals or groups to evolve modes of co-operation. The ecologists calls these symbioses. Politics and economics are advanced symbioses in which the original free-for-all competition has been replaced, in part, by co-operative mechanisms with an ethical content. (Leopold 1949, quoted in Zimmerman et al., 87)

These modes of cooperation are facilitated by the evolution of altruistic sentiments towards members of one's kin group and larger community. Leopold sees the historical development of human ethical systems as involving an expanding sense of what constitutes the moral community. This expansion is stimulated by increasing levels of social interaction and competition with groups that were formerly outside the morally community, but with whom it would now be advantageous to develop modes of social cooperation. But we now realize that, from an ecological standpoint, human beings are members of a broader *biotic* community. Evolution has bequeathed to us the capacity to extend altruistic moral sentiments towards the members of this biotic community. As a species it is also evolutionarily advantageous for us to do so, as human beings have collectively "acquired the power to destroy the integrity, diversity, and stability of the environing and supporting economy of nature" (Callicott 1998). This extension of the moral community is for Leopold "an evolutionary possibility and an ecological necessity".

As yet we have the beginnings of an argument for a life-based ethic, but no reason for extending moral sentiment to the community *as such*. Leopold's conception of the ecological community is influenced by the organismic conception of the community inspired by Forbes (1887) and developed by Clements (1916), the self-regulating community concept of Elton (1927), and the ecosystem concept of Tansley (1935). The general tendency of this holistic tradition of ecological research is to suggest that there are principles that govern the evolution and development of communities that derive from physical and organizational principles operating at the level of the system as a whole. Thus the object of our altruistic sentiments, if they are to motivate behaviours that are actually conducive to the welfare of the members of the biotic community (in which we are now included), must be directed to the stability and integrity of the whole as well as to the parts. This extension of ethics to whole ecosystems is what Leopold calls the "Land Ethic".

What is the value theory that underlies and gives moral force to the Land Ethic? Leopold is ambiguous on the question of whether the altruistic sentiments we express toward members of the moral community (and to the community as such) are to be justified in terms of ecologically enlightened self-interest (we ought to value nature for its own sake because if we don't we'll suffer the consequences) or in terms of the recognition of intrinsic values in the world which warrant our moral respect. Leopold uses the language of "rights" at times in his discussions of our obligations to the Land, and he was a strong critic of anthropocentric arguments for environmental protection. Yet his sociobiological account of the origins of moral sentiments is grounded in the notion that moral sentiments serve an adaptive function *for us*, making it difficult to interpret moral values as anything other than psychological projections onto the world[9].

Callicott attempts to resolve this dilemma by adopting a Humean-inspired subjectivistic conception of value that still allows one to talk about the intrinsic value of objects in the world. There is no value without a valu*er*; value is a verb first and a noun only derivatively. Something has value if and only if it is valued. But granting that all value is subjective in origin, one can still distinguish between objects that are valued for their own sake, and objects that are valued for the sake of what they can do for the valuer. This is how Callicott chooses to define "intrinsic" and "instrumental" value, respectively. Intrinsic value, interpreted subjectively, is that to which we are disposed to feel love and/or respect. For the natural world to have intrinsic value is simply to say that it elicits such other-regarding sentiments in the valuer.

So from the "inside", from the perspective of the community member with the appropriate moral sensitivities, obligations toward the community are

---

[9] For readings of Leopold's famous maxim, "a thing is right when it tends to preserve the integrity, stability, and beauty of the biotic community. It is wrong when it tends otherwise", as an anthropocentrically motivated principle for environmental management rather than as an expression of a nonanthropocentric value theory, see Lehmann 1981 and Norton 1991.

experienced as deonticly binding — they require no further justification for the community member. From the "outside", from the perspective of an impartial observer who does not share those sentiments, the justification for valuing the community intrinsically can only been seen as prudential, an adaptive survival strategy made necessary by humanity's increasing capacity to undermine the "integrity and stability of the biotic community".

Callicott's approach to the is-ought problem is quite different from Rolston's. Rolston believes that the naturalistic fallacy should not be regarded as a fallacy, that nature carries intrinsic value and we can come to know that value by paying close attention to what is actually going on in nature. Callicott accepts the Humean convention that one cannot derive an "ought" from an "is", but argues that his environmental ethic doesn't require this. Moral sentiments are not "entailed" by any descriptive fact about a community; they are elicited, stimulated, coaxed, sometimes through rational argumentation, but more often through deep immersion in the realities of life within the community.

In this section we have seen how philosophical issues within nonanthropocentric environmental ethics are deeply intertwined with theoretical issues in biology and ecology. What is an organism? What is life? How does evolution occur? What determines how communities of organisms develop over time? What is an ecosystem? What aspects of human existence are products of our evolutionary and ecological heritage? All of these questions are directly relevant to a philosophical project that aims to ground ethical obligations towards nature in properties of natural systems.

3. Radical Environmental Philosophy

Radical environmental philosophy is distinguished from environmental ethics in its emphasis on the historical, cultural and political processes that give rise to attitudes and practices toward the environment. Radical environmental philosophers are committed to the wrongness of contemporary relationships between human beings and the environment, but they may be anthropocentrist or nonanthropocentrist in their understanding of what makes such relationships wrong. Deep Ecologists are nonanthropocentrists, but Socialist and Social Ecologists typically are not, and Ecofeminists will vary depending on their theoretical orientation. The essential feature of radical environmental philosophy is a commitment to the view that contemporary attitudes and practices with respect to the environment are deeply embedded in historical, cultural, religious and political structures, and that changing these attitudes and practices will require changes in these deep structures.

The discussion which follows will address two questions: i) what is the central philosophical challenge of radical environmental philosophy?, and ii) how are ecological concepts employed in the articulation of the various positions in radical environmental philosophy?

The Central Problem: Social Change

The primary theoretical concern of radical environmental philosophers is the problem of understanding *social change*. Given where we are today, and a conception of where we would like to be, what sorts of changes are required in order to get from here to there? Are changes in material social practices driven by changes in the values and beliefs of a culture, or are changes in values and beliefs driven by changes in material social practices (the "idealism"/ "materialism" debate)? Does social change require collective political action or is it sufficient to target one's efforts at changing the attitudes and practices of individuals (the "collectivism"/"individualism" debate)?

For example, consider the contrast between Deep Ecology and Socialist Ecology. Deep Ecologists believe that what we do to the environment (our economic and technological practices) is determined by what we think about it (our beliefs and value systems), and that the root cause of environmental degradation is the anthropocentric value system of contemporary Western culture. According to the Deep Ecologist, people will treat nature with respect only when they come to value nature for its own sake, independent of its instrumental relationships to human needs and interest. They thus encourage and promote the adoption of alternative "worldviews" which stress the interdependence and interconnectedness of all things, and which ultimately facilitate an expansion of the individual sense of self to include greater and greater identifications with the natural world. The Deep Ecological theory of social change is individualist (social change is driven by changes in the attitudes and practices of individuals) and idealist (social change is driven by changes in the world of "ideas", of beliefs and value systems).

Socialist Ecologists, by contrast, believe that the way we *think* about the environment is determined by what we *do* to it. Capitalist modes of economic production require that the environment be viewed in purely instrumental terms, and only changes in the socio-economic base will suffice to change society's attitudes and practices towards the environment. The Ecosocialist theory of social change is materialist (in the sense of Marx) and collectivist.

Is the problem of understanding social change a problem for normative social and political philosophy? I would suggest that it is, but only peripherally. The problem can be described as follows. A society at a given time and place may be characterized by specifying such things as its religious, scientific, ethical and political beliefs and values, institutional structure, level of technology, economic organization, etc. If you take these categories and identify them with the axes of an abstract space of social "states", then a society can be specified by a point in the space of states. Call the society we live in today "A". The normative problem of social and political theory is the problem of characterizing the ideal state (call it "B"), of finding that point in the state space which is the most morally and politically desirable. The problem which concerns us, however, and which concerns any social change movement, is *how to get from A to B*. This is not a problem for normative social and political theory, I would submit, but a problem for social *science*. The question is, do we have any good theories of how societies *actually* change, of (to push the state space metaphor) the *dynamics* of social change? Normative social and political theory is useful for picking out a privileged set of end points, of finding out which direction we would like to go in the state space, but the exercise will be of little use if we have no idea how social change actually works. Do changes in the world of ideas, in how we think about the world (the conceptual components of our list of social attributes), determine changes in the world of material practice, in what we do to the world (the material components of our list)? Or is it the other way around? Or is it some more complicated dynamic? To a certain extent these questions can be asked and investigated independently of particular moral or political commitments.

Radical environmental philosophy presupposes commitments to both a normative conception of what society should be like, and a conception of how this ideal society can be achieved. The former question is a problem for normative social and political theory, but the latter question is a problem for social science, or the philosophy of social science. The two problems are not unrelated, but they are rarely distinguished in the environmental philosophy literature.

Ecology and Radical Environmental Philosophy

A striking feature of radical environmental philosophy is the use of the term "ecology" in defining its various positions. We have Deep Ecology, Social Ecology, Socialist Ecology, Ecological Feminism, and broader terms such as Political Ecology and Radical Ecology. The term "ecology" is roughly synonymous with "environmentalism" in this usage, and it is easy to suggest that ecological science has little to do with radical environmental philosophy, that philosophers are simply drawing on an association between ecology and environmental concern that goes back to the beginnings of the modern environmental movement in the 1960s. That this is going on is certainly true, but radical environmental philosophers seem to want to draw more from ecology than this simple association with environmentalism, or the mere recognition that applied ecological science may be a valuable tool for predicting environmental impacts and constructing environmental policy. They believe that ecology has something deeper to offer, that it can function as a *lens*, a scientific and conceptual framework through which the ecological dimensions of human activity in the world are revealed. Ecology in this sense is as much a *perspective on the world* as it is a body of knowledge.

Different radical environmental philosophers see different things through the lens. Deep Ecologists argue that ecology reveals the underlying interconnectedness of all living things and the arbitrariness of principled distinctions between Self and Other; we are all nodes in a web of relations and interactions which extend well beyond the boundaries of our skin. Fritjof Capra gives the following description of the "ecological" picture of the world:

> The new paradigm may be called a holistic worldview, seeing the world as an integrated whole rather than a dissociated collection of parts. It may also be called an ecological view, if the term "ecological" is used in a much broader and deeper sense than usual. Deep ecological awareness recognizes the fundamental interdependence of all phenomena and the fact that, as individuals and societies, we are all embedded in (and ultimately dependent on) the cyclical processes of nature. (Capra 1996, 6)

Socialist Ecologists assert that ecology reveals the underlying biophysical dimensions of economic consumption and production, and shows how human history is driven by ecological relationships among human social groups and their natural environments. They insist that environmental problems can only be addressed by conceiving economic problems as ecological problems, and vice versa:

> Socialism needs ecology because the latter stresses site specificity and reciprocity, as well as the central importance of the material interchanges within nature and between society and nature. Ecology needs socialism because the latter stresses democratic planning, and the key role of the social interchanges between human beings. By contrast, popular movements confined to the community, municipality or village cannot by themselves deal effectively with most of both the economic and ecological aspects of

the general destructiveness of global capitalism, not to speak of the destructive dialectic between economic and ecological crisis. (O'Connor 1998, in Zimmerman et al, 413.)

Social Ecologists situate ecology and society within the grand tradition of holistic, teleological, evolutionary thought:

A social ecology interprets planetary evolution and the realization of social and ecological possibilities as a holistic process, rather than merely as a mechanism for adaptation. This evolution can only be understood adequately by examining the interaction and mutual determination between species and species; between species and ecosystem; and among species, ecosystem, and the earth as a whole and by studying particular communities and ecosystems as complex, developing wholes. Such an examination reveals that the progressive unfolding of the potentiality for freedom (as self-organization, self-determination, and self-realization) depends on the existence of symbiotic cooperation at all levels — as Kropotkin pointed out almost a century ago. We can therefore see a striking degree of continuity in nature, so that the cooperative ecological society that is the goal of a social ecology is found to be rooted in the most basic levels of being. (Clark 1998, in Zimmerman et al., 421)

Ecofeminism has been less concerned with deriving a totalizing metaphysics from ecology and evolutionary science than with using ecological concepts as a framework for an epistemology and an ethic grounded in a contextualized and corporeal knowing subject — i.e. an ecological or relational self:

[I]t is unnecessary to adopt any of the stratagems of deep ecology — the indistinguishable self, the expanded self, or the transpersonal self — in order to provide an alternative to anthropocentrism or human self-interest. This can be better done through the relational account of the self, which clearly recognizes the distinctness of nature but also our relationship and continuity with it. On this relational account, respect for the other results neither from the containment of the self nor from a transcendence of self, but is an expression of self in relationship, not egoistic self as merged with the other but self as embedded in a network of essential relationships with distinct others. (Plumwood 1991, 20)

The notion of an "ecological self" is a recurring theme in all the radical environmental philosophies. This highlights a point made earlier, that environmental philosophy investigates not only the ecological dimensions of human activity in the world, but also the ecological dimensions of human *nature*.

Conclusion

The purpose of this chapter has been to substantiate the claim that while environmental philosophy is widely regarded as a branch of moral, social and political philosophy concerned with the normative dimensions of human-nonhuman relations, the central problems of environmental philosophy are in fact deeply bound up with a variety of nonnormative philosophical and scientific issues which stem from the problem of understanding human activity and human nature in ecological terms. A critical conclusion that one can draw from this discussion is that environmental philosophy has managed to systematically misrepresent its central philosophical problems in a way that not only gives an inaccurate impression of the challenges of environmental philosophy, but does so in a way that reinforces this misrepresentation by attracting workers to the field who are not prepared to address these challenges.

Chapter 3

Must Environmental Philosophy Be The Handmaiden Of Environmentalism? Toward A General Philosophy Of Ecology

Introduction

The 1960s saw the rapid growth of information concerning a diverse array of environmental threats, including overpopulation and its relationship to poverty and famine, the depletion of nonrenewable resources, and the harmful effects to human and animal well-being caused by chemical pollutants. The result was the birth of modern environmentalism, a political movement predicated on the belief that current attitudes and practices toward the environment are at best imprudent, and at worst, gravely immoral, to other human beings and perhaps to nature itself. Environmental philosophy arose in the early 1970s as a response to the urgings of environmentalists for intellectual support and defence of the ethical and political commitments of environmentalism. Broadly speaking, environmental philosophy, as the discipline is conceived and practiced today, seeks to understand the root causes of humanity's dysfunctional relationship with the natural world, and to craft intellectual tools which will facilitate those changes which may be required to achieve a sustainable relationship between humans and nature. Environmental philosophy may involve the critique of anthropocentric ethical and political theories and the development of nonanthropocentric alternatives ("environmental ethics"), or it may involve deeper critical investigations into the historical, social, political, and religious roots of environmental degradation ("radical environmental philosophy"), but as a branch of philosophy, its *raison d'être* is to lend philosophical support to the ethical and political aims of environmentalism.

Or is it? Some environmental philosophers may dispute the claim that their discipline does not have a "theoretical soul" of its own, that it is merely a form of applied or practical philosophy with no contribution to make to the fundamental questions of, say, epistemology and metaphysics. This raises an interesting question: what would environmental philosophy be if it were dissociated from environmentalism? Put another way, what would environmental philosophy have to offer *as philosophy* to those who may be indifferent to the moral, social and political struggles of the environmental movement? In this chapter I try to sketch a possible answer to this question.

My conclusion, roughly, is that as the field of environmental philosophy is currently conceived by its practitioners, there is no environmental philosophy without environmentalism. But I believe that environmental philosophy can be reconceived in a way that does not entail this result, and that contrary to what one might expect of such a reconceptualization, may actually promote the normative aims of environmentalism *better* than the traditional conception, which *defines* environmental philosophy in terms of a commitment to environmentalism. According to the reconceptualization which I will propose, when you strip away the normative motivating content from environmental philosophy, what you have left is, to use an archaic but I think appropriate term, an *ecological approach to natural philosophy*, or perhaps, a *general philosophy of ecology*. Precisely what these terms mean, and how they relate to environmental philosophy as it is currently conceived and practiced, is the main focus of this chapter.

1. Environmental Philosophy as the Handmaiden of Environmentalism

I submit that an accurate and suggestive description of the relationship between environmental philosophy (as that field is generally conceived both within and outside the discipline) and environmentalism is the following: *environmental philosophy is the handmaiden of environmentalism*. The term "handmaiden" alludes to the view first articulated by Augustine and defended throughout the Medieval period concerning the proper relationship between philosophy and religion. I use this term with some trepidation, for I do not wish to be read as supporting the conceptions of gender hierarchy that underwrote the original use of the expression "handmaiden of theology". Nevertheless, the analogies that I believe do apply to the relationship between environmental philosophy and environmentalism turn on a comparison with the Medieval conception of the relationship between philosophy and religion, with which the expression is identified. During the Medieval period, philosophy was regarded as a useful tool for understanding the truths of revealed Christian doctrine, for developing theological positions in conformity with that truth, and for defending the Christian faith against the attacks of pagan philosophies and religions, but it was not itself a proper object of study for its own sake. Similarly, I want to suggest that the discipline of environmental philosophy, as it is currently conceived and practiced, is an applied or practical intellectual discipline in the service of the normative aims of environmentalism, but it is not a philosophical discipline that can be pursued for its own sake.

There are important disanalogies between these two cases that we must be clear about, so that they do not interfere with our understanding of the relevant analogies. First, I am not suggesting that environmentalism is a religion or that environmentalists are dogmatic in their adherence to scientific, ethical or political views in a way comparable to the Medieval church. Second, I am not saying environmental philosophers view *philosophy* as an instrumental tool which is unsuitable for study for its own sake, merely that, as the field is currently conceived, *environmental* philosophy is not suitable for study for its own sake. And third, I am not saying that the *reasons* why environmental philosophy is viewed as unsuitable for study for its sake are the same reasons why Medieval scholars believed that philosophy was unsuitable for study for its own sake.

We'll work backwords through these points. The connection between environmental philosophy and environmentalism is different from the connection between philosophy and religion in several ways, the most important for our purposes being that environmental philosophy is usually conceived as a *normative* discipline, a species of moral, social and political philosophy concerned with the normative dimensions of human-environment relationships, and hence is viewed as *conceptually connected* to the ethical, social and political aims of environmentalism. In this respect environmental philosophy is similar to feminist philosophy or the philosophy of race, disciplines that are conceptually tied to certain moral, social and political views concerning the status of women and people of colour (that women and people of colour are discriminated against on the basis of gender and race, respectively, that this discrimination is wrong, etc.). Philosophy, on the other hand, has never been viewed as a discipline *essentially* concerned with matters of religious faith and doctrine — a distinction has always been made between philosophy and theology — and this is an important historical disanalogy. Religious leaders in the Medieval period saw the autonomy of philosophy as perfectly conceivable, but regarded it as a potential threat to religious authority; but I am suggesting that environmental philosophy (once again, as it is currently conceived and practiced) is inconceivable as an autonomous philosophical discipline.

Environmentalism is not usefully regarded as a religion unless one adopts a definition of religion so broad as to render the concept benignly vacuous. Yet environmentalism does share some features common to religious belief: a conviction that the world in which we live is in some important respect radically unsatisfactory; a conception of an ideal world or state of existence which does not suffer from this radical unsatisfactoriness, and which is intrinsically desirable; and a general conception of how to get from where we are now to where we would like to be. But these features are common to almost any social change movement. The aspect of the analogy that I think is relevant to our discussion and to which I want to draw attention is the fact that the normative commitments of environmentalism function with respect to environmental philosophy in a similar fashion to the way religious commitments functioned with respect to philosophical theorizing in the Medieval period. Some of the normative commitments of contemporary environementalism are the following: that wiping out most of the species on the planet would be a tragedy; that human existence entirely cut off from the natural environment as we now know it would be seriously undesirable; and that destroying the resource base on which the survival of future generations depends is wrong. Now, Medieval philosophy aimed at showing not *whether* the claims of religious were true, but *why* they were true, and how we are to *understand* that truth. Similarly, environmental philosophy aims at determining not *whether* it is wrong to destroy the environment, but *why* it is wrong, and how we are to *understand* that wrongness. Yet the structure of the relationship is importantly different in the two cases, for philosophy is not conceptually tied to the claims of religious faith, but environmental philosophy is conceptually tied to the basic moral commitments of environmentalism. Without the perception of an environmental crisis, without the moral and political conviction that our current environmental attitudes and practices need to be changed, there is no environmental philosophy.

One can anticipate two possible reactions to these sorts of claims. Some environmental philosophers might happily admit that environmental philosophy is predicated on environmentalism. If there were no environmental crisis, if humanity lived in comfortable harmony with the natural environment, there would be no need for the intellectual activity that we call environmental philosophy. And wouldn't this be wonderful! We could all do different things with our time, pursue other areas of philosophy, spend more time with friends and family, maybe pick up a hobby.

On the other hand, I expect that many environmental philosophers would resist the dissolution of environmental philosophy simply on the grounds that it wasn't "needed" anymore. Surely, they might say, we have gained many insights into human nature, culture, and our relationship to the broader physical and biological world through the sustained efforts of environmental philosophers. Environmental ethicists have challenged traditional conceptions of moral value, and developed new ways of thinking about the origins and justification of ethical norms. Deep ecologists, ecofeminists and other radical environmental philosophers have developed original and insightful conceptions of the self as a "relational" or "ecological" entity. Political ecologists have shown how important it is for economists, sociologists and political scientists to study the ecological dimensions of economic, social and political activity. And there are numerous other examples of the valuable contributions of environmental philosophy to which one might point. Surely these sorts of investigations are worthwhile in themselves, and can be continued with or without the external motivating force of an impending environmental crisis.

Whether or not environmental philosophy has made many "valuable contributions" to the above-mentioned areas of intellectual thought is a debatable point, but I suspect that something like this latter response would have a considerable following among environmental philosophers. Environmental philosophy, they would say, has enriched our understanding of ourselves and our place in the natural world. If there were no environmental crisis, if humanity managed to achieve a sustainable relationship with the natural environment that allowed for the continued existence and flourishing of a great diversity of life forms on this planet in their natural habitats, then this would be all well and good, but human beings could still benefit from pursuing the sorts of questions that have traditionally concerned environmental philosophers.

I am sympathetic to this way of thinking, but also skeptical that the autonomy of environmental philosophy can be defended as the field is currently conceived and practiced. As we have already noted in Chapter 2, the sorts of questions that have traditionally concerned environmental philosophers fall into two broad categories:

1) Do we have moral obligations to protect or preserve the natural environment? If so, what are they, and to whom, or what, are they owed?
2) What are the causes of contemporary environmental attitudes and practices towards the environment, and what can we do to change them?

Answers to Question 1 fall under the heading of "environmental ethics". Answers to Question 2 fall under the heading of "radical environmental philosophy" or "political ecology". But what would be the motivation for developing an environmental ethic if, by hypothesis, human relationships with the natural world are suitably benign? And why would we care about the causes of our environmental attitudes and practices if there was no need to change them? More importantly, what is the connection between potential answers to questions 1 and 2 and the "insights" into human nature and culture which are said to be of such value? How, for example, are insights into the nature of the "self" or "knowledge" expected to issue from philosophical projects devoted to answering questions 1 and 2? Neither question 1 or 2 asks "what is the true nature of the self?", or "what is knowledge?".

The proper response to these objections is, I think, to refer to the subsidiary question that, I argued in Chapter 2, better characterizes the sorts of problems that environmental philosophers actually encounter in their attempts to answer questions 1 and 2:

3) How are we to understand the *ecological dimensions of human nature and human activity*, and what are the implications of such understanding for questions 1 and 2?

Environmental philosophers will admit that the issues and problems encompassed by this question are of primary importance in motivating, articulating and developing various positions within environmental philosophy, i.e. positions that claim to offer answers to questions 1 and 2 (recall the discussion in the introduction to Chapter 2). But does appealing to these issues and problems adequately address the charge that environmental philosophy has no distinctive philosophical subject matter apart from its commitment to environmentalism? All the issues are variations on a common theme — the nature of ecological concepts, theories and relationships — that seems to characterize and distinguish the philosophical challenges of environmental philosophy. And we can ask question 3 quite independently of questions 1 and 2, can we not?

Well, no, not yet. As stated above, question 3 reads:

3) How are we to understand the ecological dimensions of human nature and activity, and what are the implications of such understanding for questions 1 and 2?

The ecological issues which fall under question 3 are only important to environmental philosophy insofar as they help us address questions 1 and 2; their relevance to environmental philosophy remains parasitic on their role in advancing the moral, social and political aims of environmentalism. As evidence, consider the following: contemporary environmental philosophers *don't* think of the ecologist who is simply concerned with evaluating the scientific and philosophical status of ecological science as an *environmental philosopher*, nor the environmental scientist studying limits to population growth and resource consumption, nor the historian interested in the natural history of a living area and its human inhabitants, nor the philosopher interested in "ecological" approaches to naturalizing epistemology, nor the psychologist devoted to viewing perception, action and cognition in ecological terms. Environmental philosophers don't study these ecological issues and approaches *for their own sake*; rather, they bump into them time and again on their way to finding answers to questions 1 and 2, and are forced to deal with them as a consequence. Question 3 does delimit a set of new and interesting problems for philosophy, and it is true that many of the "insights" of environmental philosophy are in fact insights into the issues raised by question 3, but it is false to say that these issues are, as the field is currently conceived and practiced, the *subject matter* of environmental philosophy.

Thus I conclude that environmental philosophy has, so to speak, no philosophical *soul* of its own independent of the moral, social and political aims of environmentalism. Now, in saying this I do not wish to be interpreted as suggesting that ethics and social/political philosophy are generally less important or fundamental as philosophical disciplines than epistemology, metaphysics, and the philosophy of science, or that the ethical, social and political issues that surround human relationships with the environment are not important subjects worthy of philosophical study. I am merely making an observation about the conceptual relationship between environmental philosophy and the normative aims of environmentalism, and pointing out that an important set of nonnormative philosophical and scientific problems relating to ecology appear to play a foundational role in environmental philosophy, yet the investigation of these problems is not conceived as the proper subject matter of environmental philosophy.

2. A Proposal for Reconceiving Environmental Philosophy

There is a simple way of altering our characterization of environmental philosophy so that it both more accurately reflects the centrality of ecological issues to the problems of environmental philosophy, and gives these issues an independent life of their own. We can do this by editing our question 3 and putting it to the head of the line:

1*) How are we to understand the ecological dimensions of human nature and human activity?

Precisely how to characterize the intellectual project defined by this question is itself a philosophical exercise, about which much more will be said later. For now let us simply call it a "philosophy of ecology".

We can recover the traditional normative concerns of environmental philosophy by reconceiving them as an *applied branch* of 1*. Given a commitment to the reality of the environmental crisis and the normative intuitions characteristic of environmentalism, environmental philosophy as it has traditionally been conceived is essentially the *application* of 1* to our previous two questions:

2*) Do we have moral obligations to protect or preserve the natural environment? If so, what are they, and to whom, or what, are they owed?

3*) What are the root causes of contemporary attitudes and practices towards the environment, and how can we change them?

Thus, in the unlikely but hopeful event that the motivations for pursuing 2* and 3* were to disappear, all that we lose is a particular application of our core philosophical subject matter, not the subject matter itself.

There are grounds for viewing the sort of reconceptualization of environmental philosophy given here as, in fact, a more faithful and authentic presentation of the actual historical and conceptual relationships between ecological thought, environmental philosophy and environmentalism. As a branch of modern academic philosophy, environmental philosophy began in the 1970s, but "ecological" approaches to natural philosophy date back to Greek and Medieval organicist philosophies, and concerns over human impacts on nature in the modern period can be seen as early as the transition between the subsistance-based agriculture of the feudal system and the intensive surplus-oriented agriculture and resource exploitation of early mercantile capitalism. By the seventeenth century, any ideas of unbridled exploitation of nature were being tempered by the necessity of having to manage dwindling forest and other resource stocks for long-term use.

Ecological philosophies stressing holism and the unity of nature can be found in German and French romanticism, the early ecological thought of Alexander von Humboldt, Gilbert White and Ernst Haeckel, and nineteenth century utopian socialism. These ecological philosophes had an influence on the environmental management policies of the Dutch and English East India Companies. According to Grove (1990), by 1847 the directors of the East India Company were expressing concern about the dangers of artificially induced climatic change. "By 1852 the British Association was reporting on economic and physical effects of tropical deforestation, and by 1858 it was publishing papers on *global* climatic desiccation and changes in atmospheric composition." (Pepper 1996, 169). As Grove puts it, the modern day "awareness of a global environmental threat has, to date, consisted almost entirely in a reiteration of a set of ideas that had reached full maturity over a century ago" (1990, 14).

There is a long history of ecological thought that is part of a tradition of holistic natural philosophy, and that has influenced attitudes and practices towards the environment, particularly at times when awareness of ecological exploitation is accelerated and the damaging effects of environmental destruction become evident. One could say that modern-day environmentalism *inherited* a tradition (or more accurately, a set of traditions) of ecological thought that stretch back hundreds of years. Contemporary environmental philosophy was stimulated by the renewed public awareness of environmental issues in the 1960s, but it was merely the revival of a tradition of ecological thought that had been relatively dormant for many years. What makes contemporary environmental philosophy distinctive is its attempt to systematize and develop the central concepts and principles of this tradition of ecological thought within the more analytically rigorous and skeptical climate of twentieth century philosophy. It is simply a mistake to view contemporary environmental philosophy as essentially an attempt to give philosophical support to the normative aims of environmentalism. It is more accurate to think of it as a persisting tradition of natural philosophy, which historically comes to greater prominence under the stimulus of accelerated environmental deterioration.

Thus, contrary to the view that might be suggested by the previous discussion, my intention in reconceiving environmental philosophy as a "philosophy of ecology" or "ecological philosophy" is not to impose a *novel* conception of environmental philosophy on a more traditional conception, but to *re*establish the primacy of ecological theorizing in environmental thought.

3. What Is A Philosophy Of Ecology?

I have used the terms "philosophy of ecology" and "ecological philosophy". In what follows I try to clarify the various meanings which these terms might have, and address some natural concerns over the use of "ecology" in contexts that are arguably far removed from the domain of traditional ecological science. In particular I will consider whether a principled difference can or ought to be drawn between scientific, nonscientific and more philosophical applications of ecological concepts and theories.

The Ambiguous Domain of Ecological Science

It is traditional to begin a discussion of the nature and scope of ecological theorizing by quoting Ernst Haeckel, the first person to use the term "ecology" to denote a distinct field of scientific inquiry:

> By ecology we mean the body of knowledge concerning the economy of nature — the investigation of the total relations of the animal both to its inorganic and to its organic environment; including above all, its friendly and inimical relations with those animals and plants with which it comes directly or indirectly into contact — in a word, ecology is the study of all those complex interrelations referred to by Darwin as the conditions of the struggle for existence. (1866, trans. in Allee et al. 1949: frontispiece)

Haeckel's reference to Darwin is significant because it establishes a precedent for conceiving ecology as essentially a biological science that makes necessary reference to evolutionary concepts, such as "adaptation", "competition", and so forth. Yet Haeckel's definition remains deeply ambiguous with respect to the intended scope of ecological theorizing. How broad is the domain that includes "the total relations of the animal both to its inorganic and organic environment"? How indirect is "indirect contact" allowed to be? These are important questions, for different answers will affect the character of ecological science.

Haeckel's definition is often paraphrased as "the study of the relations between organisms and their environments" in introductory textbooks, but not all ecologists are happy with a definition so broad that it threatens to exclude almost nothing:

> The definition . . . 'ecology is the branch of biological science that deals with relations of organisms and environments' would provide the title for an encyclopedia but does not delimit a scientific discipline. (Richards 1939, 388)

Ecologists wishing to place clearer constraints on the domain of ecological theorizing have focused on that subset of the total relations between organisms and their environments that influence "the conditions of the struggle for existence". On this view, ecology is the scientific study of those factors that "determine the distribution and abundance of organisms" (Andrewartha 1961; Krebs 1978; Begon, Townsend and Harper 1990). This tradition of "population-community" ecology (O'Neill et al 1986) situates ecology firmly within the life sciences, emphasizing its overlap with the sciences of behaviour, genetics, physiology and evolution.

There is another tradition in ecology that focuses on material cycling and energy flow in ecological systems, and that has been a part of scientific ecology since Charles Elton's (1927) work on food chains. Ecological science in this "process-functional" tradition (O'Neill et al. 1986) has emphasized the dependence of organic life on complex biogeochemical and energetic processes occurring at varying spatial and temporal scales. Practitioners of this more physically and physiologically oriented ecology are less concerned with imposing *a priori* restrictions on the sphere of phenomena which may be relevant to understanding the structure and function of ecological systems. In *Fundamentals of Ecology*, an influential text by Eugene Odum that structured the field of ecology around the concept of the "ecosystem", Odum is content to define ecology as "the study of the structure and function of nature" (Odum 1971).

A recent definition of ecology accepted by the Institute of Ecosystem Studies in Millbrook, New York, attempts to accommodate both the population-community and process-functional schools of ecology:

> Ecology is the scientific study of the processes influencing the distribution and abundance of organisms, the interactions among organisms, and the interactions between organisms and the transformation and flux of energy and matter. (Likens, 1992)

This definition is admirably ecumenical, but the domain of ecology appears just as unconstrained here as it does in Haeckel's formulation. What sorts of phenomena would *not* fall under this description? What is to prevent, for example, the study of *solar nuclear processes* from being a legitimate subfield of ecology? The sun is, after all, the source of nearly all the energy which drives ecological processes on earth, and variation in solar luminosity can have an impact on global climate patterns. Or better, why not take as one's organism of study the *human* organism? Population and behavioural ecology would then overlap considerably with human demography and sociology. The study of the "interaction between organisms [humans] and the transformation and flux of energy and matter" might encompass fields as diverse as economics (material and energetic transactions among humans and between humans and the environment) and perceptual psychology (energetic transactions between perceptual systems and the environment).

It is tempting to dismiss this issue as merely academic, a quibble over wording. But consider:

> In this section we present a model of an open system. The model represents any economic or ecological system starting from the individual agent (organism) and ending with the global economy (ecosystem). (Amir 1994, 128)

The quote is from an article in *Ecological Economics*, the flagship journal for a new hybrid discipline whose subject is predicated on the view that ecological theories and principles are just as applicable to complex economic systems as they are to complex ecological systems.

There is also a tradition of research in psychology called "ecological psychology" that conceives the problems of animal and human psychology (perception, cognition, and action) as problems for the science of ecology. A book series devoted to psychological research in the ecological tradition is introduced as follows:

> This series of volumes is dedicated to furthering the development of psychology as a branch of ecological science. In its broadest

> sense, ecology is a multidisciplinary approach to the study of living systems, their environments, and the reciprocity that has evolved between the two . . .
>
> The late James J. Gibson used the term ecological psychology to emphasize this animal-environment mutuality for the study of problems of perception. He believed that analyzing the environment to be perceived was just as much a part of the psychologist's task as analyzing animals themselves. (editors' preface in Lombardo 1987)

Economics and psychology are just two examples of fields not traditionally associated with scientific or "traditional" ecology, which nevertheless have active research programs which interpret their fields as legitimate domains for scientific ecological theorizing, if not as actual subfields of ecology. Other examples include the "ecosystem approach in anthropology" (Moran 1990), and the various traditions of "human ecology" (Freese 1992; Steiner and Nauser 1993).

Should we view fields like ecological psychology, ecological economics, ecological anthropology and human ecology as branches of traditional, scientific ecology? On a list of subdisciplines within ecology in our introductory ecology textbooks, should we place these fields alongside landscape ecology, biogeochemistry, population ecology, community ecology, and behavioural ecology? Whether ecological science would have anything to gain by doing so or not, it would clearly be a departure from professional and scientific orthodoxy to admit all these fields as legitimate branches of ecology on an equal footing with the more familiar branches of ecology. Yet it is not clear that there are any *principled* grounds for denying "ecological legitimacy" to these fields. They are all striving to study their respective phenomena in a scientific manner (they're not doing ecological *poetry*), and they conceive these phenomena as *literally*, not metaphorically, ecological. The main impediment to acknowledging these diverse forms of ecological science as engaged in a *common* scientific project appears to be historical, institutional and professional tradition, not anything inherent in the subject matter.

Ecology: The Study of Ecological Phenomena

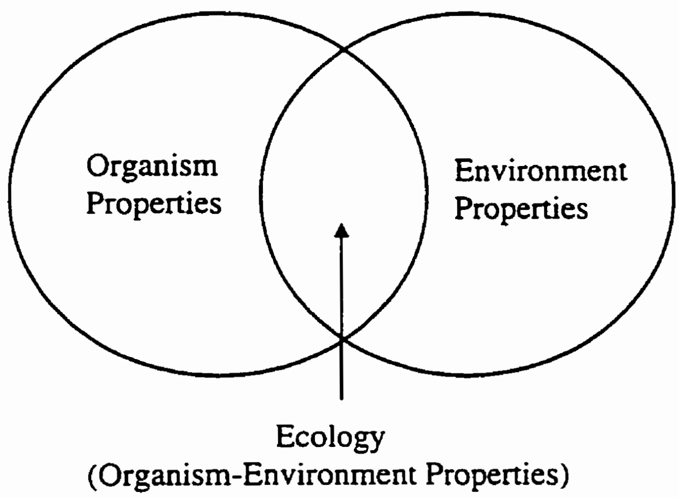
The above comment suggests a rather simple definition of ecology. Ecology, at its most general level, is the study of *ecological phenomena*. This is a more useful definition than it appears, and I believe it has several virtues that recommend it.

First, though initially vacuous, the definition acquires content when we specify what it is for a phenomenon to be "ecological". This is more an exercise in conceptual analysis than anything else; it invites an investigation into the necessary and sufficient conditions for the application of the concept. But it is a useful exercise because it forces one to consider the *object* of ecological theorizing rather than the specific techniques, theories or methologies that characterize particular forms of ecological science. We want to know what it is about a given subject matter that suggests to the investigator that an ecological approach would be useful or appropriate.

Second, we can distinguish ecological *science* from other forms of ecological inquiry simply by defining ecological science as "the *scientific* study of ecological phenomena". It is important to keep the issue of the scientific status of different forms of ecological inquiry separate from the question of what it is about some phenomenon that motivates an ecological inquiry in the first place. One might want to talk about ecological approaches to family therapy, for example, and question whether such a field is properly regarded as a science (as one might question the scientific status of any form of psychotherapy), but such questioning should not by itself imply that there are no genuinely ecological dimensions to family dynamics. One may readily admit that family dynamics are chock full of ecological phenomena, but be very skeptical of claims that we have anything approaching a *science* of such phenomena.

Third, we can now give a correspondingly straightforward definition of a "philosophy of ecology". If ecology is the study of ecological phenomena, then a philosophy of ecology is "the *philosophical* study of ecological phenomena". This definition fails to capture the second-order character of much philosophical theorizing, however, so one will also want to talk about the philosophical study of the *study* of ecological phenomena, i.e. the philosophical study of ecology. We will say more about the different ways that one might conceive a philosophy of ecology later in the chapter.

Now we must return to our initial definition and consider what it is that we mean by an "ecological phenomenon". Earlier we saw that Haeckel's definition of ecology is often paraphrased as "the study of the relations between organisms and their environments". What is the nature of the object of study picked out by this definition? Is it the study of organisms? No, because that fails to mention relationships to environments. Is it the study of environments? That can't be right; environments are always environments *of* something, they make necessary reference to a thing which is environed. The correct reading is to take the definition at face value; the object of study in ecology is the *relationships* between organisms and their environments. Ecological phenomena thus reside at the intersection of the biological and the physical:



It is useful to distinguish three distinct but related types of ecological properties or phenomena:

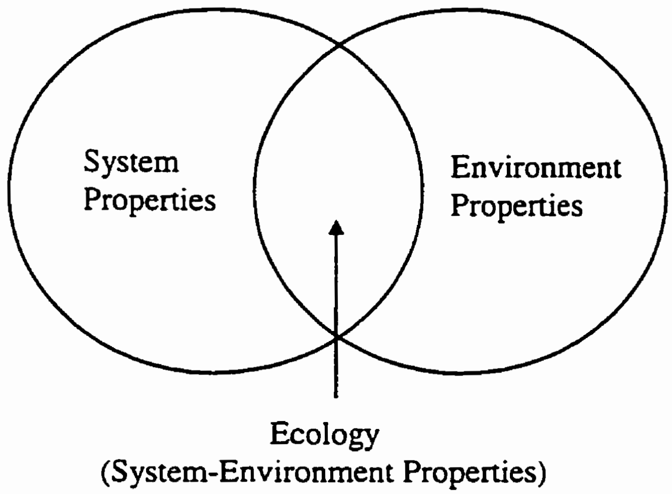
i) properties of *biological entities* that depend on or make essential reference to environmental relations;

ii) properties of *environments* that depend on or make essential reference to relations to organisms; and

iii) properties of the *relations* that obtain between organisms and their environments.

Different research traditions in ecology can be distinguished in part by which of these three categories of ecological phenomena are their main focus of study. Population-community ecology is organism-centered, and focuses on phenomena of type i). Biogeochemistry and other forms of empirically-oriented ecosystem theory focus on phenomena of type ii). Various forms of theoretical ecosystem ecology and systems ecology focus on phenomena of type iii).

It is worth expanding on this last type of ecological phenomena, for it is in theoretical ecosystem and systems ecology (I'll use "systems ecology" to refer to both from now on) that one is most likely to find theoretical and conceptual tools that would lend themselves to application outside the traditional domain of ecological science. Systems ecology conceives ecological systems as networks of causally interacting dynamical systems whose properties can be profitably studied using theoretical tools derived from thermodynamics and engineering mathematics (systems theory, control theory, information theory, etc.). The nodes or compartments of an ecological network correspond to functionally defined ecological types (predators, filter feeders, deposited detritus, microbiota, etc.), but the ecological phenomena of interest to systems ecologists are those that exist only at the systems- or network-level; they depend in no way on the details of the biological or physical natures of the entities represented by the compartments. The only physical property of the component nodes that matters is their status as receivers and donors of matter and energy. All the network-level properties of the system are a function only of the relations that hold between the various nodes of the network and the physical constraints operating on each of the component processes (basically, matter must be conserved while some energy is always dissipated at each transfer in the form of heat). It would take the discussion too far afield to discuss in detail the network-level properties that emerge from systems of this type. Suffice it to say that they exhibit holistic properties which, if applicable to real-world ecosystems, suggest an irreducibly holistic character to the dynamics of complex ecological systems[10]. For present purposes what I want to highlight about systems ecology is its focus on relational properties that make no reference to the material or biological natures of the entities making up the network (apart from their conformity to basic physical laws). Thus, one can talk about ecological phenomena in the abstract, as relations that hold between *systems* and their environments.



This is a more general characterization of ecological phenomena than is given in our previous definition, which had assumed that environments were defined relative to organisms. Organism-environment systems are a specific and important type of ecological system, but the basic nature of an ecological phenomenon or property is that it depend in an essential way on the interaction between systems and their environments. There is no reason to impose *a priori* constraints on the types of systems and environments that may exhibit these relational dependencies. So we may be talking about populations of organisms

[10] See Ulanowicz (1986) and Higashi and Burns (1992) for examples of systems-level properties of ecological networks. These are discussed in Chapter 5.

in interaction with their biotic and abiotic environments, or we may be talking about the organelles of a single cell in interaction with the biotic and abiotic environment within the cell, or even a network of connected neurons in the human brain. All these systems can be regarded as "eco-systems" which are the site of ecological phenomena, and can be legitimate objects of study for an ecological science.

One can generalize this conception of ecological phenomena even further to include systems which may be of a purely conceptual or formal nature, such as a network of related ideas or concepts, or purely mathematical relations among mathematical entities. Once we leave the realm of systems constrained by physical laws we are no longer talking about ecological *science*, but it may still be appropriate to talk about ecological phenomena in such contexts. In fact, the term "ecology" is often used in precisely this sense in nonscientific literature. A quick search of our university library catalogue revealed a plethora of books that have "ecology" in the title, but which make little or no reference to physical or biological science, such as: *The Ecology of Mind*, *The Ecology of Mental Disorder*, *The Ecology of Preschool Behaviour*, *The Ecology of Public Administration*, *The Ecology of Religion*, *The Ecology of the Airwaves*, and *The Ecology of the School*. "Ecology" is used in these contexts to refer to a network of relationships or associations, and it is intended to connote a sense of multiple-connectedness, mutually defining relationships and organizational complexity. On the analysis being offered here, these may be legitimate and literal uses of the term "ecology". It is quite likely, for example, that many mental disorders cannot be understood without reference to a multitude of environing biological, psychological and social factors. Simply saying this doesn't imply that we know a great deal about mental disorder, of course, but it does imply that any attempt to reduce mental disorder to one or another of its component parts or causes is likely to miss something important.

In summary, ecological phenomena are phenomena that make essential reference to relationships between systems and their environments. Traditional ecological science assumes that the system-environment complex involves both biotic and abiotic components, but I have tried to argue that ecological phenomena are essentially relational and not dependent on the ontological character of the system or environment in question.

Types of Philosophy of Ecology

We can now consider in greater detail what a philosophy of ecology would look like. A philosophy of ecology in its broadest sense would engage in the philosophical study of ecological phenomena, and in the philosophical study of that study. This conception is consistent with the way the philosophy of physics or biology is understood and practiced. A philosopher of physics may be interested in the philosophically interesting aspects of certain physical phenomena (e.g. quantum nonlocality), and in the philosophical aspects of the nature and interpretation of physical theories (e.g. realist and instrumentalist interpretations of quantum mechanics). Similarly, a philosopher of ecology will be interested in the philosophical aspects of ecological phenomena and in the philosophical study of ecological concepts and theories which are used to understand those phenomena. For example, ecological psychology views *perception* as an ecological phenomenon; perception is not conceived as something that goes on in the brain, but rather as an "achievement" of the organism-environment system in dynamic interaction (Gibson 1979). A philosopher of ecology will want to understand (among other things) precisely what this claim means, what the evidence is for its truth, and what the implications of it are for our understanding of the psychology and epistemology of perception, and for the philosophy of mind and action. Ecological psychologists also claim that perceptual contact with the environment involves perceiving the meaning and value of objects within the environment. Such a claim suggests broader connections to epistemology, semantics and theories of value. A philosopher of ecology will try to understand, evaluate and explore the philosophical implications of such views.

Given the broad scope and diversity of phenomena that may be called ecological, it seems worthwhile to distinguish different types of philosophy of ecology corresponding to different restrictions on the scope of ecological theorizing. I offer here three types of philosophy of ecology, in order of increasing generality:

i) *Philosophy of ecology as the philosophy of the special science of ecology*
This is philosophy of ecology conceived as a specialization within the philosophy of science. Ecology is understood along traditional lines as that science that studies patterns in the distribution, composition and abundance of species populations, or, more broadly, the study of organism-environment relationships in natural ecological communities. A philosopher of ecology studies the empirical and theoretical foundations of ecological theories, the status of ecological laws, the nature of explanation and confirmation in ecological science, conceptual change in the history of ecology, and so on.

There is already a growing literature in the philosophy of ecology in the sense described here, much of it written by ecologists rather than professional philosophers[11].

ii) *Philosophy of ecology as the philosophy of system-environment relationships.*
Here the system-environment complex is assumed to have biotic and abiotic components. It may be a single cell, an individual, a community, an ecosystem, or even the global biosphere, and where the relationships in question may include include more abstract relations of function, representation, and evaluation. On this conception, a philosophy of ecology is a program to investigate the evolutionary and ecological dimensions of the phenomena of perception, action, cognition and evaluation in natural biophysical systems.

---

[11] See for example Saarinen 1982, Peters 1991, and Shrader-Frechette and McCoy 1993.

In this category I would include the philosophical study of various ecological and dynamical systems approaches to cognition (Gibson 1986; Kugler and Turvey 1987; Port and van Gelder 1995; Reed 1996; Clark 1997), developmental systems approaches to biological development (Oyama 1985), evolutionary epistemologies (Bradie 1997), sociobiology and evolutionary ethics (Sesardic 1995), as well as more recent work on teleosemantics in the philosophy of biology and the philosophy of mind (Millikan 1984, Dretske 1988, Sterelny 1990).

iii) *Philosophy of ecology as perspective on any given philosophical or scientific issue*
Ecology is viewed here as a distinctive perspective, a way of looking at the world in "ecological terms". Consider analogies with feminist theory, where a "feminist approach to X" is conceived as any approach which affirms the significance of the concept of "gender" for a complete understanding of X. One can speak of feminist ethics, feminist epistemology, feminist political theory, feminist philosophy of science, feminist film and literary criticism, feminist legal theory, feminist theology, and so on. Similarly, one can speak of ecological approaches to history, economics, political theory, and so on. Ecological "theory", like feminist "theory", becomes a set of theoretical tools and perspectives for revealing certain aspects of the world which would otherwise go unnoticed.

The key concepts for an ecological perspective are "system", "environment", "organization", "network", "hierarchy", "interdependence", "reciprocity", "connectedness", "complexity", and "context". Environmental philosophers are more familiar than most with ecological approaches in philosophy, theology, the humanities and the social sciences. (See Pepper 1996 and the readings in Merchant 1994, and Zimmerman et al. 1998 for sources.)

There are at least two good reasons for decomposing the philosophy of ecology into different forms or subfields. First, philosophy of ecology is simply too big a subject to be regarded as a single monolithic discipline. Progress in the philosophy of ecology will require specialization as well as synthesis, focused attention on small, circumscribed problems as well as big-picture work and more speculative theorizing. But no one can be a specialist in every aspect of their chosen field, so philosophers of ecology, like researchers in any other field, will have to choose problems to work on that match their interests and aptitudes. It helps that the philosophy of ecology can be viewed as having parts, an anatomy which allows those unfamiliar with the field to situate a given project within the larger picture.

Second, decomposing the philosophy of ecology in the way suggested here makes it easier to see connections between the problems of environmental philosophy and the philosophy of ecology. As I argued in Chapter 2, anthropocentric environmental ethics appeals to ecology as a source of information concerning the nature and severity of the environmental crisis. This information functions as a constraint on normative theorizing, but our understanding of the potential and limitations of ecology to inform us of the existence and magnitude of environmental threats is far from perfect. A philosophy of ecology conceived on the model of the philosophy of biology or physics [type (i)] could be very helpful in evaluating the potential and limitations of ecological science to address pressing environmental problems, such as human overpopulation, global climate change, and species extinction. Similarly, a philosophy of ecology of type (ii) could contribute to the development of naturalistic theories of value and evaluation, which may be used to further the philosophical goals of a nonanthropocentric environmental ethic. And a philosophy of ecology of type (iii), which focuses on ecological approaches in economics, history, anthropology, geography, and so on, is fundamental to the critical project of radical environmental philosophy.

Conclusion

In this chapter I have argued that, as it is conceived and practiced by environmental practitioners today, environmental philosophy has no essential connection to the issues and problems that interest workers in other areas of philosophy (or science, or the other humanities) independent of the moral, social and political aims of environmentalism. I expressed this relationship by drawing an analogy between philosophy and religion in the Medieval period and contemporary environmental philosophy and environmentalism; environmental philosophy, I suggested, is the *handmaiden* of environmentalism. However, I argued that the autonomy of environmental philosophy could be salvaged by reconceiving the fundamental philosophical challenge of environmental philosophy as the challenge of understanding the ecological dimensions of human nature and human activity in the world. This was not intended as a novel reconceptualization, but rather as a restoration of the true historical relationship between ecological philosophies and environmental concerns. I analysed the notion of a philosophy of ecology in some detail, and sketched some of the ways that a philosophy of ecology could contribute to the traditional philosophical problems of environmental philosophy.

Reconceiving environmental philosophy in the manner suggested here would benefit environmental philosophy on a professional level as well:

1) It would present environmental philosophy as a philosophical program with broad relevance to problems in many areas of philosophy besides moral, social and political philosophy, such as the philosophy of mind, the philosophy of science, the philosophy of biology, and epistemology. This in turn would attract the attention of workers who, because of their backgrounds in these other areas of philosophy, could help make significant progress on the difficult philosophical problems which lie at the core of environmental philosophy.

2) It would establish environmental philosophy as a field with distinct subdisciplines, not all of which need be directly concerned with the traditional problem of justifying ethical and policy perspectives on the environment. The environmental philosopher would be able to study ecological approaches to perception, for example, without regard to precisely how such investigations are expected to contribute to the resolution of environmental problems. This would facilitate progress and philosophical sophistication in the specific areas of philosophy that are most needed in environmental philosophy.

3) It would bring together and unify a number of research areas in science and philosophy that already make heavy use of ecological concepts and theories, such as "ecological psychology", "ecological economics", and "ecological epistemology". Such fields become a rich source of theoretical, empirical and conceptual resources for the new environmental philosophy.

A possible objection to the proposed reconceptualization of environmental philosophy as a general philosophy of ecology might run as follows. In this chapter I argue that a general philosophy of ecology would treat the traditional normative problems of environmental philosophy as applied problems for a broader science and philosophy of ecology. This appears suggest to that I regard environmental philosophy as a *proper subset*, so to speak, of a general philosophy of ecology. If so, then how is this consistent with saying that I advocate a reconceptualization of environmental philosophy with the *whole* of your new philosophy of ecology?

I would argue that my position is consistent as long as we are distinguishing between environmental philosophy as it is currently conceived and practiced, and environmental philosophy as I would *like to see it* conceived and practiced. But let me clarify a point. As contemporary environmental philosophers view the discipline, the primary concern of environmental philosophy is with ethical, social and political issues concerning humanity's relationship with the natural environment. My claim is not that environmental philosophy ought to address a *new* set of nonnormative scientific and philosophical issues concerning the ecological dimensions of human nature and human-environment relations that it was not addressing before; rather, my claim is that over its *long* history, environmental philosophy has *always* addressed these issues, but that in conceiving itself as the "handmaiden" of modern-day environmentalism, contemporary environmental philosophy has *forgotten* that these nonnormative issues are central to the tradition of ecological philosophy that it has inherited. My aim is not to dismiss contemporary environmental philosophy, but to reanimate it with the philosophical tools that are required to carry out the projects that it pursues. Given the present situation, the easiest way to make this point is to segregate the normative and nonnormative components of contemporary ecological theorizing, but the purpose of this segregation is to allow these nonnormative components the freedom to bloom, in the hope that they will consequently be more effective in servicing the normative goals of environmental philosophy.

Chapter 4

The Problem of Unification in Ecology, and Elements of a Solution

With this chapter we begin Part Two of the dissertation. In the next four chapters I attempt to articulate a conceptual and theoretical framework that may function as launching pad, so to speak, for further investigations in ecological science and the philosophy of ecology. The central theme of these chapters is the importance of a unified approach to ecological science.

Introduction

In the present chapter I consider a variety of motivations for pursuing a unification program in ecological science, and attempt a diagnosis of the unification problem. I discuss what I believe are two essential elements of a solution to the unification problem, and conclude with some general comments concerning the prospects for a unified ecological science that makes useful and generalizable predictions.

1. Reasons for Wanting a Unified Ecological Science

1) *Progress in Theory Development*

Ecology is home to a large number of subdisciplines that lie along a continuum between purely biological and purely physical studies. The subdiscipline of *biogeochemistry*, for example, is concerned with patterns in the flow of elements and nutrients in ecosystems, while *evolutionary ecology* uses the tools of evolutionary theory and population genetics to understand the structure and development of species communities. *Landscape ecology* studies the ecology of large heterogeneous land mosaics, such as whole landscapes and regions, while *behavioural ecology* focuses on the ecological dimensions of plant and animal behaviour.

Specialization into subdisciplines is a mark of all mature sciences, but disciplinary specialization in fields such as physics, for example, is supported by a shared network of concepts and agreement on their interpretation in physical and mathematical terms[1]. Subdisciplines in ecology, by contrast, tend to develop narrowly focused theories, methodologies, definitions and lexicons, which results in divergences in the understanding of even basic theoretical terms, like "regulation", "development", "community", "ecosystem" and "evolution" (Pickett et al. 1994). The result is a fragmented science characterized by, at best, a respectful pluralism of concepts, theories and methodologies, and at worst, a continuing legacy of seemingly irresolvable disputes over foundational issues (see McIntosh 1987).

Theoretical and methodological pluralism may not be a bad thing in itself, but in the case of ecology the problem is aggravated by the organizational, cross-scale complexity of its putative subject matter, viz. real-world ecological systems. While it may be simpler to study the dynamics of species populations independently of considerations of biogeochemical cycling (and vice versa), in reality these are not independent processes, and an ecological science that fails to capture the relevant dependencies is to that extent theoretically impoverished. Ecological systems are hierarchically structured networks of interacting biotic and abiotic entities and processes. The various ecological subdisciplines carve ecological systems into (usually highly temporally and spatially restricted) component processes and study these processes in relative isolation from one another. There is little effort to synthesize the information acquired in these process studies, and hence little possibility of understanding ecological processes that cut across spatio-temporal scales of organization and subdisciplinary boundaries.

---

[1] This is not to suggest that all subdisciplines within physics share a common methodology. I would defend the claim, however, that the interpretation of basic physical concepts such as energy, mass, and force, is sufficiently constrained by their use within a variety of mathematically formalized (and empirically successful) physical theories, that they may legitimately be regarded as shared, foundational concepts for physics.

2) *Ability of Ecology to Successfully Address Environmental Problems*

For precisely the reasons described above, ecology has not been overly successful at helping to understand and deal with human impacts on ecological systems. Environmental problems are "whole-system" problems that cannot be adequately addressed at the level of component process analyses. Air, water and soil pollution, terrestrial and marine habitat destruction and biodiversity loss, ozone depletion, CO_2 and other greenhouse gas emissions, exponential population growth and resource consumption — all of these anthropogenic influences impose stresses on global and local ecological systems as a whole, with effects that the current state of ecological theory is unable to predict with the degree of certainty desired by environmental policy makers. Dealing effectively with human-accelerated environmental change requires not only integrating many topic areas, scales and levels within ecology, but also (particularly at regional scales) collaboration and integration with the physical and social sciences (Pickett et al. 1994). A necessary precondition for successful environmental risk assessment and management (and, where needed, restoration) is an ecological science with sufficient internal coherence to function as a framework for integrating ecologically relevant information from diverse sources and constructing models of ecological processes which are faithful to real-world ecological phenomena.

3) *Interdisciplinary Connections*

Ecologists are often surprised to learn that other branches of natural and social science have research traditions that make heavy use of ecological concepts, methods and theories. Recalling our earlier discussion in Chapter 3, anthropology, for example, has a school of theoretical practice that calls itself the "ecosystem approach to anthropology" (Moran 1990). Economics and ecology have a long tradition of mutual influence, but recent developments in "evolutionary economics" and "ecological economics" have brought the disciplines into even closer association (Costanza and Wainger 1991; Faber and Proops 1996). And as we shall see in greater detail in Chapter 7, there is an active theoretical and experimental tradition in "ecological psychology" that is descended from the work of perceptual theorist J. J. Gibson (Gibson 1950, 1966, 1979). These diverse fields of research share the view that ecology has a relevance and applicability beyond the traditional domain of forests, fields and streams, that the various phenomena that they investigate — anthropological, economic, psychological — can profitably be viewed as *ecological* phenomena. The unique perspective on ecological phenomena offered by these nontraditional forms of ecological science may be a powerful asset in the development of ecological theory.

4) *Potential Contributions to Philosophy*

Philosophy has a tradition of theorizing in metaphysics, epistemology, the philosophy of mind, and value theory, that can be, and has been, characterized as "ecological". Broad appeals to ecology in support of process ontologies, relational metaphysics and nonanthropocentric value theories can be found, not surprisingly, in the environmental philosophy literature (see for example Golley 1987, Wittbecker 1990, Johnson 1991, Rolston 1993, Westra 1994, and Buege 1997). But ecological approaches to philosophical problems can also be found in many different areas. For example:

- The formal semantics and epistemology of Barwise and Perry (1983) is strongly influenced by a Gibsonian ecological conception of the organism-environment relationship.
- Naturalized epistemologies in the Quinean tradition have drawn on psychology and cognitive science as a framework for a philosophical understanding of perception, belief and knowledge, but Lorraine Code has recently argued from a feminist perspective that ecology offers a more suitable empirical and conceptual framework for understanding the subject-

object/knower-known relationship, the contextual and social nature of knowledge production, and the role of the body in perception and action (Code 1996).

- The recent "dynamical turn" in cognitive science, influenced by work in situated robotics, animate vision, artificial life, connectionism and dynamical systems theory, emphasizes the regulatory and coordinative function of continual, real-time perceptual contact between agent and environment (Port and van Gelder 1995; Boden 1996; Clark 1997).
- Ecology is a science of complex systems, and developments in complexity and self-organization theory have spawned a growing literature on the relevance of these fields for the philosophy of physics, biology, and ecology (Brooks et al. 1989; Weber et al. 1990; Weber and Depew 1996).

Much of the philosophical literature on the relevance of ecology for philosophy reflects the division within ecology between demographic- and evolutionary-oriented traditions on the one hand (e.g. most of the work on evolutionary epistemology, evolutionary ethics, etc.), and physiological- and systems-oriented traditions on the other (e.g. Barwise and Perry's "situation semantics", dynamical systems approaches to cognition, etc.). To a great extent, progress in the application of ecological concepts and theories within philosophy is dependent on the reconciliation of these two traditions.

2. A Diagnosis of The Problem: Ecology as Demography versus Ecology as Physiology

Ecological systems are composed of individual organisms, grouped into single-species populations and multi-species communities, in dynamic interaction with their respective biotic and abiotic environments (ecosystems). This sequence is often depicted as a nested ecological hierarchy, with communities nested within ecosystems, populations within communities, and organisms within populations. The units of this hierarchy map roughly onto subdisciplinary boundaries in ecology; behavioural ecologists study individuals or small groups of organisms, population and community ecologists study whole populations and communities, and ecosystem ecologists study whole ecosystems, with an emphasis on physical processes governing relations between biotic and abiotic components of the ecosystem.

The most serious challenge facing the unity of ecological science is not the professional division of labour among levels of organization, but the difficulty of relating phenomena at one level of organization to phenomena at higher and lower levels of organization. This problem is compounded by competing conceptions of the aims and methodology of ecological science which prioritize one type of analysis over another at a given level. The types of analysis in question can be broadly described as "demographic" and "physiological" modes of investigation (Hagen 1989)[2].

The *demographic* perspective on ecological systems focuses on patterns and causes of change in the distribution and abundance of organisms in space and time. Abiotic factors are usually considered to be external forcing functions altering the dynamics of organisms and aggregations of organisms (Picket et al. 1994, 7). The demographic approach is closely associated with an evolutionary perspective via the belief that "organisms are as they are, and live where they do, because of their evolutionary history" (Begon et al. 1990, 3). The phenomena of

---

[2] Within the literature, the demographic approach is most often associated with "population-community ecology", and the physiological approach with "ecosystem ecology" or "process-functional ecology". But the "ecosystem" concept is used by both population-community and process-functional theorists, and communities are often studied from a network perspective (e.g. food web theory) that is closely associated with the physiological approach. Hagen's distinction between "demographic" and "physiological" perspectives cuts across the more common, and more ambiguous, "population/ecosystem" distinction, and is more informative concerning the methodological differences that divide ecologists.

central interest for demographic ecological science can be represented by the following equation:

$$N_{future} = N_{now} + B - D + I - E.$$

The numbers of a particular organism that will at some time occupy a particular site of interest (N_{future}) is equal to the numbers presently there (N_{now}), plus the number of births between now and then (B), minus the number of deaths (D), plus the number of immigrants (I), minus the number of emigrants (E) (Begon et al. 1990, 122). Begon et al. offer a conception of ecology that gives primacy to the demographic perspective:

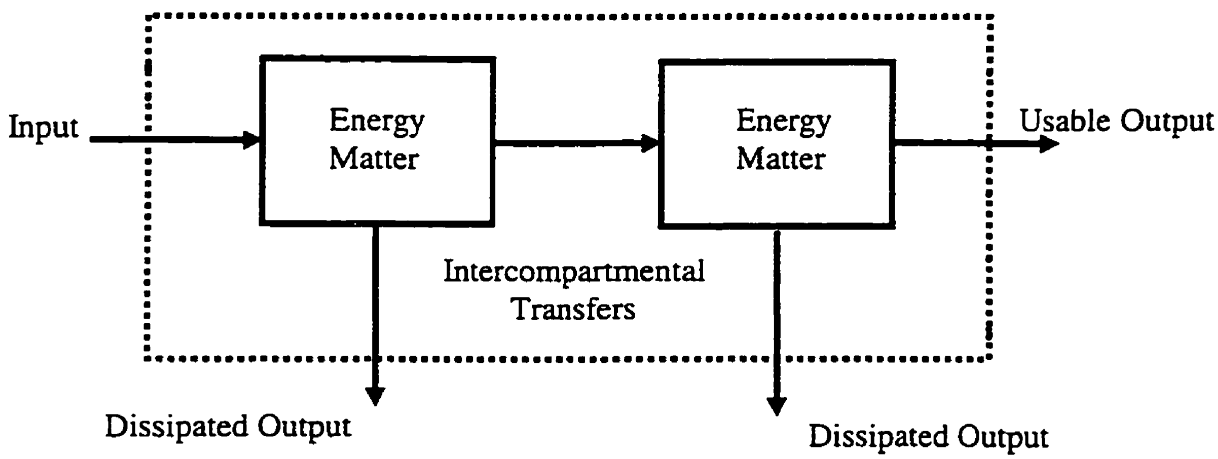
> These facts of life [represented in the equation above] define the main aim of ecology: to describe, explain and understand the distribution and abundance of organisms. Ecologists studying the effects of temperature, or light, or a pollutant such as mercury, on a particular organism will probably concentrate on just one phase or aspect of the organism's life; but the study has ecological relevance only insofar as the particular phase or aspect affects the birth, death or migration of the organism. Ultimately the aim is to improve our understanding of N_{now} or to predict N_{future}. . . . In all cases . . . ecologists are interested in the number of individuals, the distributions of individuals, the *demographic processes* (birth, death and migration) which influence these, and the way these demographic processes are themselves influenced by environmental factors. (1990, 122; italics in the original)

By contrast, the *physiological* tradition in ecology focuses on patterns of material and energy flow in ecological systems, and the processes controlling them. The abiotic environment is explicitly included in the system, and the complex embedded dynamics and heterogeneity of organisms are often "black-boxed" and taken as constants (Pickett et al. 1994, 7). Physiological metaphors dominate this theoretical perspective; ecological systems are described as "developing" over time, exhibiting organism-like properties such as "metabolism" and "homeostasis" (Hagen 1989). These phenomena are described in terms of fluxes of energy and matter, according to the following basic equation:

$$E_{future} = E_{now} + I - O_{usable} - O_{dissipated}.$$

(Here we have chosen to represent energy flows only.) The basic unit of analysis is a *physical system* with boundaries reflecting the investigator's choice of level of organization and spatio-temporal scale. *I* represents external energetic inputs from the environment across the system boundary. *O* represents exports from the system to the environment, of which a proportion is always radiated as dissipated heat, in conformity with the second law of thermodynamics ("respiration", another physiological term). For multi-component systems one distinguishes inputs from the environment external to the whole system and inputs from one compartment to another, and outputs to the environment from the whole system and outputs from one component to another (see figure below).

Systems and network analysis is an important tool for researchers in the physiological tradition. A simple two-component systems model can be generically represented as follows:



The compartments in the model may represent any number of biotic and abiotic ecosystem components (plants, detritus, bacteria, carnivores, etc.). In all cases what is described in the model is the transformation and flux of matter and energy in ecological systems.

Within the physiological tradition in ecology, to say that an ecological system "develops" over time, or exhibits "metabolism" or "homeostasis", is to say that the system demonstrates patterns of energy and material flow that are analogous to patterns found in organisms. For example, organisms maintain their complex internal organization and structure by exploiting high quality (low entropy) material and energetic resources in their environments (food, sunlight, etc.). This high quality energy is used to do work within the system (maintenance, repair, locomotion, etc.). The energy is degraded in the process, and is ultimately exported back into the environment as waste or heat. Many ecologists in the physiological tradition argue that ecological systems exhibit similar patterns of material cycling and energy dissipation, and that ecosystem processes can be profitably explored from this perspective. One author goes as far as to *define* ecology as the "biology of ecosystems", by which he means the study of the flux of energy and matter, the physiology, of ecosystems (Margalef 1969, 4).

The problem of the unity of traditional ecological science is the problem of relating and reconciling the demographic and physiological perspectives on ecological systems. That demographic and physiological ecological phenomena are deeply interdependent is an ecological fact:

> Individual organisms, species populations, and communities inhabit ecosystems, and by definition must be affected by ecosystem processes — nutrient fluxes, productivity, and the physical environment. Conversely, ecosystem processes must be affected by organisms; there can be no primary production without plants, and no nitrogen cycle without microbes. (Jones and Lawton 1995, 3)

Yet within traditional ecology there is little general theory relating ecosystem processes to the activities, dynamics, and assemblages of species. As we have seen, population and community ecologists use changes in population numbers, dN/dt, as the fundamental currency in their models, while ecosystem theorists track rates of energy exchange, dE/dt. Interestingly, a focus on energy usage is characteristic of the models of ecologists working at the level of *individual* organisms as well. For such ecologists,

> even though the proximal currency may be sometimes by nitrogen, protein, or even predation risk, rather than energy per se,

> thermodynamic considerations are paramount. Individual organisms are viewed as maximizing their fitness by acquiring scarce resources from the environment, using them to maintain homeostasis of the individual, and allocating them to offspring. (Brown 1995, 182)

In summary, the traditional ecological hierarchy correlates with fundamental modeling currencies as follows :

Ecosystem	dE/dt
Community	dN/dt
Population	dN/dt
Organism	dE/dt

At the level of individual organisms and at the level of whole ecosystems, the theoretical focus is on rates and patterns of energy flow. At the level of assemblages of organisms in populations and communities, the theoretical concern is mainly with rates of population change, and factors that affect patterns of distribution and abundance in species populations. Thus, the main challenge facing proponents of a unified, multi-scalar ecological science is to find principled and illuminating ways of relating changes in energy flow to changes in population number, i.e. relations between dE/dt and dN/dt.

3. Elements of a Solution

In this section I discuss what I consider to be two essential components of any prospective unification program in ecological science. The first component is the adoption of a theoretical framework that allows for contributions from the *complex systems sciences* — thermodynamics, network theory, information theory, etc. — to inform theoretical development and empirical studies. The second component is an emphasis on the importance of the *niche* concept as a theoretical construction linking various levels in the ecological hierarchy.

1) *Complexity and Unification*

The professional division of labour among ecologists tends to line up with levels of organization within the ecological hierarchy, making it possible for most ecologists to pursue their investigations without directly confronting the problem of relating population dynamics to ecosystem processes or the ecological dynamics of individual organism-environment systems.

For those who do theorize about such matters, a popular view is that there *are* no scientifically interesting relationships between dE/dt and dN/dt[3]. The issue is often regarded as a question of the autonomy of the biological sciences from the physical sciences. If, for example, one were to claim that changes in population number could be correlated in a systematic way with changes in energy flow within an ecological system, this suggests to many that biological phenomena are somehow a predictable consequence of deterministic physical laws (see, for example, Mayr 1985). But many biologists and philosophers of biology reject the claim that population phenomena are predictable in this way. What drives population change, it is argued, is (in the main, at least) evolutionary selection pressures acting on individual organisms, whose actual effects on total population numbers are a complex product of the interaction of genetic, behavioural and environmental factors. Any given population change may result in a commensurate change in energy flow (say, in the total amount of useful thermodynamic work being performed by the population), but this change in energy flow is not the *cause* of the population change, nor is the correlation a predictable regularity that might function as an ecological law relating dE/dt and dN/dt. Population phenomena must *conform* to physical

---

[3] I include within this category Richard Dawkins, Ernst Mayr, Francisco Ayala, and Michael Ruse — generally, evolutionary theorists and philosophers who identify more with the "British" school of selectionist and adaptationist biological and ecological theory, than with the "German" school of developmental and organismic biological and ecological theory.

laws, of course (the second law of thermodynamics, for example), but they are not to be understood as *manifestations* of physical laws.

The alternative view, that population phenomena are partly or wholly explained as a manifestation of physical laws, is propounded by workers within the "complex systems" approach to biological and evolutionary theory. Though a consensus appears to be emerging on basic structural features of the complex systems model of ecological and evolutionary change, there is still considerable variation in theoretical perspective among workers in this field[4]. The general idea is easily expressed, however. All components of the ecological hierarchy — organisms, populations, communities, ecosystems — are to be regarded as far-from-equilibrium *complex systems* that develop and are structured in accordance with thermodynamic imperatives and constraints. These thermodynamic imperatives push complex systems toward greater states of internal organization, structure and complexity. The result is a common set of emergent dynamical, thermodynamic and self-organizational properties shared by all members of the ecological hierarchy. Certain of these components — organisms and species — may be distinctive in their ability to participate in a characteristic evolutionary dynamic (exhibiting the processes variation, selection, and retention associated with evolution by natural section), but ultimately this evolutionary dynamic may itself be seen as an emergent property of developing ecosystems, a means by which diversity, organizational complexity and structure are generated and maintained within complex systems. At the very least, according to the complex systems approach, evolutionary dynamics must be understood within the context of the co-evolution of systems and environments, and against the background of the order-producing dynamics of self-organizing processes.

---

[4] For discussion of the complex systems tradition in biology and evolutionary theory, see Brooks et al. 1989, Weber et al. 1990, and Weber and Depew 1996.

The idea that a common energetic/thermodynamic currency may be used to describe species interactions and evolutionary dynamics has a long history in ecological thought, going back at least as far as Boltzmann, who said that "[the] struggle for existence is a struggle for free energy available for work" (Boltzmann 1905). Related views were developed by A. J. Lotka (1922, 1925) and H. T. Odum (Odum and Pinkerton 1955; Odum 1983), who argued that the biological systems that prevail in competitive, energy-limited environments are those that maximize their power output, the rate of transformation of energy into work. More recently, ecologist James Brown has defended a more biological version of Lotka's "maximum power principle", asserting that fitness should be defined as "reproductive power", dW/dt, the rate at which energy can be transformed into work to produce offspring (Brown et al. 1993; Brown 1994, 1995).

Optimism concerning the existence of a common energetic currency to describe evolutionary and population phenomena is not shared by all theorists in the complex systems tradition. Some would assert as an obvious fact that "the notion of fitness cannot be reduced to the uniform currency of energetics" (Weber and Depew 1996, 45). Yet all complex systems theorists insist that energetics *is* relevant to natural selection and population dynamics, and that the physical requirements of energy flow *can* be components of fitness.

Though the complex systems approach in ecology is, at the present time, more speculative and less rigorously developed than neo-Darwinian evolutionary theory and population ecology, I am persuaded that some version of the complex systems approach will emerge as a theoretically and empirically satisfying alternative to traditional neo-Darwinism as a foundation for ecological science[5].

---

[5] Note that in saying this I am not committed to a rejection of natural selection as a key mechanism in evolutionary dynamics. What the complex systems approach does entail, however, is a more complex relationship between

The first component of a solution to the unity problem in traditional ecological science, then, is a broadened theoretical perspective that brings into play the diverse resources of the sciences of complexity. These include various forms of dynamical systems theory, network theory, information theory, nonequilibrium thermodynamics, and self-organization theory[6].

2) *The Niche Concept*

In ecology, the "niche" of a species is characterized by the set of environmental resources and conditions that constitute the unique habitat and resource-use requirements of that species, including specific relations to food and enemies. The niche concept is most commonly attributed to species and populations in relation to their environments, but some theorists have applied the concept to individual organism-environment systems[7]. As a concept that relates an organism or population functionally to its environment in terms of resource usage and behavioural constraint, the niche is an important theoretical tool for linking organisms and populations to their biotic and abiotic environments. Ecologist James Brown concurs:

> The concept of niche characterizes the effect of the ecosystem on the species; the extent to which the environment meets the requirements for survival and reproduction and thus limits abundance and distribution. In meeting its niche requirements the [species] does physical work on the ecosystem. In acquiring resources and avoiding intolerable conditions or creating tolerable ones, the species alters the distribution of energy and matter and

---

"essentialist" and "population" modes of thinking in evolutionary theory than is assumed by neo-Darwinians who believe that acceptance of Darwinism implies a complete rejection of essentialism in biology (see Mayr 1975, and Sober 1980, for statements of this view).

[6] See Coveney and Highfield 1995 for a readable introduction to complexity theory.

[7] I examine different variants of the niche concept more thoroughly in Chapter 6.

> the composition of materials and other organisms in its environment. (Brown 1994, 22)

It is through this work that species have their impact on the structure and function of ecosystems:

> The work may take many forms: mechanical — transporting inanimate materials or other organisms; chemical — active uptake of nutrients or photosynthesis; and biological — selective predation on certain species or protection of other species from physical stress or predation. Ultimately, however, all these effects represent physical work, because they are accomplished by the transformation of energy. To assess the effects of species on ecosystems requires understanding the nature, magnitude, and consequences of this work. (1994, 23)

According to Brown, a unified ecology requires a "thermodynamicized" niche theory, one that permits assessments of the energetic impact of individual species on ecosystem processes.

4. Unification and Predictability

In this section I want to consider the issue of predictability in ecological science, and the role that alternative theoretical perspectives can play in discovering phenomenological regularities that might serve as a foundation for predictive generalizations.

Interestingly, James Brown, a supporter of a complex systems approach to unification, is quite pessimistic about the prospects for a predictive unified ecology:

> Unfortunately, I do not see any easy way to make general predictive statements about the impacts of individual species on ecosystems. . . . What traits of kangaroo rats would predict that they would have a greater impact on one shrubland-grassland ecotone than grazing livestock? (1994, 23)

The point, of course, is that different species have different niche requirements, and these will vary from environment to environment.

> The work performed on the ecosystem by each species will depend on the particular abiotic conditions and biotic composition of the ecosystem in which the species is embedded. Furthermore, the nonlinearities that are inherent in any complex system of interactions will cause some small effects to be amplified and other large ones to be diminished, and this in turn will make it very difficult to predict outcomes. Within small, highly specified systems . . . it may be possible to make such predictions, but at the sacrifice of the ability to generalize to other, even superficially similar systems. The impacts of species will be as unique and as dependent on the local environment . . . as the effect of the local environment on the abundance and distribution of species. (1994, 23)

If we grant this pessimistic conclusion concerning predictability, does it follow that the project of a unified ecological science is fatally undermined? To a certain extent, yes, insofar as we are concerned with the ability to make useful generalizations about particular species-ecosystem interactions. Intractable uniqueness, variation and complexity imposes a base level of uncertainty that will often frustrate the search for regularities in ecological phenomena. In particular, these patterns will often not be found in contexts that are most desired for applied ecological and environmental science. Conservation biologists may want to know the impact of the removal of a given species from a given ecosystem, but the necessary empirical regularities that underwrite predictive theories may not *exist* at this level of description.

On the other hand, there is no reason to believe that predictive regularities relating species and ecosystems cannot be found *at all*. The scientific challenge is to discover those patterns of regularity and predictability that *do* exist, and construct theories that allow for explanation and prediction where such regularities occur. Given the complex, hierarchical structure of ecological systems, one should expect that, as one moves from smaller to larger spatio-temporal scales, one will encounter levels of organization at which predictive regularities exist, followed by regions of inter-level complexity that support no useful scientific generalizations (Wimsatt 1994).

Further, the detectability of ecological regularities will depend to a certain extent on the theoretical tools and assumptions that are brought to bear on ecological systems. The development of new theories, and new ways of conceiving ecological phenomena, may enable the detection and exploitation of previously unnoticed regularities. This is precisely the reason why ecological theories derived from non-traditional sources, such as ecological psychology, may be useful in the development of a predictive ecological science, for they may offer a new perspective on the nature of ecological phenomena that reveal phenomenological regularities that could not have been discovered otherwise.

Conclusion

In this chapter I have offered some grounds for pursuing a unification program in ecological science, and suggested that two key ingredients may be important to the success of such a program. The first is adoption of a complex systems framework for understanding ecological and evolutionary phenomena. The second is a theory of the ecological niche that embeds this concept within such a complex systems framework.

We should address a possible concern at this stage. The vision of the philosophy of ecology that I presented at the end of Chapter 3 is one of a heterogenous collection of ecological subdisciplines, with none regarded as foundational to the whole enteprise. Yet in this chapter I defend the notion of a "unified" ecological science that would serve as a framework for subsequent philosophical analysis. This might suggest that my commitment to a nonfoundational pluralism in the philosophy of ecology is disingenuous.

In response, I would describe my position as comparable to that of a philosopher of science who believes it is a good thing to have different people study different aspects of science and different philosophical problems raised by those aspects, and even to hold different positions with respect to these philosophical problems, but who yet, in her own research program, pursues a unified framework for understanding science. Thus, my commitment to pluralism is essentially methodological.

This position is consistent, I believe, with the claim that there exist a variety of ecological disciplines, in fields as distinct as physics, biology, economics, psychology, anthropology and sociology, that ought to be conceived as engaged in a common intellectual activity, namely, the study of ecological phenomena. A theory of ecological phenomena *as such* is necessarily a unifying theory, in the sense that it captures the essential features of the phenomena that make up the domain of these respective subdisciplines. I believe that the complex systems interpretation of ecological science is capable of capturing at least some aspects of all ecological phenomena that are instantiated in physical systems. Some of these may be trivial (e.g. they always involve relationships between a focal system and an external environment), while others, hopefully, are more substantive (e.g. self-organization in systems complex enough to exhibit it).

Chapter 5

Complex Systems Ecology

Introduction

In Chapter 4 I suggested that a key ingredient for a unification program in ecology is the adoption of a complex systems perspective on ecological and evolutionary phenomena. In this chapter I examine the tradition that I call "complex systems ecology". This tradition can viewed as a complex systems approach to ecosystem ecology, one that studies the material, energetic, and informational properties of ecological systems from the theoretical perspective of the sciences of complexity.

1. Ecosystem Ecology

Ecosystem ecology is often described as the study of the flow of matter, energy, and *information* in ecosystems. References to the flow of information entered the lexicon of ecosystem ecology with the rise of the "systems" approach to ecosystem analysis in the mid-1950s, and have proliferated more recently in the literature of the complex systems approaches to evolution and ecology.

If we accept the definition of ecosystem ecology as the study of the flow of matter, energy and information in ecosystems, then the relationship between the various types of ecosystem science can be represented as follows:

Type of Ecosystem Science	*Matter*		*Energy*		*Information*
Biogeochemistry	X				
Ecosystem Energetics	X		X		
Systems Ecology	X		X		X
Complex Systems Ecology	X	⇔	X	⇔	X

Biogeochemistry is the study of elemental, mineral and nutrient fluxes in ecosystems[1]. *Ecosystem energetics* is the study of the flow and dissipation of energy in ecosystems[2]. *Systems ecology* is the name given to the formal, quantitative analysis of ecosystem structure and flows[3]. What I am calling *complex systems ecology* is a form of complex systems theory that has evolved out of more recent developments in the systems ecology, information theory, network theory, and far-from-equilibrium thermodynamics[4].

2. Theoretical Components of Complex Systems Ecology

One can identify three different but mutually supporting research orientations within complex systems ecology (CSE). These orientations are associated with three types of theory: i) hierarchy theory, ii) network theory, and iii) thermodynamics.

1) Hierarchy Theory

Hierarchy theorists focus on hierarchical organization in ecological systems, address problems of scale, aggregation and decomposition in ecological modeling, and study constraint relationships on system dynamics imposed by hierarchical ordering (Pattee 1973; Allen and Starr 1982; Salthe 1985; O'Neill et al 1986; Allen and Hoekstra 1992; Ahl and Allen 1996). Theoretical biology has long been concerned with hierarchical organization in biological systems (von Bertalanffy 1968), but the practical and theoretical usefulness of hierarchy theory is more widely accepted in ecology (Kolasa and Picket [eds] 1994).

---

[1] See, for example, Boorman and Likens 1979.

[2] See Weigert 1988 for a comprehensive overview.

[3] Shugart and O'Neill 1979 is a useful collection of early papers.

[4] Jørgensen 1997 is the best single-text reference for the whole range of theoretical approaches in contemporary complex systems ecology. For a recent collection, see Patten and Jørgensen 1995.

2) Network Theory

Network theorists study network interactions within multi-component systems and formulate measures of network organization and development (Odum 1983; Ulanowicz 1986; Patten 1991). Network approaches have their origins in systems theory and input-output methods derived from theoretical economics (Ulanowicz 1986). Given an interaction or flow matrix for an ecological system, network theory allows one to analyse the contribution that each component of the network makes to overall system behaviour, and conversely, the contribution that the overall system makes to component behaviours.

3) Thermodynamics

Thermodynamicists search for thermodynamic principles which may govern the evolution and development of ordered structures in ecosystems, and complex systems generally (Jorgensen and Meyer 1979; Odum 1983; Brooks and Wiley 1988; Nicolis and Prigogine 1989; Johnson 1992; Schneider and Kay 1994; Swenson 1997). Thermodynamic variables are proposed which complex systems are said to optimize in some way, such as the rate of total entropy production or potential energy dissipation (Patten and Jorgensen 1995).

These three components of CSE are deeply interconnected, and CSE theory may be seen as an attempt to articulate a general framework from which these interconnections can be understood or deduced. The phenomenon of self-organization in complex systems, for example, is often associated with systems maintained far from thermodynamic equilibrium, but it is also a mechanism for the creation of hierarchical levels of organization exhibiting the order and modularity studied by hierarchy theorists (Nicolis and Prigogine 1989). Similarly, though the formation of positive feedback (or "autocatalytic") cycles is essentially a network phenomenon, such cycles also act as agents of ecosystem development and organization which are amenable to thermodynamic description (Ulanowicz 1986).

Though there are significant differences among CSE theorists over the proper understanding of specific mechanisms of complex systems development, and over the correct interpretation of important systems concepts (such as "information"), the degree of agreement over the broad picture of complex systems development is also notable. Attempts to synthesize hierarchical, network and thermodynamic perspectives on ecosystems are topics of current research (Jorgensen 1997), and effectively define the field of CSE as a distinct research endeavour apart from research within its separate components. Recent collaborative works between ecologists, theoretical biologists and philosophers show considerable overlap (cf. Brooks et al 1989; Weber et al 1990).

3. Ecosystem Phenomenology

What precisely are the phenomena that CSE is trying to explain? At a general level these include the brute fact that nature is structured in hierarchical levels that can be decomposed into weakly interacting subsystems, and that levels of organization seem to develop and co-evolve with the entities that reside at that level (Wimsatt 1994, 242). More concretely, natural historians have long observed and noted regularities in patterns of ecological succession, and ecosystem ecologists have constructed lists of regularities which are intended to characterize the gross phenomena of ecosystem development. The most famous of these lists is found in Eugene Odum's (1969) "The Strategy of Ecosystem Development" (see Table 5.1).

Note that Odum's list is really a collection of *hypotheses* concerning ecosystem phenomenology rather than a list of *observed* regularities.

Ecosystem Attributes	Developmental Stages	Mature State
	Community	*Energetics*
Gross production/ community respiration (P/R ratio)	Greater or less than 1	Approaches 1
Gross production/ standing crop biomass (P/B ratio)	High	Low
Biomass supported/ unit energy flow (B/E ratio)	Low	High
Net community production (yield)	High	Low
Food chains	Linear, predominantly grazing	Web-like, predominantly detritus
	Community	*Structure*
Total organic matter	Small	Large
Inorganic nutrients	Extrabiotic	Intrabiotic
Species diversity-variety component	Low	High
Biochemical diversity	Low	High
Stratification and spatial heterogeneity	Poorly organized	Well organized
	Life	*History*
Niche specialization	Broad	Closed
Size of organism	Small	Large
Life cycles	Short, simple	Long, complex
	Nutrient	*Cycling*
Mineral cycles	Open	Closed
Nutrient exchange rate, between organism and environment	Rapid	Slow
Role of detritus in nutrient regeneration	Unimportant	Important

	Selection	*Pressure*
Growth form	For rapid growth ("r" selection)	For feedback control ("k" selection)
Production	Quantity	Quality
	Overall	*Homeostasis*
Internal symbiosis	Undeveloped	Developed
Nutrient conservation	Poor	Good
Stability (resistance to external perturbations)	Poor	Good
Entropy	High	Low
Information	Low	High

Table 5.1: Trends to be expected in ecosystem development. From Odum (1969).

A considerable amount of data collection and analysis is required to confirm or disconfirm any one of these hypotheses. Indeed, the great majority of empirically-oriented ecosystem studies can be conceived as attempts to correct and refine our understanding of what the phenomena of ecosystem development actually are. Joel Hagen notes that Eugene Odum's inventory of hypothetical trends in ecosystem development was an important stimulus to the Hubbard Brook study (Hagen 1992, 185), as it offered clear statements of empirical regularities that could be the subject of experimental studies. In fact, Bormann and Likens presented evidence that contradicted several of Odum's hypotheses. In the forest ecosystem which they studied, Bormann and Likens found that both biomass and species diversity reached a maximum during the aggradation phase of development and then *declined* as the ecosystem reached maturity. Nor was ecosystem stability related to biological diversity in as simple a manner as was previously thought by ecosystem ecologists. The stability-diversity thesis, equilibrium concepts and monotonic progression models of ecological succession have all come into question in recent years (McIntosh 1985), and the establishment of phenomenological regularities across a broad spectrum of ecosystem types remains a challenge for ecosystem ecology.

CSE theorists are not overly concerned by the lack of consensus on specific phenomenological principles. CSE is aimed at explaining and unifying *broad sets* of phenomenological trends rather than the details of specific processes. For example, ecologist Robert Ulanowicz points out that

> the largest number of Odum's attributes (2, 3, 7, 15, 16, 17, 20, and 21) can be construed in some way to imply that mature systems exhibit more cycling and greater internalization of medium. That is, the system tends to conserve medium both by storing it in the components and by cycling it within the system. (Ulanowicz 1986, 123)

It is these broad patterns of increased cycling, storage, and development away from thermodynamic ground that are the subject of Ulanowicz's theory, and CSE theory generally. James Kay (1994, 13) gives a list of ten features of ecosystem organization that his exergy-based, dissipative systems approach to complex systems is intended to explain or illuminate (see Table 2). Kay's list effectively captures the level of generality that systems ecology is capable of addressing at this stage of its development.

1. More energy capture. *Inflow*
2. More effective use of energy. *Exergy destruction rate*
3. More energy flow activity within the system. *Total system throughput*
4. More cycling of energy and material.
   A) Greater numbers of cycles. *Number of cycles*
   B) Longer cycles. *Average cycle length*
   C) The amount of material flowing in cycles (as versus straight throughflow) increases. *Finn cycling index*
   D) Turnover time of cycles or cycling rate decreases. *Decrease in production/biomass (P/B) ratio*
   E) Less leaking of material out of the system. *Exports*
5. Higher average trophic structure

	A) Longer trophic food chains. *Number of trophic levels in the Lindemann spine*
	B) Species will occupy higher *average trophic levels*
	C) Greater *trophic efficiencies*
6.	More articulated food web. *Ascendency*
7.	Higher *respiration*
8.	Higher *transpiration* in terrestrial systems
9.	Larger ecosystem *biomass*
10	More types of organisms (higher *diversity*)

Table 2: Patterns and measures of ecosystem development. From Schneider and Kay (1994)

4. Information Flow

Both systems ecology and complex systems ecology deal with the concept of information flow, but the older systems ecology used the concept of information in its *syntactic*, and *dynamical* forms, while CSE shares with other forms of complex systems theory the ambition to construct physical theories of meaningful, *semantic* information. The double-headed arrows in the diagram of section 1 indicate that CSE is concerned with the relationships between information, energy and matter that underlie the self-organizing processes of complex natural systems.

What do I mean by the terms "syntactic", "dynamical" and "semantic" with respect to the concept of information? Roughly the following: "Syntactic" information is used to describe the information concept that developed out of Claude Shannon's mathematical information theory (Shannon and Weaver 1949). Mathematical information theory is a formalism for describing the information-carrying capacity of a communication channel, and is unconcerned with the *meaning* of the messages that are sent and received. It is basically a theory detailing how probability distributions change as functions of changing constraints, which may be epistemic (e.g., learning the outcome of an experiment) or nonepistemic (e.g. change in the network structure of an ecosystem). Mathematical information theory has been used in ecology as an index of biological diversity, and to quantify the multiplicity of flow pathways among the components of food webs and ecosystems.

"Dynamical" information refers to the phenomenon whereby a large change in the dynamical activity of a system *A*, as measured by the magnitude of energy and momentum exchanges, may be controlled by a small dynamical change in another system, *B*. The small amount of energy required to flip a switch or turn a dial may initiate a rocket launch; the internal "signal" that an organism is running low on fuel reserves may initiate a complex and energetically costly set of foraging or hunting behaviours; a radio-controlled airplane responds to the low-energy muscular and electrical activity of the a young child holding the controller — these are all examples of informational connections between one system and another. The dynamical notion of information control is present in the writings of some systems ecologists, and is expressed in the belief that ecosystems are "cybernetic systems" (Patten and Odum 1981). Whole-system behaviours are thought to be regulated by feedback relationships among ecosystem components, which determine (in part) how matter and energy flows through the ecosystem.

"Semantic" information involves the concepts of reference, meaning, and intentionality. The paradigm cases of semantic information flow involve everyday experiences of linguistic communication among human beings, in which linguistic symbols are employed that (somehow) convey messages with content, about ideas or states of affairs, or whatnot. Yet biology is replete with the vocabulary of communication; we see such terms as "recognition", "messenger-RNA", and "signaling" throughout the pages of modern textbooks in biochemistry and molecular biology, and we say that information is "coded" in DNA, and this information contains the genetic "program" for the construction of bodily forms. The notion of information exchange that is expressed in these terms has affinities with the syntactic and dynamical notions of information, but it also has a connection to semantic notions of meaning and reference. When I say that CSE theorists seek to develop physical theories of semantic information, I mean they are attempting, like many complex systems researchers, to discover the physical/dynamical foundations of the kinds of semantic information properties that are characteristic of biological systems.

5. Network Models of Ecosystems

One cannot gain a full appreciation of what CSE theory is about without acquiring a familiarity with the models that are used within the discipline. In the remaining sections I will explore one set of models, network models, of ecosystem organization and development. The presentation is fairly technical, but it will serve as an important foundation for later discussions of conceptual issues in the foundations of complex systems theories, the subject of the last two chapters of the dissertation. I also want to use this discussion to illustrate the claim often made by CSE theorists that ecological systems are irreducible wholes that possess system-level properties that are not reducible to the properties of their component parts. Specifically, I will look at some arguments for holism derived from the formal apparatus of network ecology as found in the work of CSE theorists Bernard Patten and Robert Ulanowicz. As mentioned above, network ecology is a branch of systems ecology and CSE theory that represents ecological systems as a set of compartments linked together via a network of pathways through which energetic and material substances flow. The arguments for holism are grounded in certain generic properties of complex physical networks which arise as a consequence of the cycling of energy and matter in closed loops. In the literature these are known respectively as i) *the dominance of indirect effects*, ii) *network amplification*, iii) *network homogenization*, and iv) *network "ascendency"*. The first three network properties are derived from matrix methods used in network theory, and are associated with the work of Bernard Patten (Patten 1985, 1989, 1991). The fourth property, network "ascendency", is based on the information-theoretic network analysis of Robert Ulanowicz (1986, 1997).

6. The Network Formalism[5]

Figure 5.1 depicts a network of energy exchanges for the Cone Spring ecosystem measured in kcal/m^2/y (Tilly 1968). The arrows pointing outward represent exports of energy in a form still usable to other systems. At each node, the second law of thermodynamics requires that a certain amount of energy be dissipated. These respirational flows are given the special ground symbol.

When the numbers of components and flows becomes large, pictorial representation of a network becomes cumbersome, and analytical methods require a more abstract way of portraying flow networks. Matrix algebra can be used to represent networks of any size and perform various analytical calculations. Matrix methods for analyzing flows in networks are derived mainly from economic theory, and the technique is sometimes called "flow" or "input-output" analysis. Bruce Hannon (1973) was the first to use input-output analysis in an ecological context.

It is useful to describe a network in terms of single $n \times n$ square matrix and three n-element column vectors. In the Cone Spring example there are five nodes and eight internal transfers. Calling Tij the transfer of energy from compartment i to compartment j, one can represent the Tij as the elements of a 5 $\times$ 5 matrix:

$$[T] = \begin{bmatrix} 0 & 8881 & 0 & 0 & 0 \\ 0 & 0 & 5205 & 2309 & 0 \\ 0 & 1600 & 0 & 75 & 0 \\ 0 & 200 & 0 & 0 & 370 \\ 0 & 167 & 0 & 0 & 0 \end{bmatrix}$$

The external inputs to compartment i are denoted by Di (for "donor"), the exports from compartment i by Ei, and the respiration by Ri:

---

[5] The following presentation of network theory and input-output flow analysis draws heavily from Ulanowicz (1986).

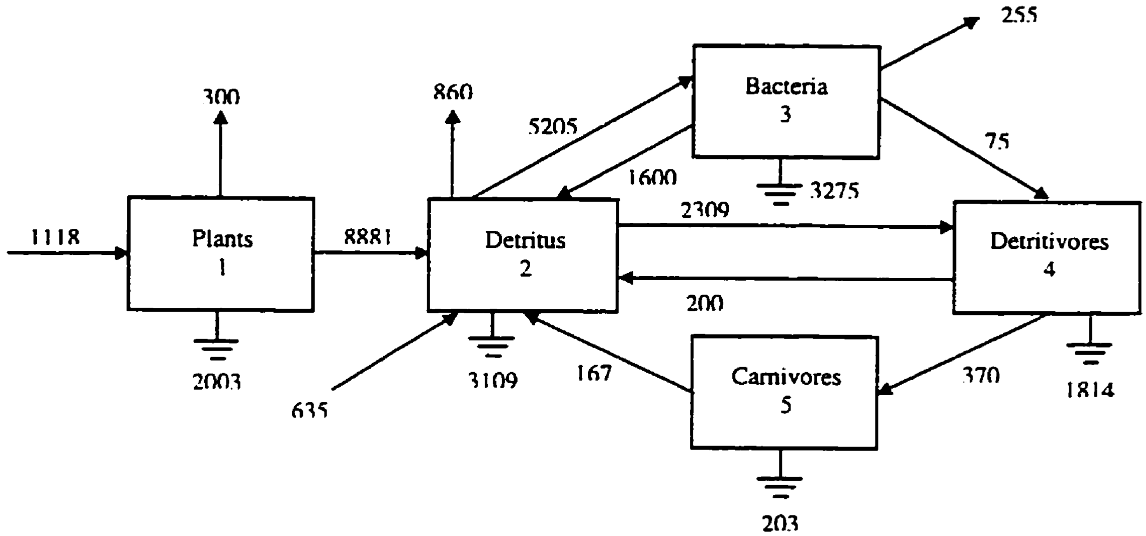


Figure 5.1: Network representation of energy exchanges in the Cone Spring ecosystem (Tilly 1968), including imports, usable exports, and dissipations. (Redrawn from Ulanowicz 1986, 32)

$$[D]=\begin{bmatrix}11184\\635\\0\\0\\0\end{bmatrix},\ [E]=\begin{bmatrix}300\\860\\255\\0\\0\end{bmatrix},\ [R]=\begin{bmatrix}2003\\3109\\3275\\1814\\203\end{bmatrix}.$$

The analysis of flows is facilitated when the system in question is at steady-state —that is, when the sum of all the inputs exactly balances the sum of all the outputs for each node — but systems not at steady-state can be analyzed with flow analysis as well.

The description of flow topology is done in terms of quantities that are independent of the magnitudes of the flows. A normalizing factor for each node is found by summing either the inputs or the outputs of the given node:

(1) $$T_i' = D_i + \sum_{j=1}^{n} T_{ji}$$ (total inputs to compartment *i*)

(2) $$T_i = \sum_{k=1}^{n} T_{ik} + E_i + R_i$$ (total outputs from compartment *i*)

Ti = *Ti′* when the component is at steady state. The *Ti* and *Ti′* are called *compartmental throughputs* or *throughflows* and describe the level of flow activity through the respective compartment. The *size* of the entire system (in terms of flows) is the sum of all the flows in the system, and is called the *total system throughput* (TST):

(3) $$TST = \sum_{j=1}^{n}\sum_{i=1}^{n} T_{ij} + \sum_{i=1}^{n}(E_i + R_i) + \sum_{j=1}^{n} D_j$$

Input-output analysis allows one to relate the throughputs of each compartment to the exit flows from the system. Rearranging (2) to solve for the transfers outside the system gives

(4) $$E_i + R_i = T_i - \sum_{k=1}^{n} T_{ik}.$$

We can write this relationship in matrix-vector notation by defining an identify matrix $[I]$ and a matrix of partial "feeding" coefficients $[G]$ where the constituent element g_{ik} represents the fraction of the total input to k that comes directly from i (i.e., $g_{ik} = Tik/T_k$). Then (4) becomes

(5) $$[E]+[R]=\{[I]-[G]\}[T].$$

For the sake of brevity one often writes the matrix inside the braces simply as [I - G], and it is referred to as the *Leontief matrix* (Leontief 1951). Solving (5) for the throughput in terms of the outputs gives

(6) $$[TST]=[I-G]^{-1}\{[E]+[R]\}$$

where the superscript indicates matrix inversion. The matrix $[I - G]^{-1}$ is called the *input structure matrix*, or the *Leontief inverse* (Leontief 1951). It relates the activity of any component to the final exports and internal consumption of the system.

One can perform the same manipulations from the other direction, defining a matrix of partial "host" coefficients $f_{ij} = T_{ij}'/T_i'$. Substituting into (1) yields the expression

(7) $$[TST]'=[I-F^T]^{-1}[D]$$

where $[TST]'$ is total system throughput, $[F]$ the matrix of partial host coefficients, and $[D]$ the vector of inputs to the system. The matrix $[I - F]$ is known as the *Augustinovics matrix* from input-output theory (Augustinovics 1970). $[I - F^T]$ is a transposed version of the Augustinovics matrix (used because all vectors are being treated as column vectors here), but we will call it by the same name. $[I - F^T]^{-1}$ is known as *the output structure matrix* or the *Augustinovics inverse*. The throughputs of each compartment have now been related to the system's external inputs.

Knowing the output structure matrix it is easy to calculate the ultimate fate of any unit of input to the system. Let the input vector $[D]$ be a unit vector. Equation (7) then yields $[TST]'$, the matrix of throughputs for each compartment that would result from the single unit input. The accompanying internal transfers T_{ij} are calculated by denormalizing the $[F]$ matrix according to the

$[TST]'$ vector just calculated (i.e. by multiplying each f_{ij} with the respective T_i'). The sum of the exports and respirations may be determined by balance and then apportioned in the same ratio as they appear in the full network. For the Cone Springs example, the matrix of partial host coefficients is

$$[F]=\begin{bmatrix}0 & .794 & 0 & 0 & 0\\ 0 & 0 & .453 & .201 & 0\\ 0 & .307 & 0 & .014 & 0\\ 0 & .084 & 0 & 0 & .155\\ 0 & .451 & 0 & 0 & 0\end{bmatrix}$$

Now let's say we want to trace the fate of a singe unit of input to the *detritus* compartment. We calculate the output structure matrix for the network,

$$[I-F^T]^{-1}=\begin{bmatrix}1.000 & 0 & 0 & 0 & 0\\ .958 & 1.210 & .374 & .186 & .545\\ .434 & .547 & 1.170 & .084 & .247\\ .199 & .251 & .092 & 1.040 & .113\\ .031 & .039 & .014 & .161 & 1.020\end{bmatrix},$$

and then multiply this matrix by a unit vector for the compartment 2, the detritus compartment — i.e. a column vector with one in the second position and zero elsewhere — yielding the throughputs generated by the unit input. This is just the second column vector of the output structure matrix,

$$[TST]_2'=\begin{bmatrix}0\\ 1.210\\ .547\\ .251\\ .039\end{bmatrix}.$$

To get the transfer matrix generated by this unit input, you multiply each row of $[F]$ by its corresponding throughput in $[TST]_2'$:

$$[TST]=\begin{bmatrix}0 & 0 & 0 & 0 & 0\\ 0 & 0 & .547 & .243 & 0\\ 0 & .168 & 0 & .008 & 0\\ 0 & .021 & 0 & 0 & .039\\ 0 & .018 & 0 & 0 & 0\end{bmatrix}.$$

One then subtracts internal outputs from internal inputs to find the sum of exports and respirations from each component. The fraction of this sum which is respired/exported is estimated by the ratio of total usable exports and respirations for the whole system. Figure 5.2 shows the fate of the unit input to the detritus compartment for the Cone Spring ecosystem.

One can use the input and output structure matrices to analyze any direct flow within the system. But what about *indirect* flows? Consider a simple chain of flows $A \rightarrow B \rightarrow C \rightarrow D$. *A* and *B* are related by direct flows, but *A* and *C* are related by an indirect flow that passes through *B* first, and *A* and *D* by a longer indirect flow passing through *B* and *C*. If one asks only for the contribution to the input of *D* from direct flows (pathways of length 1) one need only count the contribution from C, but if one asks for the contribution of all pathways of length 2, then one must include *B*, and so forth.

The input and output structure matrices also contain information on the magnitudes of all indirect flows occurring between any two components of the system. Recall that the components of $[F]$ represent the fraction of flow through compartment *i* which proceeds *directly* to compartment *j*. If one multiplies $[F]$ by itself, the result is denoted by $[F]^2$. Inspection of the product matrix reveals that the *i-j*th component of $[F]^2$ represents the fraction of the total flow through *i*, which flows into j along all pathways of exactly *two* transfers. If one asks the question, "What is the total fraction of T_i that flows to compartment *j* along all pathways of *all* lengths?", the answer is obtained by summing *all* the powers of $[F]$, that is,

(8) $$\sum_{m=1}^{\infty}[F]^m = [F] + [F]^2 + [F]^3 + \ldots$$

If there are no cycles in the network, the powers of $[F]$ will always truncate prior to reaching $[F]^n$ (where *n* is the number of compartments). If cycles are present in the network then the powers of $[F]$ form an infinite sequence. In this case the

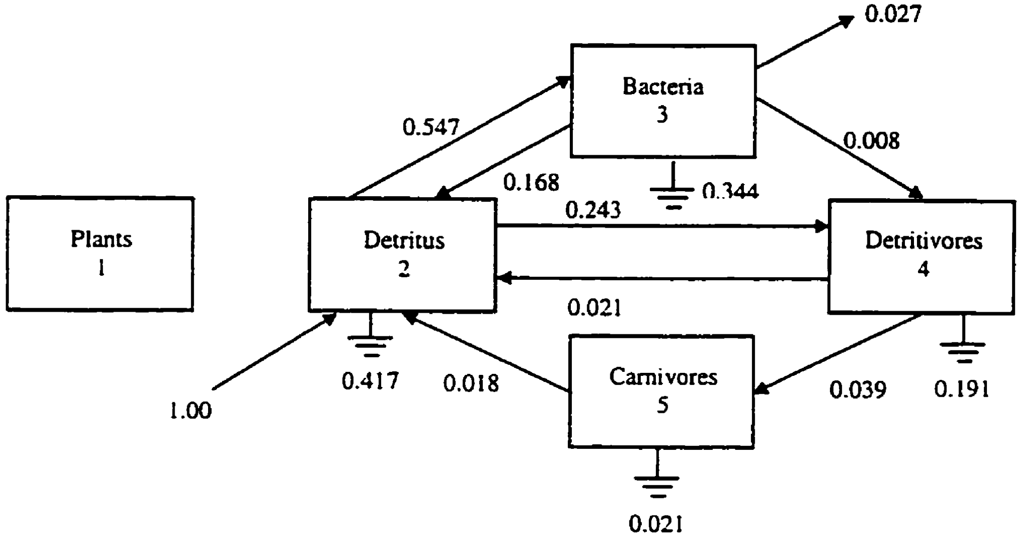


Figure 5.2: The fate of a single unit of energy accompanying the detritus imported into the Cone Spring ecosystem. (Redrawn from Ulanowicz 1986, 41)

series may diverge or it may converge to a finite limit[6]. If one adds the identity matrix ($[F]^0$) to the series, the limit of the series is a well known expression that we learn in high school with our first exposure to limits,

$$(9) \qquad \sum_{m=0}^{\infty}[F]^m = [F]^0 + [F]^1 + [F]^2 + \ldots = [I - F]^{-1}.$$

But this limit is nothing more than the Augustinovics inverse. Thus, if one subtracts unity from each diagonal element of the output structure matrix, the *i-j* component of the resulting matrix represents exactly the fraction of T_j flowing to *i* over *all* possible pathways. An exactly parallel argument shows that the components of the Leontief inverse matrix $[I - G]^{-1}$ represent the fraction of T_j which is *dependent upon i* via all pathways of all lengths.

7. Network Properties

Matrix methods can be used to investigate many properties of ecological systems, including energy efficiencies in transfers through food chains, food web analysis, rates of production, consumption and decomposition, and mineral and nutrient cycling. Here we want to focus on properties of network structure and function which are regarded as "holistic" by practitioners. We now have sufficient background to discuss the first three network properties — dominance of indirect effects, network amplification and network homogeneity. We will

---

[6] Patten et al (1976) discuss the necessary and sufficient conditions for the convergence of this series, which include the requirement that all eigenvalues of [F] be less than one in modulus, and at least one norm of [F] must be less than one. When these conditions are translated into the network formalism they amount to the simple requirement that the network not be isolated, that it be open to external inputs. That is, a network analysis of a system at a given focal level must necessarily make reference to the environment of that system. The ultimate significance of this connection between the existence of a complete account of the influence of network structure on individual components ("causal closure" of the network, as Patten puts it) and the thermodynamic openness of the network remains to be evaluated.

need to introduce Ulanowicz's information-theoretic formalism before discussing network ascendency.

Dominance of Indirect Effects, Network Amplification, and Network Homogenization

Recall that the consecutive powers of [*F*] and [*G*] represent contributions to the throughput of a component due to successively longer pathways in the network. An interesting question to ask is "Is the contribution of the *indirect* portion ($[F]^2$ + $[F]^3$ + . . .) of the throughput greater or less than the contribution of the *direct* portion ([*F*])?". That is, what is the ratio of indirect to direct flows?

Higashi and Patten (1989) have shown that it is a mathematical consequence of network structure that the ratio of indirect to direct flows in a network *increases* with increasing (a) system *size* (number of components) (b) system *connectivity* (density of interactions), (c) compartment *storage* (flow impedance), (d) feedback and nonfeedback *cycling*, and (e) *strength of direct flows*. In fact, as a network becomes larger and more complex, the contribution of the indirect flows tends to *exceed* the contribution of the direct flows; that is, I/D is greater than one. This result is known as the **dominance of indirect effects.**

That indirect flows could dominate direct flows is somewhat surprising given that flow magnitudes diminish exponentially as path lengths increase as a result of the dissipation which accompanies all energy-matter transactions. The dominance effect is due to the fact that complex networks have *closed loops* which allow matter and energy to cycle through the system. The more complex the network, the greater the ability of the network to trap, store and cycle matter and energy. This allows for the possibility of successive cycles through the system to contribute more to a given throughput than the direct transfers. But dissipation occurs at every stage in the cycle, so eventually the magnitudes of the transfers fall off and the sum converges to a finite limit.

Two other network properties can be deduced from the formalism which are closely related to the dominance of indirect effects. The first is called

network amplification. This occurs when one unit of input to component *A* in the network is "amplified" to produce *more* than one unit of throughflow at component *B*. This sounds like it should violate the second law of thermodynamics, but it is due to *recycling* of the same, as yet undissipated, organic-matter bond energy that originally reached compartment *B* from *A* through direct transfers. The first-passage transfer is strictly nonamplifying.

The second network property is called **network homogenization**. Some compartments receive more external inputs than others so that that energy-matter flows in networks tend to be initially heterogeneous at the system boundaries (for example, only plants receive solar energy inputs). The power series matrices which represent the cumulative effects of network transfers of all path lengths tend to have equal values rowwise and columnwise, indicating a more or less uniform distribution of mass and energy through the network. The effect of the network structure on the original inputs is thus to "smooth out" and distribute flows homogeneously throughout the network.

8. Information Theory and Network "Ascendency"

A related network effect which is often described as "holistic" is the effect of *autocatalytic feedback* on network structure. Systems ecologist Robert Ulanowicz has developed a theory of ecosystem growth and development which he calls "ascendency theory", and which is based on the effects of autocatalytic feedback on network organization (Ulanowicz 1986, 1997). Autocatalysis, or "indirect mutualism" (the mutual reinforcement of three or more components of a network), it is argued, is the "agent" that drives ecosystem growth and development.

Ulanowicz uses *information theory* to quantify network growth and organization. In this section I will show how the input-output formalism can be interpreted in information-theoretic terms and used to quantify the growth and development of ecosystems, and how autocatalytic feedback can function to drive a system to greater levels of growth and development.

Information Theory

Information theory is a formalism for quantifying changes in probabilities or probability distributions. Given a series of events $e_1, e_2, \ldots, e_n$, with prior probabilities $p_1, p_2, \ldots, p_n$ (where each p_i is less than or equal to one, and the sum of p_i's is one), then the "uncertainty" H of a given event occurring is a function of its inverse probability, $1/p_i$. The measure of uncertainty should satisfy certain desiderata: i) it should be non-negative ($H(p_i) \geq 0$), ii) it should be decisive when there is no residual uncertainty ($H(1) = 0$); and iii) co-occurrences of two unrelated outcomes should equal the sum of the uncertainties of the individual outcomes ($H(p_i,q_j) = H(p_i) + H(q_j)$). It can be formally demonstrated that only the logarithmic function

(10) $$H_i = K \log (1/p)$$

(where K is an undefined constant of proportionality) satisfies the three requirements. The average uncertainty H of a probability distribution is found by weighting the uncertainties of each event by the probability for that event. Thus,

(11) $$H = -K \sum_i p_i \log p_i \,.$$

Uncertainty is intuitively greatest when there is no reason for expecting one event over another, when all events are equally probable. The average uncertainty is then

(12) $$H_{\max} = -K \sum_i (1/n) \log(1/n),$$

or

(13) $$H_{\max} = K \log n.$$

Any knowledge that is acquired which results in a reduction of uncertainty is "information". If the prior probability of an event is given by p_i^*, then the information gained in the change to the new probability p_i is

(14) $$(-K \log p_i^*) - (-K \log p_i)$$

or

(15) $$K \log (p_i/p_i^*).$$

The average (expected) decrease in uncertainty for a probability distribution is thus

(16) $$I = K\sum_i p_i \log(p_i / p_i^*\}.$$

The joint probability $p(a_j, b_i)$ is the probability that events a_j and b_i will occur together or in a given sequence. If the joint probability is normalized by the overall probability that a_j occurs, the result is the conditional probability

(17) $$p(b_i \mid a_j) = p(a_j, b_i)/p(a_j).$$

Now, what is the reduction in uncertainty about b_i provided by a knowledge of a_j? The prior uncertainty about b_i is given by equation (10),

(18) $$H(b_i) = -K\log p(b_i).$$

The decrease upon knowing a_j is

$$[-K \log p(b_i)] - [-K \log p(b_i \mid a_j)]$$

$$= K \log p(b_i \mid a_j) - K \log p(b_i)$$

(19) $$= K \log [p(b_i \mid a_j)/p(b_i)].$$

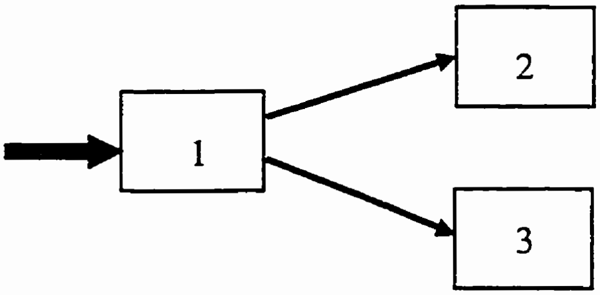
This expression need not be positive for every pair of occurrences i and j, but when each term is weighted by the corresponding joint probability, one obtains the non-negative quantity called the *average mutual information* (*AMI*),

(20) $$AMI(b;a) = K \sum_i \sum_j p(a_j, b_i) \log[p(b_i \mid a_j) / p(b_i)].$$

which represents the amount of the original uncertainty (H) about b_i, which is resolved by a knowledge of a_j.

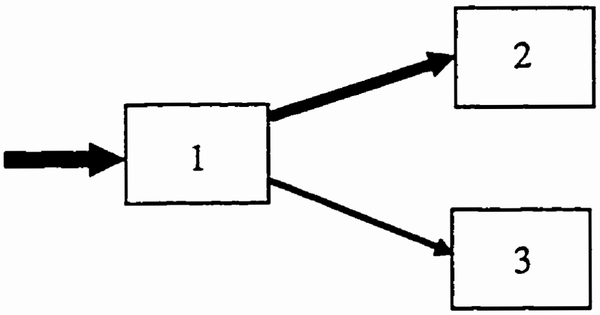
In communications theory the a_j's and b_i's are usually taken to represent, respectively, the sending of the jth cipher and the reception of the ith cipher in a communication channel. But information theory is applicable whenever there are changes in probability assignments. There is no need to interpret all applications of information theory in the "sender-message-receiver" vocabulary of communications theory. Nor is there any need to interpret the probabilities in epistemic terms as measures of subjective ignorance. It is often convenient to introduce definitions using the language of personal probabilities, but as is well known, subjective interpretation of probabilities is neither an inherent feature of probability theory nor information theory. Ulanowicz uses information theory to quantify changes in the network properties of an ecosystem as it grows and develops. These properties are objective features of the system, and the changes in probability assignment are measures of these changes, not measures of ignorance or degree of belief of an observer.

Consider three nodes of a network connected by flows as in the following diagram:



The widths of the arrows indicate the magnitude of the flows. In this case the flow into node 1 is equally distributed to nodes 2 and 3, i.e. half the flow goes to 2 and half to 3. Let us ask the question, "what are the constraints on the action of a quantum of medium upon exiting node 1?". That is, given the possible paths it *could* follow, are there any constraints on the path that it *will* follow. In this case there are no constraints; given that it exits from node 1, the quantum is as likely to end up in node 2 as in node 3. That is, $p(2 \mid 1) = p(3 \mid 1) = 1/2$.

Now consider the situation if we change the path weightings:



Now node 2 receives twice as much flow as node 3. A quantum of medium entering node 1 is now constrained by the disproportionate flow weightings, and we have $p(2 | 1) = 2/3$, while $p(3 | 1) = 1/3$. If the earlier configuration represented a state of maximum uncertainty (H_{max}) in the behaviour of the quantum entering the system, then the new configuration represents a reduction in uncertainty, and hence *an increase in information.*

The interpretation of the conditional and joint probabilities in equation (20) for average mutual information should now be clear. Let T_{ji} be the flow from compartment j to compartment i, T_j the sum of all the outputs from compartment j, T_i' the sum of all inputs to compartment i, and TST the total system throughput. The probability $p(a_j)$ is estimated by

(21) $$p(a_j) = T_j/TST,$$

and the probability $p(b_i)$ by

(22) $$p(b_i) = T_i'/TST.$$

For later convenience we denote $p(a_j)$ as Q_j, and $p(b_i)$ as Q_i'. Now the only way for a quantum to *both* leave j *and* enter i is for it to be part of the flow T_{ji}. Therefore the *joint probabilities* are estimated by

(23) $$p(a_j, b_i) = T_{ji}/TST.$$

The *conditional probabilities* are given by Bayes' Theorem,

(24) $$p(b_i | a_j) = p(a_j, b_i)/p(a_j) = T_{ji}/T_j,$$

but notice that this term is simply the coefficient f_{ji} from the matrix of partial host coefficients [F], i.e. $T_{ji}/T_j = f_{ji}$. This allows us to write the joint probability as the product of the host coefficent and Q_j

(25) $$p(a_j, b_i) = p(b_i | a_j)\, p(a_j) = f_{ji}Q_j.$$

Finally, substituting all these terms into the equation for mutual information gives

(26) $$AMI(b;a) = K\sum_{i=0}^{n+2}\sum_{j=0}^{n+2} f_{ji}\, Q_j \log\left(f_{ji} / Q_i'\right).$$

The indices run from 0 to $n + 2$ because we need terms to represent external inputs to the system (the 0 term), a sink for all useable exports (the $n + 1$ term) and a sink for all dissipations (the $n + 2$ term).

Figure 5.3 shows three closed flow networks that have identical system throughputs (96 units). The network in Figure 5.3(a) is maximally connected, with each compartment exchanging medium with all compartments in equal amounts. Knowing that a quantum of medium is leaving compartment 1 gives you no information about where it will end up. Measured in units of *K*, the average mutual information (*AMI*) calculated for this configuration from equation (26) is 0.

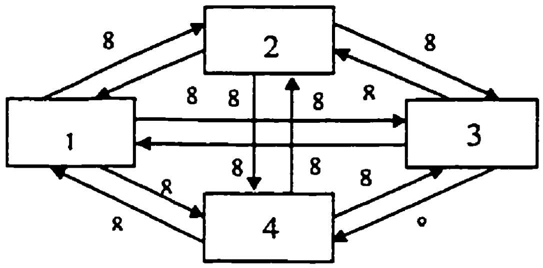
The network in Figure 5.3(b) is better articulated, with greater determinacy in the flow structure. If we know that a quantum of medium is exiting compartment 1, you know that it's not going to compartment 4, but there's a 50-50 chance of it going to compartment 2 or 3. The *AMI* for this configuration is equal to 1 (in units of *K*).

The network in Figure 5.3(c) is maximally articulated, with no indeterminacy or uncertainty in the flow of a quantum of medium leaving any compartment any where in the network. The *AMI* for this configuration is equal to 2 (in units of *K*).

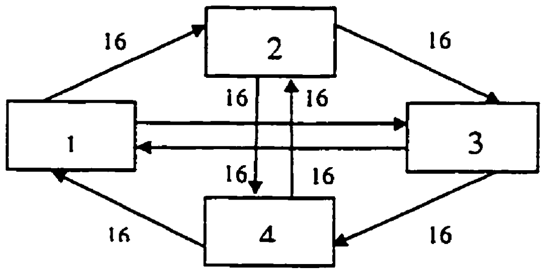
AMI gives a measure of the organization of a network, but it gives no indication of the size of the network as measured, say, by total system throughput (*TST*). The constant *K* has been retained in all the information expressions, but it has not been defined. A natural way of connecting *AMI* to network size is to set *K* equal to the *TST* of the network. The resulting quantity is given by

(27) $$A = TST \sum_{i=0}^{n+2} \sum_{j=0}^{n+2} f_{ji} Q_j \log\left(f_{ji} / Q_i'\right).$$

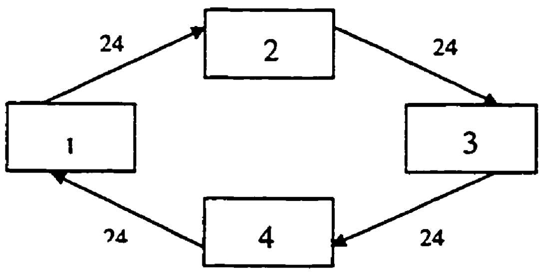
This quantity is a measure of the product of network size and network organization. Ulanowicz calls it the "network ascendency". The ascendencies



(a) Total System Throughput (TST) = 96
Average Mutual Information (AMI) = 0
Ascendency (TST x AMI) = 0



(b) Total System Throughput (TST) = 96
Average Mutual Information (AMI) = 1
Ascendency (TST x AMI) = 96



(c) Total System Throughput (TST) = 96
Average Mutual Information (AMI) = 2
Ascendency (TST x AMI) = 192

Figure 5.3: Three flow networks with identical system throughputs, but different network structures.

for networks (a), (b) and (c) in Figure 5.3 are respectively 0, 96, and 192 kcal "bits"/m^2/yr. When a network grows in size or increases its degree of organization, its ascendency A rises. An increase in A is thus a measure of "growth" and "development", in purely network-theoretic terms.

Ulanowicz argues that autocatalytic network cycles are powerful network-level agents of growth and development. In its simplest form autocatalytic feedback occurs when the activity of a given component increases the activity of one or more other components that in turn increase the activity of the original element still more. Such feedback is usually represented graphically in the form of a unidirectional closed cycle, as in Figure 5.4.

Ulanowicz argues that such cycles exhibits five characteristics that justify attributing a form of causal agency to the feedback cycle itself. A positive feedback cycle is (a) semi-autonomous, (b) emergent, (c) growth enhancing, (d) selective, and (e) competitive (Ulanowicz 1986, 54-61).

Autonomy

A perfectly closed cycle has no external inputs and is in this sense an autonomous entity; the functioning of any component depends only on itself and the activity of the other components in the cycle. Real cycles obey thermodynamic constraints and are always open to external inputs and dissipative outputs, hence the term "semi-autonomous".

Emergence

Closed cycles may only be apparent at certain scales of observation. Suppose, for example, that at a given level of observation one sees only a subset of the components in a particular cycle, as in Figure 5.5. There are no cycles in the path connecting the components in the subset. Only by expanding the scale observation does cycling become evident. This is what Ulanowicz means by "emergence"; the feedback cycle only appears at a certain scale of observation.

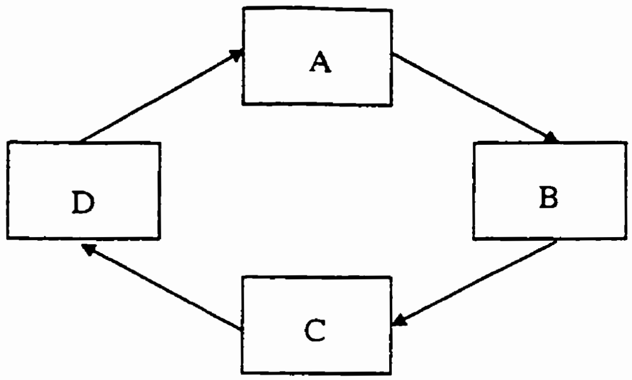


Figure 5.4: An autocatalytic feedback cycle.

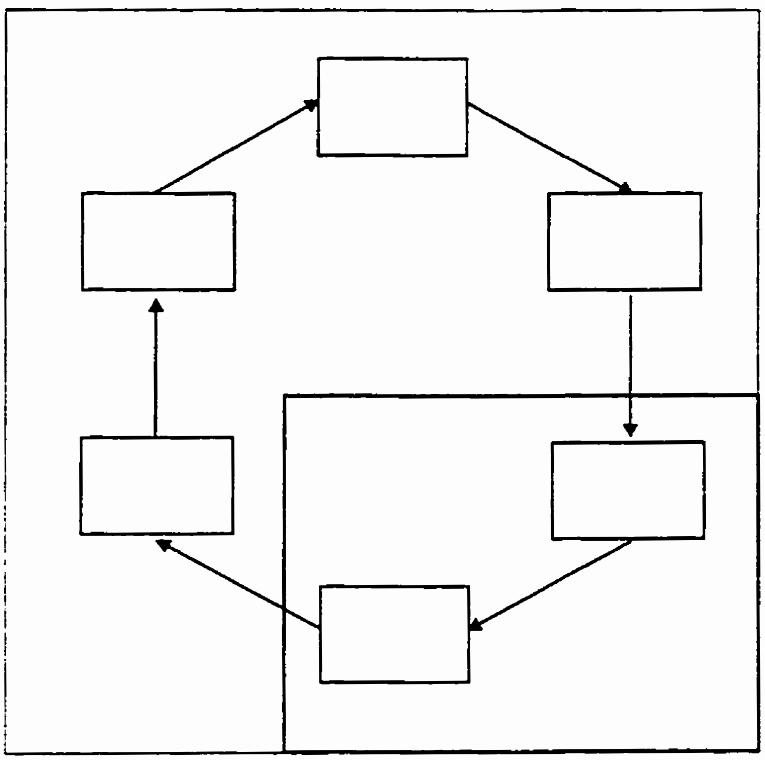


Figure 5.5: Enlarging the scope of a system to include an entire feedback loop.

Growth-Enhancement

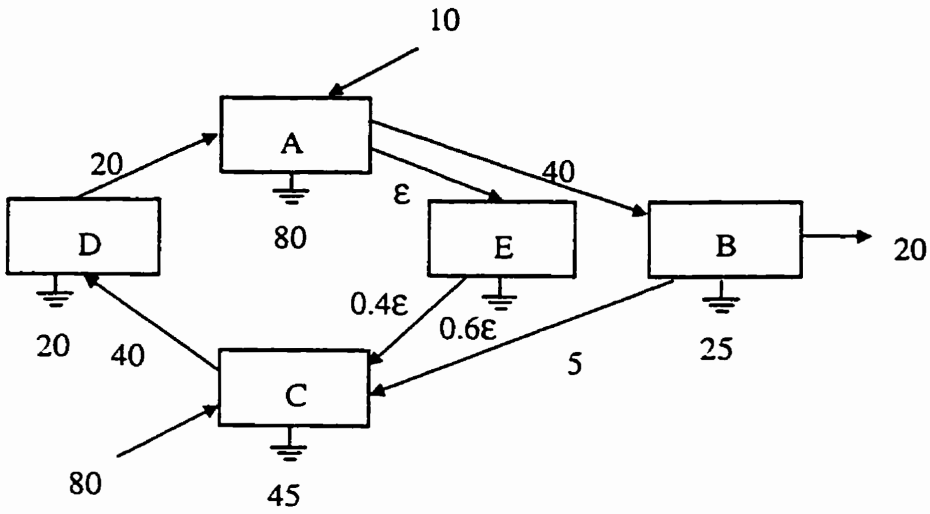
That positive feedback is growth-enhancing is virtually tautological. In the absence of overwhelming constraints, an increase in activity anywhere in the cycle serves to engender greater activity everywhere else in the loop. The activity level of the cycle is progressively elevated until it is restrained in some way from further increase.

Selection

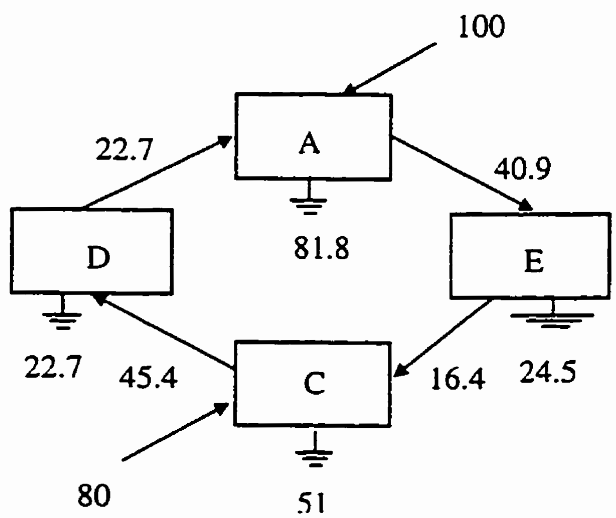
Consider what happens when a perturbation changes the activity of any component in the cycle. If the change diminishes the outputs of the given node, then the negative result will propagate around the cycle upon itself. Conversely, if the change is incremental, it will be reflected positively upon itself. By its very nature, positive feedback discriminates among the perturbations occurring in the cycle. The persistance of the characteristics of component elements are directly influenced by the feedback structure in which they occur. Feedback thus exerts a kind of "selection pressure" on the activities of the components.

Competition

Feedback may also function as a selective agency in a more robust sense. Imagine that through some mechanism, a new element enters the system giving rise to the configuration in Figure 5.6(a). The new species, E, is seen to be more efficient at conveying a small amount of flow material, ε, from A to C. The pathway through E is progressively rewarded, and, if the whole system is acting near its limits (as it eventually must), the continued growth of the pathway $A \rightarrow E \rightarrow C$ will occur at the expense of the activity at B. After a while B is displaced by E in the cycle, as shown in Figure 5.6(b).



(a)



(b)

Figure 5.6: The displacement of component B in the feedback loop by a more efficient species. (a) The new species in the network. (b) E totally displaces B. (Redrawn from Ulanowicz 1986, 58.)

It is possible to imagine all the components of the cycle being replaced in a similar manner, so that an identifiable structure may persist beyond the lifetimes of its constituents, all the while playing an active role in guiding its eventual make-up. This example also illustrates the "competition" of components for a place in the cycle.

The growth enhancing characteristics of autocatalytic cycling impels the system toward greater levels of activity, or system throughput (increase in "size"). But the flow is not being enhanced uniformly in the network. Rather, a greater portion is more narrowly channeled along those feedback pathways of higher transfer efficiencies. In the absence of mechanisms generating new components and/or pathways, the evolving network topology would appear less random, or better articulated. This progressive articulation, Ulanowicz argues, depends only on the efficiency ratios of the various flows and drives the system to more complex states of organization.

Conclusion

In this chapter I introduced the notion of a "complex systems ecology", or CSE. CSE theory is an approach to complex systems that is distinctively ecological in character, in that it was developed by ecosystem ecologists for the purpose of studying real-world ecological phenomena, such as a patterns of succession in ecosystem development. Yet it offers a general framework for studying natural complexity, and should be seen as one among several emerging schools of general complexity theory. A list of such schools might include: the "Sante Fe" school, which emphasizes computer models of complex systems phenomena that are exhibited by systems on the border between regularity and chaos (see Lewin 1992); the "Santiago" school of autopoeitic theory developed by Maturana and Varela (1984); the "homeokinetics" school of Arthur Iberall (1972); the "synergetics" school of Herman Haken (1987), the "order through fluctuations" school of Ilya Prigogine (1980), and the "infodynamics" school of Salthe (1994). A comparative study of these different approaches to complexity theory is an important future project for philosophers of science. Given the relevance of complexity theory to a unified ecological science, such a study would also be an important contribution to a general philosophy of ecology.

Chapter 6

Niche Concepts in Ecology

Introduction

In this chapter I take up the topic of the niche concept in ecology, and the role this concept might play in a complex systems approach to ecological systems. I review the classical niche concepts of Grinnell, Elton, Hutchinson and MacArthur, and the systems-theoretic niche concept of CSE theorist Bernard Patten. Patten's niche concept is embedded within his formal network theory of organism-environment relations which he calls "environ theory", and is highly compatible with the Gibsonian niche concept that will be discussed in Chapter 7. I will explore the connections between Patten's and Gibson's niche concepts in greater detail in that chapter. A synthesis of these two niche concepts is my choice for a suitable complex systems theory of the niche.

1. Grinnell's Niche Concept

The concept of an ecological "niche" has a long history in ecology dating back Grinnell's (1917) work on the California thrasher. He used the term "niche" to describe the factors that influenced where one might find the species, and he included considerations involving the food of the species, the preferences of birds for certain types of vegetation structure and other details that influenced where the species could be found. Grinnell developed the niche concept with the intention of explaining how attributes of individuals determined the manner in which they would fit into a range of environmental conditions.

The Grinnellian niche is often called the "habitat" or "place" niche, because it is thought that Grinnell focused on environmental factors rather than on attributes of the organism itself, but this conception of the Grinnellian niche is inconsistent with Grinnell's own work, which makes reference not only to the habitat uses of organisms, but also the behaviour and physiology of the organism.

2. Elton's Niche Concept

One reason why the Grinnellian niche is identified with the habit variables that determine where a species lives is through its contrast with the niche concept of Elton (1927). Elton emphasized the *function* of the species and defined the niche of the species as "its place in the biotic community, its relation to food and enemies". The ecologist, Elton suggested, should cultivate the habit of looking at animals from a point of view that revealed what an animal is *doing* within an ecological community. Is it a carnivore, herbivore or omnivore? Does it feed at the low end of the food chain or at the high end? When an ecologist says "there goes a badger", she should include in her thoughts some idea of the animal's place in the community to which it belongs, just as if she had said "there goes the vicar" (1927, 64). For these reasons, the Eltonian niche is often called the "functional" or "role" niche of an organism.

I am not convinced that the traditional ways of distinguishing the Grinnellian and Eltonian niches are useful or accurate. It may be more fruitful to consider Grinnell and Elton's niche concepts in light of their respective theoretical orientations (Griesemer 1992). Grinnell was an evolutionary biologist who was interested primarily in factors that influenced speciation, and hence was motivated to examine fine differences in environmental and life situation that might account for evolutionary divergences. Elton was a community ecologist who sought to uncover similarities in structure across communities in terms of the constraining and organizing effects of food chains and cycles.

A closer examination of Grinnell and Elton's niche concepts reveals considerable overlap: both were intended to characterize the "place" of an organism within its ecological context, and both included biotic and abiotic factors in the characterization of the niche. Grinnell's operational niche concept is somewhat more fine-grained than Elton's; Grinnell assumed that no two organisms would occupy the same niche, while Elton appealed to the notion of a "vacant niche" to explain *convergent evolution* — a phenomenon in which phylogenetically unrelated species in different ecosystems evolve to the point of being strikingly similar in physical appearance and behaviour. Convergent evolution was posited (and still is, by many) as evidence for the existence of similar functional roles for organisms in geographically separated communities. Yet the differences in Grinnell and Elton's attitudes toward the notion of vacant niches are not as great as is commonly assumed. Grinnell admitted that the niches of different species in different communities could be very *similar* — he simply didn't believe they could be *identical*, and on this point Elton likely would have agreed.

3. Hutchinson's Niche Concept

In an effort to synthesize the Grinnellian and Eltonian niche concepts, and to develop a niche concept suitable for mathematical analysis, G. E. Hutchinson formalized the niche in terms of the occupation of a hypervolume of a phase space whose dimensions represent all the "relevant" environmental factors acting on organisms (Hutchinson 1957). The "fundamental niche" represented the range of environmental factors that would permit the occupying species to persist indefinitely. The set of conditions found in the physical environment that corresponded to points of the fundamental niche was called a "biotope". In a particular location, the fundamental niche of the species can be restricted either because all the conditions under which a species might live do not occur, or because the species is excluded by competing species. This restricted set of environmental conditions, that fraction of the fundamental niche in which the species actually persists, was called the "realized niche". Hutchinson later generalized this distinction by referring to the two senses as "preinteractive" and "postinteractive" niches, respectively.

In Hutchinson's model, one could envision different species as occupying different volumes of an abstract hyperspace. According to a time-honoured principle of ecological theory, "the competitive exclusion principle", no two species will occupy the same niche, because, it is assumed, species competing for the exact same resources cannot coexist; one will always drive the other to extinction. Some disagreement over the utility of the niche concept stems from the apparent circularity of this principle. If species are observed to coexist, then by the competitive exclusion principle they must have different niches to avoid competition, whether these differences are discernible or not. If species do not coexist, then they must overlap in their niches and competition prevents, or would prevent, coexistence. The worry is that one can always tell a "competitionist" story that would explain any observed community relationship. Empirical studies of competition have highlighted just how difficult it is to actually test the hypothesis that a particular community relation is the product of competition between species.

4. MacArthur's Niche Concept

A final transformation of the niche concept came with MacArthur's (1968) operationalization of the dimensions of Hutchinson's abstract phase space in terms of variations in resource utilization, with species represented in terms of clouds of points or probability densities within the niche space. The niche of a species is thus defined by the distribution of a species with respect to one or more quantified resource-related variables (e.g., the size of seeds eaten by birds). This conception remains the basis for modern "niche theory", as it is practiced in population and community ecology. Much of competition theory in population ecology, for example, is developed in terms of competition coefficients that express the relative effects of members of one species on another over a range of environments. These coefficients include terms that can be interpreted as measures of niche breadth and overlap.

Colwell (1992) argues that the fundamental distinction between the various niche concepts described here is that the Grinnell/Elton niche is conceived as an attribute of the *environment* of a species, while the

Hutchinson/MacArthur niche concepts are conceived as attributes of a *species or population*. The "environmental niche" concept, as he calls it, asserts that a niche is a "place" within the environment that could support the life-processes of a species, that a species could *occupy*, and hence, that could conceivably be vacant as well. The "population niche" concept, on the other hand, is "at its base simply an ecological description of the phenotype of some particular population or species" (Colwell 1992, 241). It follows, then, as an analytic truth, that no two species can occupy the same niche, and the statement that two species have "similar niches" represents nothing more than a shorthand description of ecological, morphological, and behavioural similarity. Colwell suggests that the population niche concept is the more fruitful for ecological theory, though he does not reject the environmental niche concept outright — his concern is that explanations of ecological phenomena that appeal to the environment niche concept are extremely difficult to falsify.

5. Patten's System-Theoretic Niche Concept

CSE theorist Bernard Patten uses a network formalism to define the niche concept and to describe niche relationships (Patten and Auble 1981, Patten 1982). In what follows I will make reference to network concepts discussed in Chapter 5.

The key concept in Patten's network theory approach to ecosystem theory is the "environ", a conception of the environment of a focal system within a network. The environ of a system contains two types of environment, a description of how all the components of a network influence the functioning of the focal system (the "input environ"), and a description of how the focal system affects the functioning of all the other components (the "output environ").

Environs

Recall our discussion of network analysis in Chapter 5. The *input* and *output structure matrices* of a network are given by the expressions

(1) $[I - G]^{-1}$ ("input structure matrix")

and

(2) $[I - F]^{-1}$ ("output structure matrix").

(1) and (2) are also known as the Leontief and Augustinovics inverses. [G] and [F] are matrices of partial "feeding" and "host" coefficients respectively, where each coefficient represents the fraction of the total input (output) to (from) a compartment j that comes (goes) directly from (to) another compartment i. The input and output structure matrices can be used to calculate the ultimate fate of any unit of input or output to or from any component of the network. In chapter 5 we calculated the fate of one unit of input to the detritus compartment for the Cone Spring ecosystem.

Figure 6.1 and Figure 6.2 show similar fates for one unit of input and output for *every* compartment of an oyster reef ecosystem. Each unit of *input* defines a network which characterizes the *influence* of that unit on every other component in the network, and each unit of *output* defines a network which characterizes the *contribution* of every other component of the network to that output. These input and output networks are what Patten calls "environs". Each component of a network has two environs, an input environ and an output environ, and when summed across all components they reconstruct the original flow network without remainder.

The environs of a component represent the *within-system environment* of that component. If we define the "environment of an object" loosely as "that which surrounds an object and is capable of exerting a causal (or other) influence on that object", then we have a rough definition of the *input* environ of a component in a network. The *output* environ is a different kind of "environment"; it represents that part of the within-system environment which can be *acted upon by* a component. The result is a *dual* conception of environment

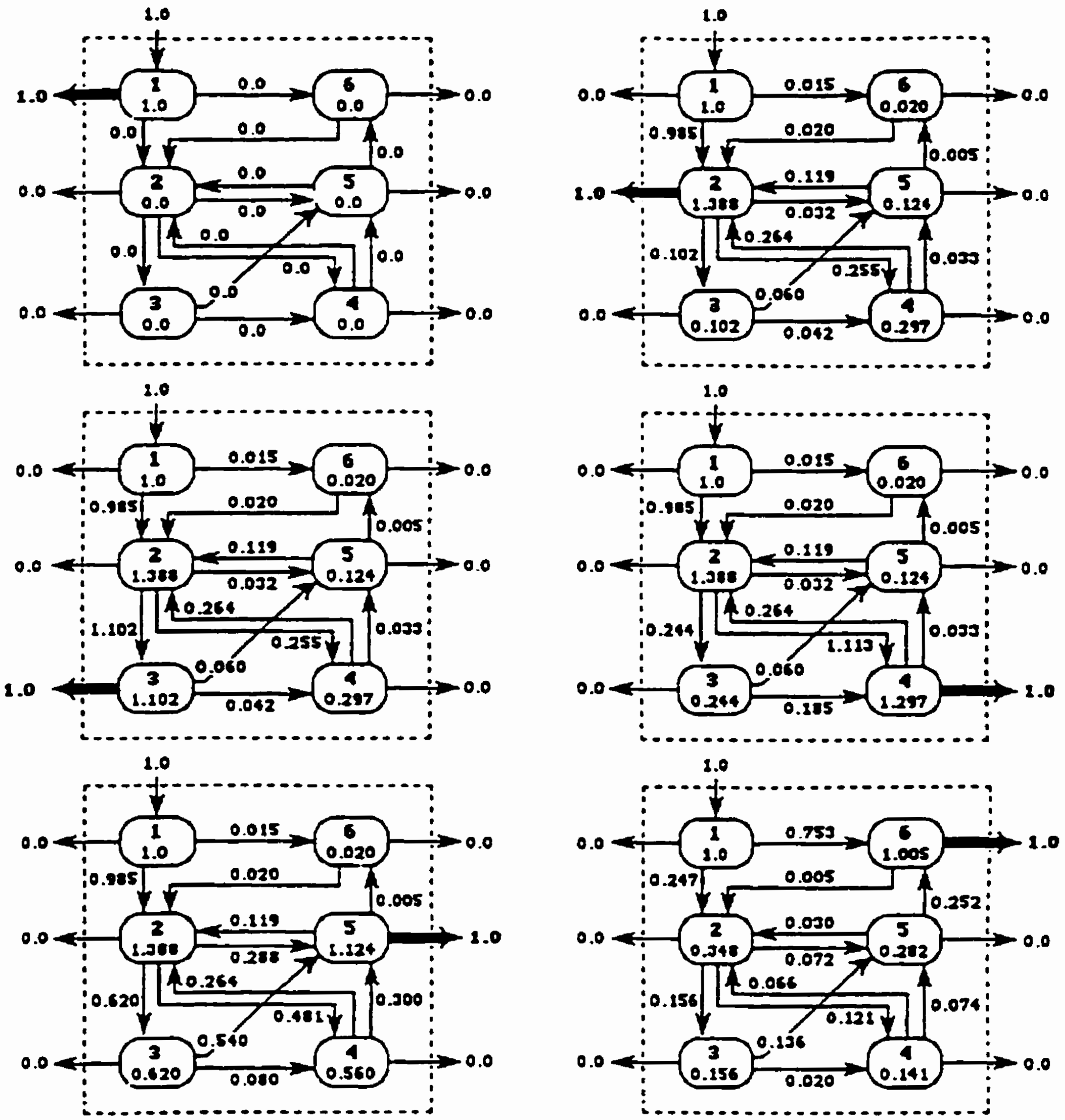


Figure 6.1: Nondimensional unit input environs associated with each compartment referenced by one unit of output (heavy arrows) of an oyster reef ecosystem. (From Patten 1992.)

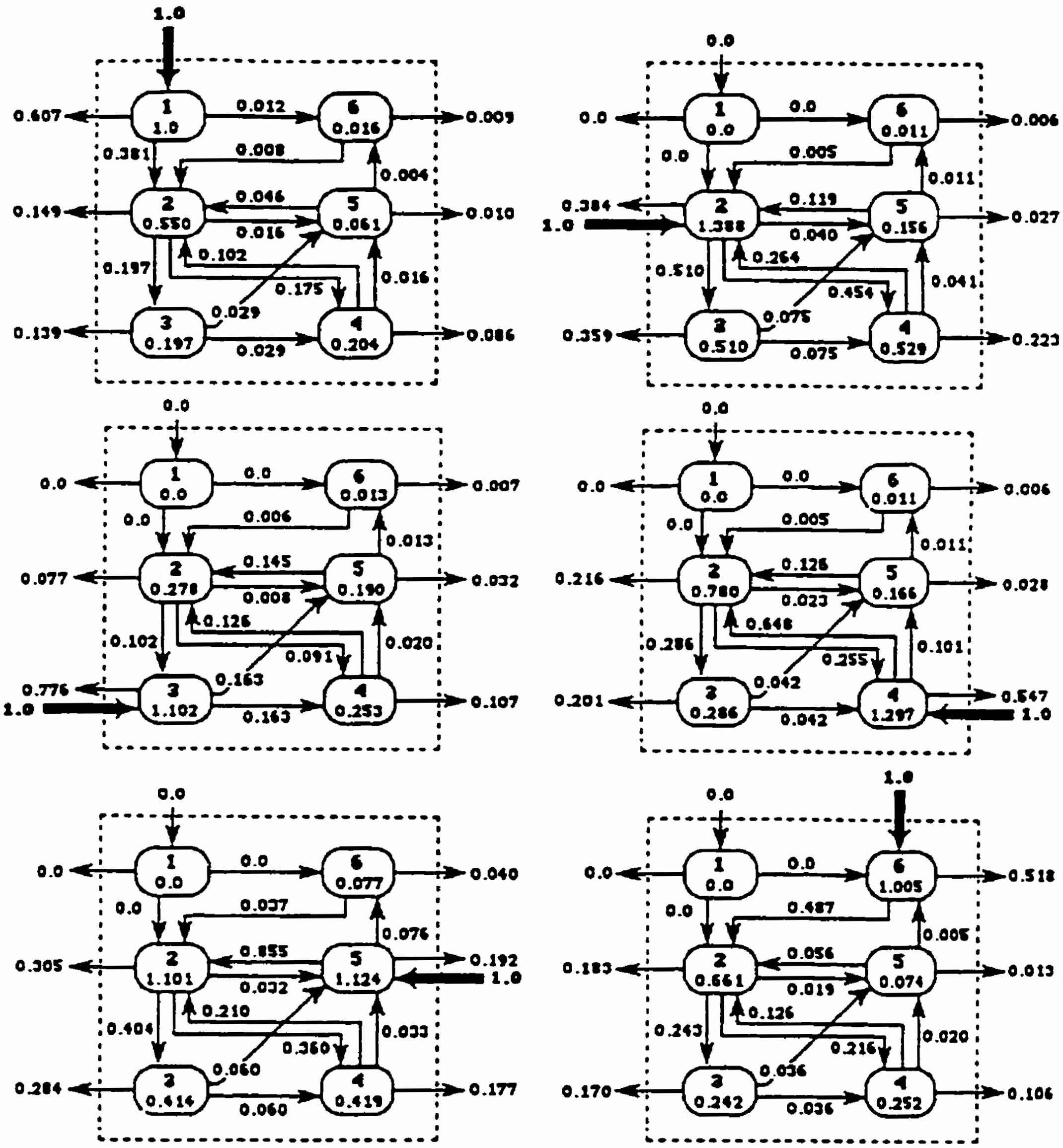


Figure 6.2: Nondimensional unit output environs associated with each compartment referenced by one unit of input (heavy arrows) of an oyster reef ecosystem. (From Patten 1992.)

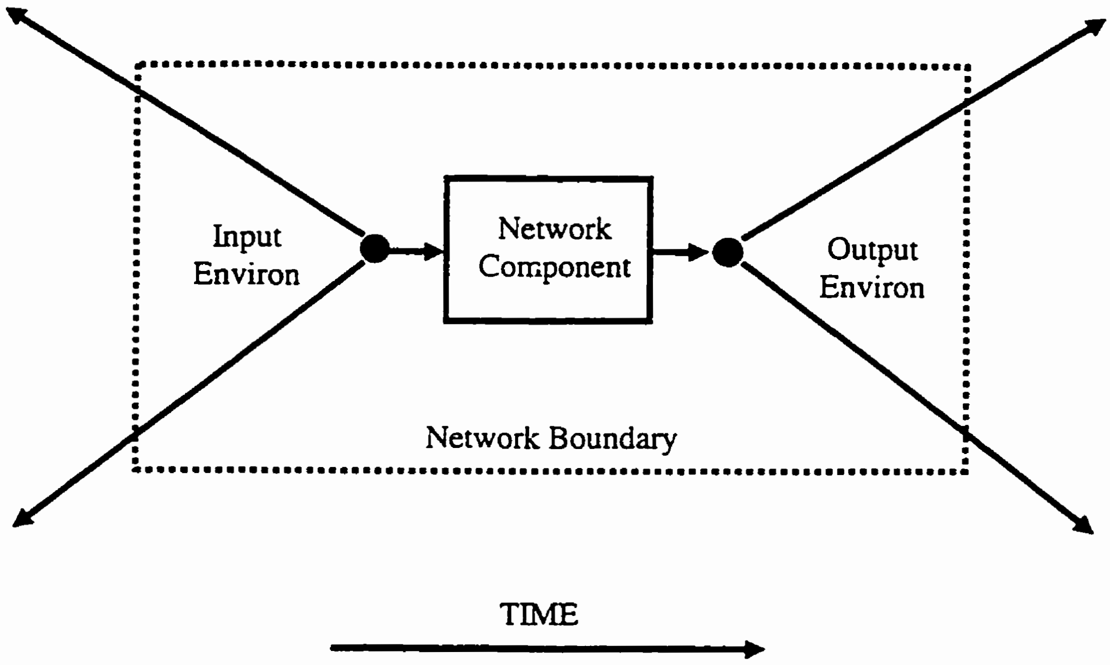
with is both spatially and *temporally* extended. Environs originate at a node in a network and trace the history of causal influence and impact of a node both backward and forward in time to the system boundary.

Schematically, the input and output environ of a component can be represented as in Figure 6.3 (a). Note that when direct or indirect cycles are present, a given component may appear in both input and output environs (Figure 6.3(b)). Figure 6.4(a) shows a simple hypothetical network with components labelled H_i through H_m, and a directed graph representation of the input and output environs of component H_i showing its direct and proximal interaction sequences. H'' is a system output, and the H' are system inputs.

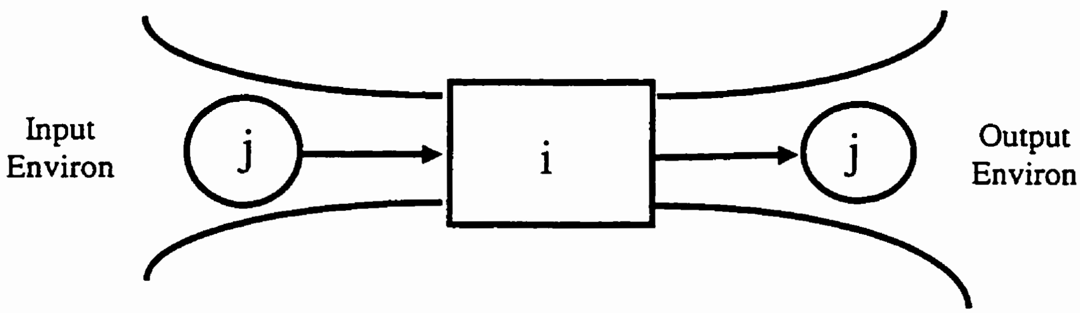
The network in 6.4(a) can be treated as a component in a larger network, and it will define its own set of input and output environs in this larger network. We see that the environ concept is scale-independent, and any system in a compositional hierarchical is amenable to environ theory analysis. Each component system in a hierarchical network has an environment which is exhaustively specified by its input and output environs. The component-environ unit is naturally described as a kind of "eco-system", a system in conjunction with the ecologically/dynamically relevant portions of its environment.

Systems Theory of the Niche

Patten and Auble (1981) argue that the niche concepts of Grinnell, Elton and Hutchinson can be understood as restrictions of the input and output environs of an organism (or species). Though they acknowledge that the common "habit"/"functional role" distinction overstates the differences between Grinnell and Elton's niche concepts, they show that *a* habitat conception and *a* functional role conception of the niche can be distinguished in their model in terms of the orientation within the ecosystem network. A list of environmental or "habitat" factors that impact and constrain the functioning of a focal system can be associated with the *input* environ of the system, while a description of the effect that a focal system has on the functioning of other components in the network,

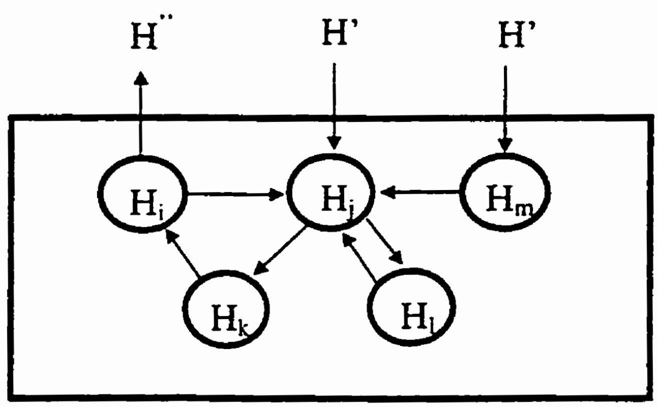


(a)

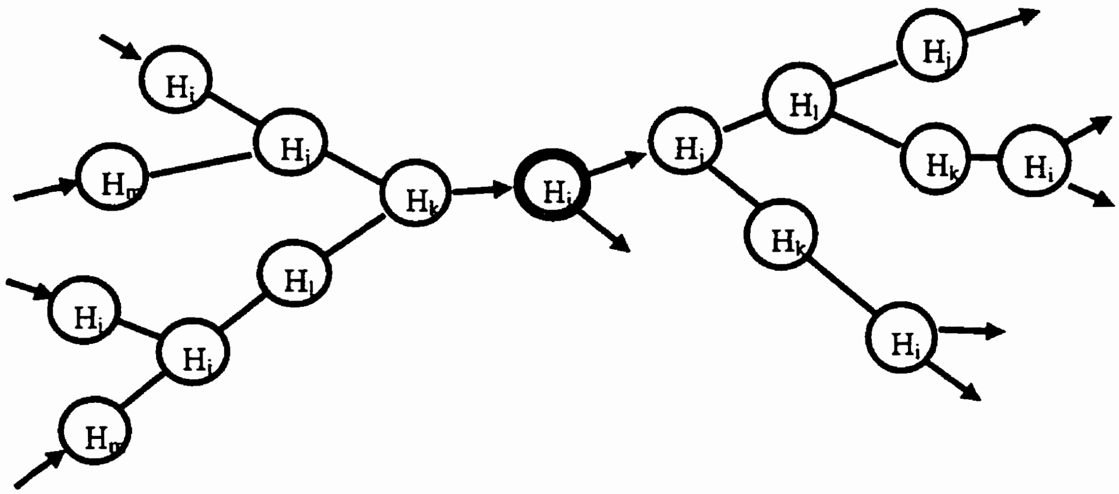


(b)

Figure 6.3: (a) Schematic representation of the input and output environ of a component. (b) A given component may appear in both input and output environs.



(a)



(b)

Figure 6.4: (a) Directed-graph representation of a hypothetical network model of an open ecosystem; (b) the input and output environs of node H_i. (Redrawn from Burns, Patten and Higashi 1991, 219.)

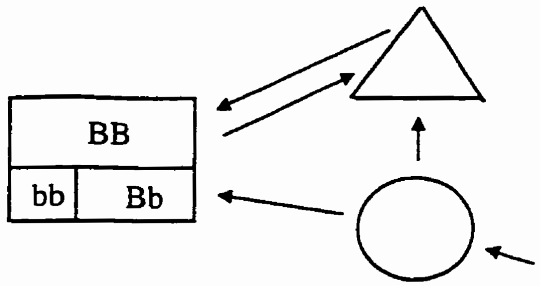
the functional role of the system within the network, can be associated with the *output* environ of the system. Patten and Auble also show how Hutchinson's "fundamental" and "realized" niche concepts can be interpreted in terms of input and output environs, but the details will not concern us here.

The environ concept is better viewed as an *extended* niche concept. The classical niche concepts restrict their attention (for the most part) to direct, proximal interactions; they do not represent the environment of an organism as a structure that extends spatially all the way out to the margins of the ecosystem within which the organism resides, or temporally to causes originating at earlier times or effects that occur at later times. The environ concept, however, is of a spatially and temporally extended structure. For Patten and Auble, the environ concept offers a superior framework for addressing the sorts of questions that the classical niche concepts were designed to address.

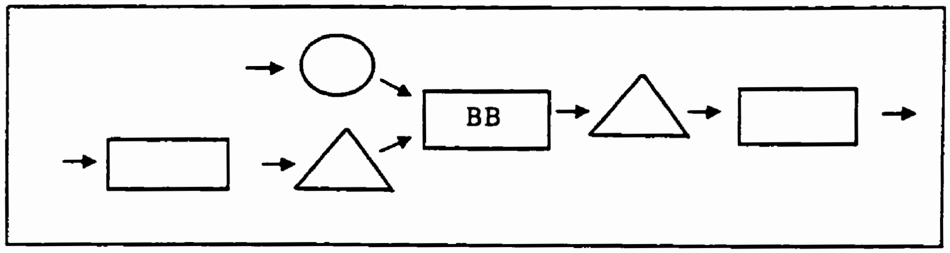
Environs and Selective Environments

One of the virtues of the environ theory approach to network analysis is that it provides a framework for "opening up the black boxes of nature". A compartment or node in a network need not be treated as a simple black box, but as a network or system in its own right (Figure 6.5). This by itself is an unremarkable feature of network representations, but as Burns, Patten and Higashi (1991) show, environ theory allows one to explore how lower-level components interact *differentially* with higher-level environments.

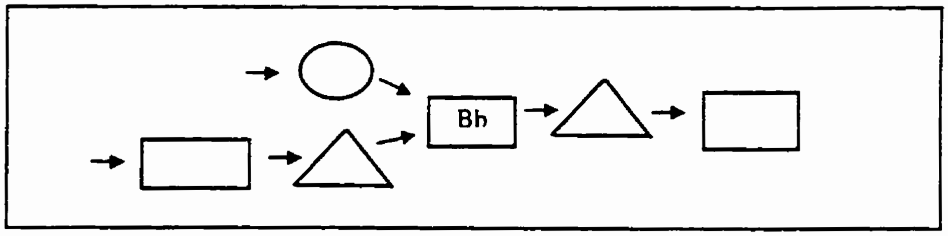
Consider the simple schematic network in Figure 6.5(a). The square, the triangle and the circle represent network components at the focal level of analysis. The square has been decomposed into three component subsystems, represented by differences in shape and labelled BB, Bb and bb. If we assume that each component interacts in the same way with the other members of the focal-level network, then the input and output environs for the three components are as shown in Figure 6.5(b), (c) and (d). In this case the input environs for each



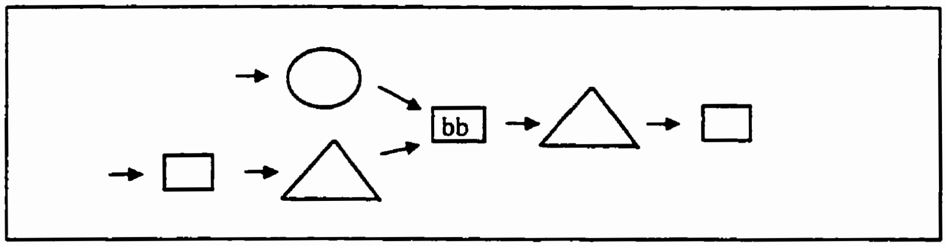
(a)



(b)



(c)



(d)

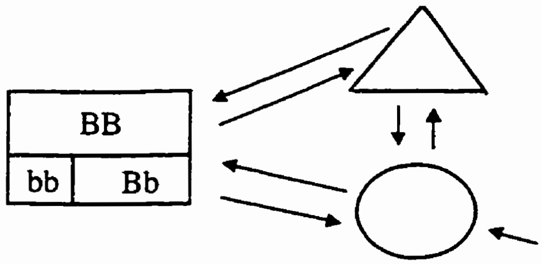
Figure 6.5: Input and output environs for component subsystems of focal system. (Redrawn from Burns, Patten and Higashi, 1991, 223.)

of the components are structurally identical, i.e. each component "sees" and "acts upon" the same environment.

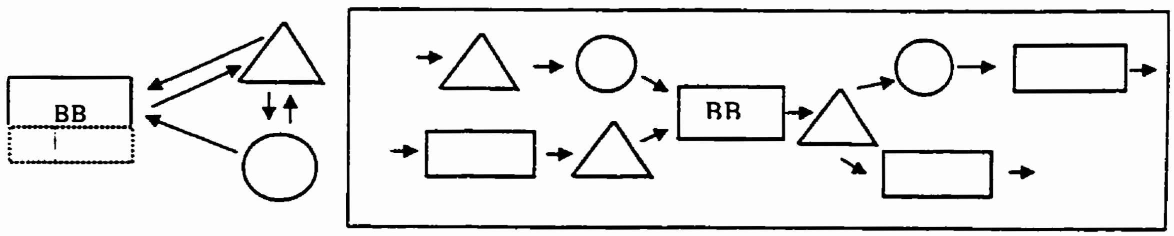
Alternatively, we can consider situations where the individual components in a subsystem interact with different members of the focal network in different ways. Figure 6.6(a) shows the same set of components with a different network structure. Figures 6.6(b), (c) and (d) show input and output environs for the components of the subsystem where each component interacts differently with the other members of the network. The result is that each component "sees" or is "is influenced by" a different subset of the focal level environment (the input environs are all different), and responds differentially to these influences (the output environs are all different).

The analysis is completely general, but an obvious application of the above ideas is to interpret inputs environs as the seat of *evolutionary selection pressures*, i.e. the environment that an evolutionary system "sees" and to which it adapts. Burns et al. argue that the single input-environs mode captures the usual understanding of Darwinian selection of varying organisms. The focal system could be a population of organisms which vary in a trait affecting their relative success in interactions with the other components of the focal network (which could be prey, predators, parasites, competitors, etc.).

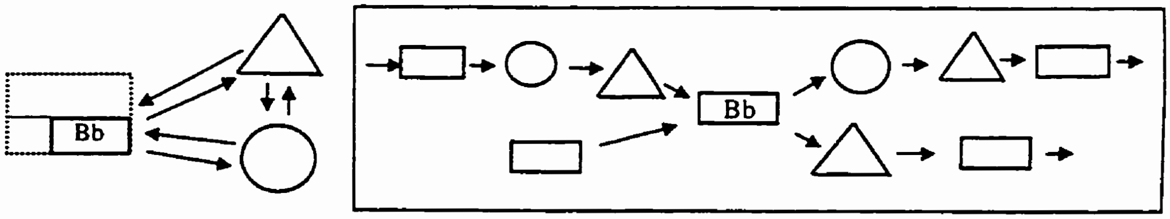
The structured input-environs model (Figure 6.6) allows for differential selection pressures on different phenotypic traits. Imagine a population of benthic (bottom-dwelling) marine invertebrates with two larval phenotypes: one negative, the other positive phototaxic (i.e. movement away and toward a light source). The two larval types might interact with and be subject to selective pressures from different species or guilds (e.g. deep-water fishes vs. deep-water plants and plant-like organisms which stay closer to the bottom). The two larval trait groups have different input environs. The differential success of one could eventually result in a single type and a single input environ for the whole population, or a mixed population could evolve and stabilize.



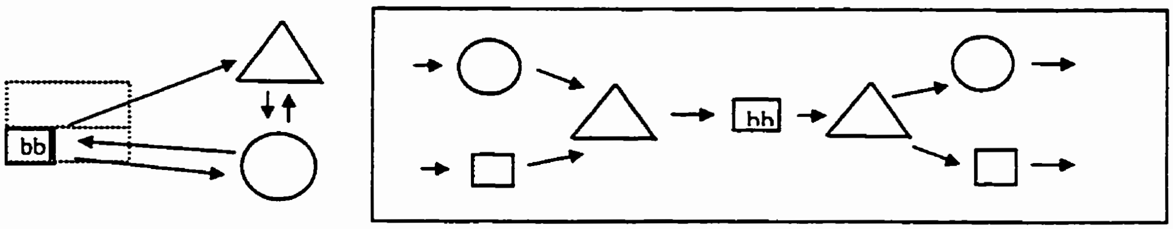
a)



b)



c)



(d)

Figure 6.6: Components of subsystem interact with different members of the network in different ways, leading to different input and output environs for each component. (Redrawn from Burns, Patten and Higashi 1991, 224.)

Different input environments may gives rise to divergence of variants at a node. At one time such models of "sympatric speciation" were popular, but these are no longer in vogue. But an analogous process might be important at levels above and below the species-population where spatial scales and stronger interactive strengths, respectively, might promote differentiation of coexistent variants (Burns et al. 1991, 225). More generally, the structured input-environ mode of selection can be observed to result in a change in network structure, unlike the single input-environ mode which can only result in a change in rate or strength of interactions.

Conclusion

The niche remains a dominant concept in ecology, though its function in theory is distinct from its function as a general organizing concept. In its common meaning, the niche is interpreted vaguely as a species' place in an orderly natural environment. In modern population and community ecology, however, the niche concept has been formalized and delimited in scope, and "niche theory" is virtually synonymous with competition theory, where niche overlap is interpreted as a direct measure of competition. The difficulty of testing competition hypotheses has led to a decline in the popularity of competition theory in recent years, and hence the niche concept has experienced a decline in use as well.

In seeking a conception of the niche that has connections to complex systems ecology, I introduced Bernard Patten's network theory of the niche. This theory embeds the classical niche concepts within Patten's "environ theory" approach to ecosystem analysis, and reveals the niche as a general systems concept that may be applied to systems existing at various spatial and temporal scales. An important feature of this conception of the niche is its dual input-output character. The input environ of a system describes the influence of the network on the system — it is what the system "sees" when it "looks out" into its environment. The output environ describes the influence of the system on the rest of the network — it is what the system "does" to the other components. The input and output environs are strictly dual in the sense that the one can be generated from the other simply by reversing the direction of the arrows of influence. In the next chapter I will show how the environ concept relates to the conceptions of niche and environment found in the writings of ecological psychologists.

Chapter 7

Ecological Psychology: Resources for a Unified Ecology

Introduction

In Chapter 4 I described ecological science as a fragmented discipline, both within traditional ecology and between ecological disciplines in fields outside of traditional ecology. I argued that a unified theoretical perspective on ecological phenomena is desirable, and suggested that progress toward a unified ecology would be served by the adoption of a complex systems perspective on ecological and evolutionary phenomena, and by the development of a theory of the niche that can be integrated within such a perspective.

Chapters 5 and 6 were devoted to discussions of complex systems approaches in ecology and the niche concept, respectively. In this chapter I continue the discussion of complexity theory and niche concepts, but within the theoretical framework of a nontraditional ecological discipline: *ecological psychology*.

The chapter is divided into two parts. Part 1 is an introduction to the basic concepts and framework of ecological psychology. I begin with the seminal contributions of James J. Gibson and the concepts of "affordance" and "ecological information". In the next two sections I consider some examples of research in ecological psychology within the Gibsonian tradition, as well as developments in "neo-Gibsonian" theory, an attempt at a synthesis of Gibsonian perceptual psychology and dynamical systems approaches to coordinated action. Neo-Gibsonian theory can be viewed as an application of a complex systems conception of the ecological niche to problems in psychology and cognitive science.

In Part 2 I discuss the application of the Gibsonian framework to problems in traditional ecology: the behaviour of individual organisms, the dynamics of populations, the structure of communities, and the growth and development of ecosystems. I also discuss the character of a "complex systems ecology" that encorporates the Gibsonian framework.

PART 1

1. Gibsonian Perceptual Psychology: Environment, Information, and Affordances

J. J. Gibson developed what came to be known as the "ecological approach to perception" in a series of papers and monographs spanning a thirty year period (Gibson 1950, 1966, 1979). The key concepts in Gibson's perceptual psychology are the notions of "environment", "information", and "affordance".

Consider two animals, a gopher and spider, situated in an open area, surrounded by a variety of objects (grass, trees, tree stumps, a small pond). There is a sense in which the gopher and the spider share a common environment — they are surrounded by the same physical and energetic "stuff" — yet in another sense, the gopher and the spider live in very different environments. For the gopher, a tree is something that obstructs its motion, that it can hide behind, but cannot climb. For the spider, the tree is a climbable thing. The gopher can burrow into the dirt and soil, but the spider (let's assume it's not a burrowing spider) cannot. The spider may be able to walk across the surface of the pond, but the gopher cannot. Different aspects of the shared environment of the gopher and the spider respectively afford different opportunities for behaviour and action. Gibson argued that the study of animal (and human) behaviour must make reference to the concept of the "ecological" environment of an animal, the environment that affords the opportunities and resources on which the life of an animal depends.

Gibson used the term "affordance" to refer to those properties of the ecological environment of an animal that support its behavioural potentialities. The pond affords walking-on for the spider (and any other water-walker), but not for the gopher; the "walk-on-ability" of the pond surface is an affordance property of the ecological environment of the spider. Similarly, a coffee cup affords grasping, has the affordance of "graspability", for some animals, but not for others. These affordance properties of the environment are relational properties, but they are not subjective; they are properties of the environment that are indexed to the behavioural and morphological traits of organisms.

For Gibson, *perception* is understood as the ability of an animal to have its behaviour be guided or regulated by *information* that specifies the relationship of the animal to its ecological environment. *That this information is itself a part of the ecological environment of the animal* is Gibson's most distinctive theoretical claim, for the dominant cognitivist view in psychology presumes that the full informative content of perceptions is not present in environmental sense data, but is a feature, rather, only of internal mental representations (e.g. Fodor 1980; Marr 1982). In this regard Gibson distances himself dramatically from both orthodox cognitive psychology and classical behaviourism, for it is his claim that the traditional distinctions between stimulus, response, and internal information processing, are theoretical constructions that have no basis in ecological reality. For Gibson, information is a *resource* that animals are able to exploit in the furtherance of their behavioural goals. Perception is essentially the "pick-up" of this information — a direct, unmediated sensitivity to properties of the ecological environment — and its use in the service of the control and regulation of action[1].

---

[1] In this chapter I am choosing to bracket philosophical arguments for and against direct realism in perception, or inferential versus non-inferential approaches to information processing in cognitive science. Apart from taking the discussion too far afield from the main line of thought of the chapter, I find that most discussions of ecological psychology by philosophers suffer from a lack of understanding of the ecological model of perception, and unfamiliarity with the experimental and theoretical work in ecological psychology that has been conducted since Gibson's last book came out in 1979. With the current rise in popularity of dynamical approaches to cognition (e.g. Port and van Gelder 1995), there is a new opportunity for philosophers to reassess the ecological approach that stresses affinities between Gibson and contemporary dynamical theories. I reserve this project for another occasion.

From the concepts of affordance and ecological information one can infer another principle theme of Gibsonian psychology: *what an animal perceives is the affordance properties of its ecological environment*. Perception, for humans and animals, is the perception *of* affordances. The objects, substances and events that make up the ecological environment of an animal are analysed in terms of their affordance properties.

For Gibson, the pressing questions for a theory of perception are i) how is the perception of affordances made possible?, and ii) how is behaviour regulated by the perception of affordances? The first question is answered, to the satisfaction of most Gibsonians, by Gibson's concept of "ecological information" (I will reserve discussion of the second question for section 7). To understand the theory of ecological information it will be helpful to introduce the notion of the "ambient optical array". Take a point in space where an observer might be located, and define a sphere of any given radius about that point. From the point of view of the hypothetical observer, the ambient optical array is a nested array of solid angles extending outward from the center of the sphere and passing through its surface. It may be helpful to think of the optical array in phenomenological terms as the array of adjacent and nested patches of varying luminosity that we observe in our visual field, but it should always be kept in mind that the optical array is a structure external to the observer, and should not be confused with its projection onto the retina of the eye.

Gibson argued that the *information* that specifies affordance properties of the ecological environment is to be identified with (for the case of visual perception) *the invariant structures of the optical array*. As the point of observation moves, the optic array changes — a flow of points of luminosity is induced over the surface of the sphere that we have arbitrarily chosen to specify the optic array. As I move forward, the patch of blue to my right moves behind me, while new points of luminosity appear from a radiating source in front of me, and disappear into a converging sink behind me (we notice this effect most strikingly when driving through heavy snow, or playing video games that recreate this flow pattern in order to create the sensation of motion). But some features of the optic array do *not* change as I move forward; they are *invariants* of the flow field. For example, rigid surfaces have a visual contour that changes as I move past them, but these changes are not arbitrary; they have an invariant property that identifies them as perspectival projections of a rigid surface in three-dimensional space. Gibson's research focused on invariants that specify fairly simple features of environmental layout, such as the size, shape, distance and relative position of objects (what is called "exterospecific information"), and invariants that specify features of the perceiving agent, such as whether the agent is stationary, rotating, or moving forward or backward ("propriospecific information"). Another class of invariants specify behavioural potentialities of an agent relative to its environmental situation, such as the graspability of an object, or the climbability of a set of stairs ("expropriospecific information"). It is important to remember, however, that these different types of invariants are merely graded differences within the category of affordances, and hence in all cases what is perceived are not properties of the environment or the agent *simpliciter*, but properties of the agent-in-relation-to-environment.

2. Examples of Research in Ecological Psychology

An understanding of the conceptual framework of ecological psychology is greatly aided by a familiarity with some examples of research in ecological psychology. These examples will also help motivate the discussion of ecological information and dynamics in section 2.4.

1) *Time-to-Contact*

Gibson envisioned an "ecological optics", a science whose subject matter is the study of invariant structures of the ambient optical array, and the affordance properties they specify. A well-known example of research in ecological optics is the study of an invariant known as the "time-to-contact parameter", or "τ" (Lee

1980). Imagine a circle drawn on a brick wall as you drive toward it; in your visual field, the circle will expand at a rate that is a function of your distance from the wall, x, and your instantaneous velocity, dx/dt (which I will write as $\dot{x}$). If nothing changes, you will hit the brick wall at the time specified by $x/\dot{x}$, or $\tau(x)$. This quantity, τ, is a measure of the inverse of the rate of dilation of an optical solid angle, and is an objective property of the ambient optic array that specifies an affordance property of the environment for any moving observer, namely, the time remaining before contact with an approaching object. If an animal is capable of detecting τ, then it can use the information provided by τ to regulate its movement.

Real-world animal locomotion involves changes in velocity, and an animal will need to regulate its movement to control its impacts with approaching objects or surfaces. For this purpose one can consider how τ changes with time, $\dot{\tau}$. It can be shown that $\dot{\tau}$ specifies several different types of collision behaviour, from decelerating controlled collision (as when, for example, a bird alights on a tree branch), to accelerating, impactful collision (as when a dolphin rams a shark in the gills), to decelerating braking (as when a car comes to a complete stop just before hitting the brick wall) (Lee et al. 1993).

Is information specifying $\dot{\tau}$ available in the optical array? David Lee and his colleagues argue that not only is such information available, but it is available in several forms. In general, any sensory variable (acoustic, for example) can yield information about $\dot{\tau}$ if that sensory variable is a power function of the distance between observer and the approaching surface (Lee et al. 1991). (This is an example of another component of the Gibsonian program, namely, the generalization of the concept of affordances and ecological information to all sensory modalities; in other words, a move from ecological *optics* to a more general ecological *physics*.)

Of course, from the fact that an affordance property is specified by an invariant of an ambient energetic field, it does not follow that animals actually use that invariant to regulate their behaviour; this needs to be established experimentally. In fact, there is evidence for the use of $\dot{\tau}$ in the regulation of the diving behaviour of gannets, a fish-eating sea-bird (the gannet begins its descent at such altitudes that it must fold back its wings prior to impact with the surface of the water in order to avoid breaking them) (Lee and Reddish 1981); in the characteristic landing behaviours of flies and pigeons (Wagner 1981; Lee et al. 1993); the mid-air "docking" behaviour of hummingbirds with birdfeeders (Lee et al. 1991); and in the control of overhand drives of top-class table-tennis players (Bootsma 1988).

2) *The Climbability of Stairs*

An example of a more complex affordance property, and one that brings out dramatically the concept of an agent-centered property of the ecological environment, is the climbability of stairs (Warren 1995). Given a set of staircases of varying rise heights and depths, human beings are able to pick out by visual inspection (even from slides) the stairs that are most comfortable for them to climb. When you put them on a stairclimbing apparatus that allows variation in riser height, the most energetically efficient riser heights for a given individual (as measured by oxygen consumption) correspond to the riser heights chosen by an individual from visual inspection. These vary as one might expect; taller people are more comfortable climbing stairs with a higher riser than shorter people. Yet clearly, what is being perceived is not an externally defined metric property of staircases, but an action-specific property of staircases that is defined in terms of intrinsic body-scaled units of the actor (in this case, leg length and riser height are correlated for optimal stair-climbing efficiency).

3) *Dynamic Touch*

Gibson focused his research on visual perception, but the theoretical program of ecological psychology may be generalized across all sensory modalities. A fascinating example of the application of ecological psychology to the haptic realm, the sensory modality of touch, has been developed by Michael Turvey and his colleagues (for a comprehensive survey, see Turvey and Carello 1995a).

Most theorizing in perception is based on vision, with acoustic perception playing a subsidiary role. Few theories of perception are constructed with touch in mind. Yet touch is arguably the oldest sensory modality; the most primitive creatures feel objects and explore surfaces with parts of their bodies. One kind of touch with which we are all familiar is "effortful" or "dynamic" touch. This is the activity we engage in when we lift, turn, carry and otherwise wield a utensil (e.g., a fork), a tool (e.g., a hammer), or any medium-sized object (e.g. a bowling ball). This type of touching is contrasted with "cutaneous touch" (the perception of an object resting on the skin) and "haptic touch" (e.g. the hands enveloping an object and sweeping thoroughly and freely over its surfaces) (Turvey and Carello 1995a, 401).

Consider the sort of perceptual theory that would be required to explain the following phenomena:

- By manipulating a wielded object one can pick up information about that object. If a rod of unknown length is grasped at one end and wielded, one can estimate with considerable accuracy, without looking, the length of the rod[2].
- If one is given an array of differently shaped objects of similar mass and constitution (pyramids, cones, spheres, cubes, cylinders, etc.) that have a short protruding handle with which to grasp them, by wielding the object (one does not touch the surface of the object itself, only the handle) one can determine with considerable accuracy, without looking, the shape of the object[3].

---

[2] In experimental trials, the subject's arm is placed through a hole in an opaque screen so that she cannot see her arm at all. Rods of unknown length are placed in her hand. With the other hand the subject adjusts the position of a visible surface to coincide with the felt location of the rod tip (Solomon and Turvey 1988).

[3] The subject is asked to wield an unknown object and asked to choose a match from among a set of visible objects (Burton, Turvey and Solomon 1990).

- One can also use an object to detect information about the environment, as with the use of a cane by people with visual impairments. With a cane, for example, one can estimate the size of an aperture (say, the distance between the bars of a jail cell) by tapping between the two sides of the aperture.
- More significantly from an ecological perspective, with a cane one can also detect properties of the environment relative to one's behavioural potentialities, such as whether a gap in the surface of support (such as a hole in the road) can a) be stepped over without breaking stride, b) would require some adjustment of ordinary walking gait, or c) can not be crossed at all. As one might expect, taller subjects will designate larger gaps as crossable than will shorter subjects (Burton 1992).
- To give a final example, while standing on a flat surface and being presented with an inclined surface just in front of you, one can make a judgment, by probing with a cane, whether the incline will support stable upright posture. As the slope of the incline is increased, there will be a critical slope at which one's judgment will go from "will support upright posture", to "will not support upright posture". Nonvisual, haptic assessment of the critical slope by an individual will tend to agree with the same assessment made by visual inspection (Fitzpatrick et al. 1994).

These examples of research in ecological psychology illustrate an important aspect of Gibson's theory of perception, namely, that perception cannot be analyzed independently of behaviour and action. In some cases the relationship between perception and action is *exploratory;* action serves to generate information about the environment. For a static observer, for example, certain relationships between objects may be ambiguous (the source of some perspective illusions, for example), but by moving around, the observer continuously samples the structure of different parts of the optic array, and generates on optical flow across her visual field that will give rise to invariants of optical structure that more accurately specify (i.e. provide information about) the true relationships between the perceiver and the observed objects.

In the case of time-to-contact, climbability, and dynamic touch, on the other hand, the relationship between perception and action is also in a strong sense *performatory*; the property that is perceived is a property required for the successful performance of an action, and sometimes is only perceivable during the performance of an action. The informational basis for this form of perception is more complex than for simple judgments of object size, distance and relative position; the specificity to particular actions and action-types makes it more difficult to conceptualize *where* the relevant perceptual invariants are located, and *how* they are used by the actor to control behaviour.

For example, what corresponds to the ambient energetic array for dynamic touch? Experimental evidence suggests that the relevant array is a tensor field defined over the principle moments of inertia of the wielded object that is mapped onto the state space defined by the muscles and tendons of the wielding arm segment. The relevant invariants that are specific to the object's length and shape are dimensionless ratios of the eigenvalues of the principle moments of inertia (Turvey and Carello 1995a).

In this case, both the affordance properties and the ambient energetic field are considerably more difficult to characterize than for a simple optical variable like time-to-contact, but the principle is the same for all cases: invariants of ambient structured energy distributions specify affordance properties of the environment.

3. The Relationship Between Perceptual Information and the Coordinative Dynamics of Movement

Gibson himself did not pursue a theory of the dynamical foundations of ecological perception, but since the early 1980s, theoretical work in ecological psychology has aimed at integrating theories of motor control and coordination with Gibson's theory of affordances and ecological information.

A schematic summary of our understanding of the ecological approach to perception and action might go as follows:

(1) An action involves the controlled release of energy by the skeletal and muscular (and other) systems of the body, giving rise to forces that result in movements of bodily parts:

$$\text{ACTION} = \Delta\ (\text{muscular force field})$$

(2) The resulting movement lawfully induces a transformation or flow within an ambient energetic array (optical, acoustic, haptic, etc.):

$$\Delta\ (\text{muscular force field}) \rightarrow \Delta\ (\text{ambient flow field})$$

(3) Invariants of the flow field lawfully specify affordance properties; these are the objects of perception:

$$\text{PERCEPTION} = \text{detection of invariants of ambient flow field}$$

(4) The detection of invariants of the ambient flow field guides subsequent action, and the cycle continues:

$$\text{PERCEPTION} \rightarrow \Delta\ (\text{muscular force field}) = \text{ACTION}$$

This pattern of circular causality is known in the literature as a "perception-action cycle".

The last step in the above summary — the postulate that the information available for perception is somehow exploited by the body in generating movement — is a basic principle of ecological psychology, but it has major implications for theories of motor coordination. Early theories of coordination posited an "executive command center" within the central nervous system that prescribed all the spatio-temporal details of a movement, and the micro-activities of the supporting neural substrate, in advance of the execution of the movement. The central nervous system then sent instructions or commands to each of the individual components of the movement, telling them what to do. But "executive command" theories of movement do not fit well with the approach we have been detailing, for the concept of ecological information is a thoroughly dynamical notion that implicates the whole body in the generation and use of perceptual information.

Fortunately, there are other models of motor coordination in the literature that are more compatible with the ecological approach to perception. Nicholai Bernstein, a Russian physiologist, was influential in identifying specific problems and general principles of human motor coordination (Bernstein 1967). He initiated a program of research that treats the problem of coordination of human and animal movements as a problem in the *reduction of degrees of freedom of the motor system*. Consider the number of degrees of freedom available in the movement of the hand and arm. The upper arm is capable of motion on three axes and the lower arm and hand are each capable of motion on two axes; a joint of the hand permits motion on either one or two axes. Approximately twenty-four muscles actuate the arm, and close to twice as many actuate the digits. At scales finer than joints and muscles there are large numbers of motoneurons, receptors, and neurons subserving muscle activation. In addition there are vast numbers of capillaries and lymphatic vessels engaged in the metabolic processes promoting muscle activation. A simple act of reaching involves the management of these many degrees of freedom at multiple length and time scales. The dimensionality of the state space required to represent a given configuration of the arm-hand system is, quite literally, astronomical. "Bernstein's Problem", as it is known in the motor science literature, is the problem of explaining in the general case how nervous systems, or nervous systems in environmental contexts, resolve this management problem; that is, how the many degrees of freedom that are in principle available to the system are in fact reduced to the relatively small number of degrees of freedom exhibited in coordinated action (Kay 1988).

One way of reducing the number of independent variables is for groups of muscles that span a number of joints to be constrained to act as a single functional unit. Such constrained muscles Bernstein called "coordinative structures". The existence of coordinative structures is testified by well-known motor phenomena, such as the difficulty of patting one's head and rubbing one's stomach at the same time. The constraint relation between the muscle groups effectively reduces the dimensionality of the state space required to specify a complete configuration of the motor system. A control hierarchy may still exist, but the "executive" is no longer responsible for all the details of lower-level control. The problem of control can now be posed as the question of how the coordinated structures are coordinated (Kay 1988).

Bernstein's ideas on coordination and Gibson's ideas on perception have been influential in the development of dynamical systems (Kelso and Schoner 1988) and self-organization models (Kugler, Kelso, and Turvey 1980; Kugler and Turvey 1988) of coordination. The underlying idea behind these approaches is to conceive the body as a complex, multi-component dynamical system, and model the dynamics of coordinative structures by analogy with the dynamics of thermodynamically open, far-from-equilibrium dissipative structures.

Conservative systems experience no energy flow, and are confined to a hypersurface of constant energy in the phase space of the system. Dissipative, or nonconservative, systems experience energy flow, and their energy hypersurfaces contract as the system evolves over time. The contraction of the energy hypersurface effects a reduction in available degrees of freedom within the system, issuing in macroscopic behaviour that is characterizable with, and controlled by, only a few parameters. The points or areas in phase space to which the system finally settles down are "attractors" of the phase space. There are only a few generic attractor-types for dissipative systems: zero-dimensional point attractors, quasi-periodic attractors of two or more dimensions, and chaotic attractors with fractal dimensions.

In short, dissipative processes effect a reduction in the complexity of the multi-dimensional systems, resulting in simpler, low-dimensional behaviours characterized by regularity and order in the collective activity of the micro-components.

Kugler et al. (1980) attempted to bring together the self-organization approach to coordination with a Gibsonian model of the information available for perception. They conceived coordinative structures as assemblages of many micro-components that are, through the imposition of an energetic/informational constraint process, assembled temporarily and flexibly so that a single micro-component may participate in many different coordinative structures on different occasions. Conversely, a single coordinative structure may require the use of different micro-components at different times. A second level of constraint is then hypothesized to assemble the specific behaviours dictated by a particular task situation.

To illustrate these points, think of the action of writing one's signature. Changes in posture, bodily orientation and surface support result in vastly different groups of muscles performing the *same action* on different occasions. One can even put a pencil in between one's toes and write a signature that has features characteristic of one's hand-written signature. Clearly there is an invariant dynamical structure that underlies the writing of a signature, and this structure is, to borrow a term from philosophers of mind, *multiply-realizable* by different sets of motor components[4]. Yet for any given act of signature-writing, a particular set of micro-components must be assembled that is informed by the biomechanical requirements of the specific task situation.

Note how behavioural modes are conceptualized within the dynamical model. A specific type of behaviour is characterized by a distinctive topological structure — the layout of attractors — in the phase space of the system. Rhythmic, periodic motions involved in walking, for example (or the many other

---

[4] Cognitivist or computational models of motor action may agree with much of this, but will argue that the brain carries a "template", "script" or "schema" for an action such as signature-writing. In general, dynamical systems approaches to cognition deny that actions are dictated by high-level commands issuing from the central nervous system. See Van Gelder and Port (1995) for a defense of the dynamical perspective.

biological functions and motions that are based on oscillatory processes), may be represented by a quasi-periodic attractor structure.

One of the predictions of any dynamical systems approach to motor coordination is that transitions from one behavioural mode to another — going from a walk to a jog to a run, for example — will exhibit phenomena characteristic of phase transitions in dynamical systems. Such phenomena have been observed in experiments on rhythmic motor coordination. If a person is asked to oscillate the two index fingers at a common frequency dictated by a metronome, there will only be two steady states observed, in-phase oscillation and out-of-phase oscillation. As the metronome frequency is gradually increased, out-of-phase coordination suddenly switches to in-phase. In-phase, however, does not switch to out-of-phase, and the out-of-phase to in-phase transition is not reversed by a reduction in frequency. The behaviour thus demonstrates the nonlinear dynamical phenomena of i) sudden, spontaneous behavioural transitions, and ii) hysteresis, as well as others, such as iii) "critical slowing down" and iv) "critical fluctuations"[5].

Indeed, experiments have shown that the same basic pattern of phase transitions is observed when two limbs are connected *optically* between two people rather than *anatomically* within a person (Schmidt, Carello and Turvey 1990). In these experiments, two seated people each oscillated a leg, with the goal of coordinating the two legs out-of-phase or in-phase as the frequency of the movement was increased. To satisfy this goal, the two people watched each other closely. As with the within-person case, the between-person case exhibited

---

[5] Some terminology: "hysteresis" — a sudden jump and its reverse do not occur at the same values of the control parameter; "critical slowing down" — the time taken by the order parameter (in this case, relative oscillation frequency) to return after a perturbation to its value before a perturbation increases as the transition point is approached; "critical fluctuations" — the variance in the order parameter becomes large as the transition point is approached. (Turvey and Carello 1995b).

a sudden behavioural transition from out-of-phase coordination to in-phase coordination, but not vice-versa; indeed, it showed all of the dynamical features of the within-person transition (with one exception: critical fluctuations were not investigated). But if the two people began their movements out-of-phase, and increased limb frequency simultaneously at the same rate without watching each other, then no transition occurred; *the phase transition depended on looking.* In this case, the coupling between the components of the coordinated system is both *informational* and *intentional* — the two people need to be watching each other, and need to intend to match rhythms, for the phenomenon to occur.

Results such as these indicate a close relationship between perceptual information and the coordinative dynamics of movement, but the precise nature of this relationship remains unclear (see Beek et al. 1994, for a survey of competing interpretations). Particularly challenging is understanding how the *intentional selection of behavioural goals* (intending to jump over that rock, intending to raise my hand, intending to match leg oscillations with another person, etc.) functions in the assembly and specification of the resulting movement dynamics[6].

---

[6] It is not surprising that this is a difficult problem, for a solution to it is, for many, tantamount to a solution to the mind-body problem for intentional agents. A fascinating theoretical research program in ecological psychology goes by the name "intentional dynamics", and is the brain-child of Robert Shaw. This program is highly formal, drawing on resources from ecological psychology, variational mechanics, control theory, dimensional analysis, and more recently, quantum mechanics (path-integral formalisms), to construct a physical theory of the dynamics of intentional, goal-directed systems. See Shaw 1987, Shaw and Kinsella-Shaw 1988, Shaw et al. 1990, and Shaw et al. 1995.

PART 2

We are now in a position to consider and assess the contribution that ecological psychology can make to traditional ecological science.

Gibson himself identified the affordance structure of the ecological environment with the traditional ecological concept of a "niche" (Gibson [1979] 1986, 128), and as shown above, recent attempts to bring together Gibsonian perceptual psychology with dynamical approaches to motor coordination suggest ways of interpreting affordances in dynamical and energetic terms. Thus, there is reason to believe that ecological psychology has conceptual resources that may contribute to the development of a unified ecological science.

My strategy is to work my way up the ecological hierarchy, starting with the behavioural ecology of individual organisms in section 3, then proceed to population and community ecology in section 4, and finally ecosystem ecology in section 5.

3. Behavioural Ecology

Ed Reed's 1996 book *Encountering the World: Toward an Ecological Psychology* is a valuable study of, among other things, the relationships between ecological psychology and behavioural ecology. Here I summarize a few of the points he makes concerning the contribution of ecological psychology to the study of animal behaviour.

Reed states as "the fundamental hypothesis of ecological psychology" that

> affordances and only the relative availability (or nonavailability) of affordances create selection pressure on the behaviour of individual organisms; hence, behaviour is regulated with respect to the affordances of the environment for a given animal. (Reed 1996, 18).

What does this hypothesis imply for behavioural ecology? The dominant tradition in behavioural ecology views individual organisms as, to quote James Brown again, "maximizing their fitness by acquiring scarce resources from the environment, using them to maintain homeostasis of the individual, and allocating them to offspring"(1995, 182). But what are the resources that organisms are said to acquire? According to Reed, standard ecological analyses of resources jump from being too fine-grained to being too coarse-grained to support such analyses. On the one hand, ecologists consider resources to be molecular, to be nutrients or energy supplies. This offers the great advantage of quantitative measurement and analysis, but at the cost of being ecologically oversimple. But from a behavioural perspective, animals don't encounter nutrients and energy in their ecological environments; rather, they encounter other animals, plants, objects, events, and places, entities that can serve as persistances underlying the regulation of behaviour.

> An animal that encounters a piece of fruit does not thereby encounter the fructose or carbohydrates contained in the fruit, even though it *ingests* them. Although frugivorous animals appear to develop a taste for combinations of sugars and carbohydrates, and maybe even for particular *kinds* of sugars and carbohydrates, this is still not quite the same as encountering those molecules as such. All terrestrial animals need oxygen, but few have encountered oxygen as such. [. . .] The ability to encounter an affordance requires a perceptual system attuned to the use of information enabling that affordance to regulate action. Interestingly, there are microorganisms that use oxygen concentrations to guide their locomotion, but this is unknown among the dominant phyla of terrestrial animals . . . .
> (Reed 1996, 18)

On the other hand, when ecologists talk about resources like "food", they often do so in a global way, one appropriate to the analysis of an evolving population, but not a behaving animal. Even when a population as a whole fails to adapt, there may be individuals who learn to use previously marginal resources; and conversely, when a population as a whole does adapt, there will be individuals who fail to learn to use the new resources (Reed 1996, 38).

Darwin's finches offer a good example of selection for morphological and behavioural traits that requires a finer grain of analysis than is allowed by treating all nuts and seeds as "food". In the case of the varying beak lengths and feeding strategies of finches observed on the scattered islands of the Galapagos, variations of size, shape, and hardness of nuts and seeds are all important factors of selection (Grant 1986).

The theory of affordances and ecological information also offers resources that may be useful for the construction of explanatory models of animal behaviour that appeal to functional hypotheses. One commonly heard criticism of behavioural ecology is that its models assume that a behaviour is adaptively optimal, and then try to figure out what the behaviour is optimized to do. The most frequently encountered functional hypothesis about foraging behaviour, for example, is that it has been selected to maximize the rate of energy intake while foraging. However, Pierce et al. remind us that

> [f]or it to be possible to test the functional hypotheses underlying optimization models of foraging behaviour, it must be possible to provide independent verification of the assumptions made about the range of strategies available to foragers and the features of the environment which are important to foragers. If these assumptions cannot be verified, confirmation of predictions must be regarded as fortuitous and devoid of explanatory power. [. . .] The features of the environment which are important to a forager cannot be determined independently of observing its behaviour. It will always be possible to identify a set of environmental characteristics with respect to which observed behaviour is consistent with a particular functional hypothesis, but this process is entirely circular. By asserting that animals perceive the environment in a particular way it would be possible to show that observed foraging behaviour was consistent with any functional hypothesis. (Pierce et al. 1987, 114)

But ecological psychology is a theory precisely of the features of the environment that are important to animals, and it offers tools for determining whether or not "animals perceive the environment in a particular way". The concept of ecological information allows experimenters to find out what information really is available in specific situations, and thus discover what variables should, in fact, be counted as informative (recall the examples of research in ecological psychology given in Part 1). Clearly, behavioural ecologists could stand to benefit from learning some of the theory and methodology of ecological psychology.

An interesting example of a study of the affordances underlying behavioural regulation is Darwin's little-known study of the adaptive behaviour of earthworms (Darwin 1881). In the following passage, Reed focuses on Darwin's studies of burrowing behaviour:

> At the exit of their burrows . . . worms will often try to "plug up" the hole leading to the surface . . . with leaves, twigs, and petioles, some of the same materials they use to line their basketlike nests. The result of this plugging is to prevent air from reaching down into the nest and, Darwin conjectured, the adaptive function here is prevent desiccation of the skin of the worms inhabiting these nests. Darwin experimented with different kinds of leaves in order to discover the ways in which the worms plugged up their burrows. In general, worms pulled leaves in by their tips. But leaves whose bases are narrower than their tips (e.g., rhododendron leaves) are pulled in by their base. Pulling leaves in by their narrow ends leads to a more efficient plug or seal on the burrow. Darwin also looked at cases in which a leaf was first grasped at a wider, disadvantageous position and found that quite often worms rotated the leaf and grasped it at a narrower place before pulling it in. After these experiments with real leaves, Darwin produced a series of artificial leaves made out of white paper and with different angles at their apices . . . . Studies with these artificial materials confirmed that worms exhibit a tendency to choose the narrowest tip to pull into a burrow and that this choice, in the vast majority of cases, was not the result of trial and error. (Reed 1996, 21-22)

Through experiment and observation, Darwin demonstrated that the manipulation of leaves by earthworms was not regulated by any simple physical properties of the leaves, such as their size or shape, but by a functional property of the leaves in relation to the habitat needs of the earthworm, namely, their utility in sealing the burrow and preventing air from coming down into their nests and drying them out. Earthworms are able to distinguish properties of leaves that make them most suitable for a particular task, and regulate their behaviour with respect to those properties. In other words, earthworm behaviour is regulated by the perception of the *affordances* of leaves[7]. This example, and others previously discussed, show that it is possible to determine what animals perceive in their environments, but one must always remember that this is an empirical issue, and cannot be known *a priori*.

4. Population and Community Ecology

In their application to ecological phenomena at the population and community level, the concepts of "affordance" and "ecological information" will make themselves felt through their association and interaction with the traditional niche concepts, and the role the niche concept plays in ecological theory. As discussed in Chapter 6, the concept of an ecological niche has a long history in ecology dating back to the work of Grinnell (1917) and Elton (1927), and has been influential in the development of theory in population and community ecology. James Gibson made very clear associations between his concept of affordances and the niche concept:

> Ecologists have the concept of a niche. A species of animal is said to utilize or occupy a certain niche in the environment. This is not quite the same as the *habitat* of the species; a niche refers more to *how* an animal lives rather than *where* it lives. I suggest that a niche is a set of affordances. (Gibson 1986, 128)

Is Gibson correct in identifying the niche concept with a set of affordances? And would doing so make any difference to the way the niche concept is used in ecology? These are difficult questions to answer because there are several different niche concepts in use within ecology, and preferences for one concept

---

[7] What is wonderful about this example is that earthworms have neither separate sensory organs (unless you count the whole epidermis as a sensory organ) nor a brain (only a cerebral ganglion), yet their behaviour shows a pattern of flexible, functionally specific regulation. Darwin himself marveled at the powers of selection and discrimination demonstrated by earthworms, and concluded that they could not be entirely instinctive; they were too variable, functionally specific and adaptable to local changes in circumstances (Reed 1996, 20-21).

over another are greatly affected by the nature of the phenomena being investigated, and the purposes of the investigation. In this section I will consider only the issue of how acceptance of a Gibsonian conception of the niche would affect the *interpretation* of the concept.

Which of the classical niche concepts comes closest to Gibson's conception of the niche in terms of the affordance structure of the ecological environment of an organism? Reed argues that Elton's niche concept best fits Gibson's conception:

> [. . .] I argue that James Gibson's . . . style of ecological psychology is an important development of Eltonian ecology. Elton . . . introduced the concept of *niche* to ecology and also clarified the meaning of *community* as distinct from *niche*. Animals inhabit certain regions of their environment — regions that are structured in ways a scientist can analyze. These are the various communities of an environment. But animals also act in and utilize their environment (sometimes going out of their communities), and it is this style of resource usage, characteristic of particular animal populations, that Elton dubbed "the niche". (Reed 1996, 39)

Reed is correct in identifying the perception and exploitation of affordances as a "style of resource usage", though the choice of Elton over Grinnell (or Hutchinson, or MacArthur) as advocating a similar conception of the niche is, I think, more difficult to justify. As discussed in Chapter 5, the idea that Grinnell was concerned only with environmental "habitat" variables, while Elton was concerned only with the "functional role" of the organism within the larger community, is not supported by their own writings. Grinnell and Elton share a very similar niche concept, though their different scientific interests led them to apply this concept in different ways. Elton employed much coarser-grained niche variables in his effort to construct a theory of how ecological communities are structured; Grinnell used much finer-grained variables in his studies of the ecology of single species. The affordance concept is applicable to individual-level and population-level styles of resource usage, and so cannot be used to distinguish between Elton and Grinnell on these grounds.

Also, the phrase "style of resource usage" is equally compatible with the Hutchinsonian/MacArthurian niche concepts (particularly the latter). I suspect that Reed avoids mention of these more modern niche concepts because over the years they have been formalized in such a way as to make it difficult to apply them in contexts outside of formal competition theory. Another possible reason is that they are often contrasted with the Grinnellian/Eltonian niche concepts through the claim that the former are "population niche" concepts, while the latter are "environmental niche" concepts (see the discussion of Chapter 6). Gibson leaned towards an environmental niche concept, as evidenced by the following:

> The natural environment offers many ways of life, and different animals have different ways of life. The niche implies a kind of animal, and the animal implies a kind of niche. Note the complementarity of the two. But note also that the environment as a whole with its unlimited possibilities existed prior to animals. [. . .] There are all kinds of nutrients in the world and all sorts of ways of getting food; all sorts of shelters or hiding places, such as holes, crevices, and caves; all sorts of materials for making shelters, nests, mounds, huts; all kinds of locomotion that the environment makes possible, such as swimming, crawling, walking, climbing, flying. These offerings have been taken advantage of; the niches have been occupied. But, for all we know, there may be many offerings of the environment that have not been taken advantage of, that is, niches not yet occupied. (Gibson 1986, 128)

According to Gibson, even if affordances are understood as relational properties of an organism-environment system, they ought still to be thought of as having an objective, independent existence. Consider some examples. The graspability of a cup is an affordance of the cup, but if there existed no creature that could in fact grasp a cup (a world of eels, perhaps), would it make sense to say the cup was no longer graspable? The inverse rate of dilation of an expanding optical contour is an affordance that specifies a property of a surface — time-to-contact

— for any hypothetical observer capable of detecting it, but does this property of the ambient optical array disappear if there is no actual observer at the center of the array? Most leaves afford plugging burrows for earthworms, but do leaves lose the ability to plug burrows if they are not being used in this way? The answer is "no" to all these questions.

Interestingly, ecological theorists in psychology and the social sciences are themselves divided on this issue. There are advocates of the view that affordances cannot be thought of as existing without the animal who perceives or uses them ("mutualists", as Reed calls them), and who believe that Gibson is simply wrong in believing otherwise (e.g., Noble 1981; Good and Still 1989). The proper interpretation of the ontology of affordances remains a much-debated topic in ecological psychology.

Nevertheless, the particular character of affordances — their status as resources for *behaviour* — recalls the "population niche" emphasis on attributes of the population (or species) in relation to its environment, and the interpretation of ecological opportunities in terms of availability of resources. Though Gibson defended the coherence of the notion that affordances exist independently of the actual presence of organisms, as stated previously, he maintained that the concept of an affordance is irreducibly relational:

> an affordance is neither an objective property nor a subjective property; or it is both if you like. An affordance cuts across the dichotomy of subjective-objective and helps us to understand its inadequacy. It is equally a fact of the environment and a fact of behaviour. It is both physical and psychical, yet neither. An affordance points both ways, to the environment and to the observer. (Gibson 1979, 129)

Gibson would reject any characterization of the niche as *either* a property of a population *or* of the environment. A conception of the niche as an affordance structure suggests that the so-called "population" and "environmental" niche concepts are really just two sides of the same coin; the former emphasizes the "inward-pointing" character of the niche concept, while the latter emphasizes the "outward-pointing" character[8].

Would it make any difference to modern niche theory if an affordance-based conception of the niche were adopted? Recall that in Chapter 5 it was stated that Colwell chose to reject the environmental niche concept because explanations of population and community dynamics and composition that appeal to the concept of a "vacant" niche are extremely difficult to test:

> In itself, the fact that any imaginative naturalist can describe an unlimited number of unfilled niches for which plausible organisms might exist casts serious doubt on the operational utility of the environmental niche concept in its broadest sense. (Colwell 1992, 245)

But as we have seen, ecological psychology has an array of tools for investigating the affordance structure of the environment of an organism, or a population of organisms. The point is similar to the one made above concerning optimality theories in behavioural ecology. There are certain determinate relationships that exist between organism behaviour and the affordances of the environment, and not any old assignment of affordance properties will capture these relationships. The condition that an affordance property be correlated with a behavioural potentiality of an organism (or type of organism) imposes greater constraints on the specification of niche variables than any of the original "environmental niche" concepts (this is the main virtue of the "population niche" concept, according to Colwell), but contrary to the population niche concept, it does not

---

[8] Why are ecologists so prone to collapse the niche concept onto one or another pole? I submit that, insofar as the ontology of the niche/affordance concept is irreducibly relational, it rests uncomfortably in the conceptual framework of modern biological science. Modern biology is flush with the successes of micro-scale, reductive theories and explanations (genetics, molecular biology, etc.), and unlike modern physics, has no recent tradition of "field" or "quantum" theories that might countenance an irreducibly relational or probabilistic physical ontology.

do this by denying that niche variables are (admittedly relational) properties of the environment.

Finally, another consequence of adopting a view of the niche as a set of affordances is that the concept becomes applicable to individual organisms as well as populations. One can talk about the affordances of leaves for earthworms in general, or about the affordances of a particular leaf for a particular earthworm. Of the niche concepts extant in the ecological literature, only Bernard Patten's systems-theoretic niche concept aims for the same level of generality as Gibson's niche concept. As noted in Chapter 6, I view Patten's "environ theory" as a potentially useful framework for developing a complex systems theory of the niche. I discuss the relationship between Patten's and Gibson's theories in greater detail later in the next section.

5. Ecosystem Ecology

In this section I examine the contribution that ecological psychology can make to ecosystem ecology, or more accurately, to "complex systems ecology". As argued in Chapter 5, ecosystem ecology studies the flow of matter, energy and *information* in ecological systems. Complex systems ecology studies how these three types of flow relate to one another, and how these relationships function in complex systems generally. Neo-Gibsonian perceptual theory offers a novel conception of information that, as should be clear from the discussion of Part 1, has much to offer such investigations.

First, I will describe an example of the application of the concept of a "perception-action" cycle to the behavioural ecology of insects presented in Kugler and Turvey (1987), and discuss its significance for ecosystem theories. Second, I will examine the relationship between Patten's environ theory approach to ecosystem theory and the theoretical framework of ecological psychology.

1) *An Ecological Example: Termite Nest Construction*

Kugler and Turvey (1987) give an example of a self-organizing information system — an "epistemic ecosystem" — in the ecological realm: the construction of termite nests. The termites in question are social insects that periodically construct nests that stand twenty feet in height and weigh upwards of ten tons, and which involve the participation of more than five million insects. The insects follow two simple principles:

(a) move in the direction of the strongest pheromone gradient, and

(b) deposit building materials at the strongest point of concentration.

In the earliest phase of nest building the insects' depositing behaviour is random. There are no pheromone gradients strong enough to influence the insects' behaviour. Once a few deposits have been made, however, the pheromone diffuses into the air, creating an attracting gradient leading to a region of highest concentration. As the number of insects is increased the likelihood that an insect moves into a vicinity of a recent deposit increases. As the number of recent deposits makes the site more attractive, more insects contribute deposits, which in turn makes the site more attractive, and so on. The result is the creation of a pillar of building material.

As the pheromone gradient region amplifies, long-range correlations begin to develop among the insects. A new phase of development begins when long-term correlations are distributed over two pillars, resulting in the construction of an arch. The pheromone field develops a singularity, a saddlepoint, midway between the tops of the two pillars, resulting in an increasing gradient field toward the saddlepoint. Deposits on the two pillars are biased toward the saddlepoint region, and the result is the eventual joining of the two pillars to form an arch.

Once the arch is formed, the saddlepoint disappears, and a single radiating pheromone field reemerges at the top of the arch. But other arches have been constructed in the vicinity, and the pheromone fields from those arches result in the emergence of new saddlepoints. These saddlepoints organize a gradient layout that eventuates in the construction of a solid dome. The flat dome has a homogeneous pheromone field, and termite behaviour returns to the first step in the cycle, with random deposits on the top of the dome. The cycle repeats, and the termite nest eventually grows into a large elaborate architectural structure.

The process by which the termite nest is generated is an example of a "perception-action" cycle. The termites are storehouses of energy, but this energy is released in a controlled way, and the controlling agent is a low-energy kinematic field, the pheromone field. The pheromone field gradient "tells the termites where to go", but does not "push" them; the termites have their own on-board energy source for that. This is what ecological psychologists regard as an informational coupling; the pheromone field carries information for the termites that, if detected, eventuates in the collective behaviour of nest building. In Gibsonian terms, the pheromone field is an ambient energetic array whose invariants specify affordances for the termites.

This example is an application of Gibsonian ideas to the behavioural ecology of social insects, but it is meant to illustrate a more general approach to understanding the role of information in the self-organizing processes of complex dynamical systems.

The application of these ideas to ecosystem modeling and management problems remains unexplored. As a contribution to a complex systems approach to ecology and evolutionary theory, however, it has the virtue of being at once a theory of self-organization applicable at many levels of description, and a theory of the semantic dimensions of the information-dynamics relationship. As such, it may be helpful in understanding informational processes in biological systems.

2) Ecological Psychology and Patten's Environ Theory

The main features of Patten's environ theory (and niche), as described in Chapter 5, are the following:

i) A given focal system specifies two environments, an *input* environment and an *output* environment, which are connected to each other via the network of circular causal pathways that are present in any sufficiently complex network. The input environment describes the influence of each component of the network on the focal system (what the focal system "sees" when it "looks out" into its environment), while the output environment describes the influence of the focal system on each of the components of the network (what the focal system "does" when it "acts" on its environment).

ii) Environments are defined in terms of their network relationships to focal systems. Thus as focal systems change, their environments change as well, and vice versa. System and environment form a coupled, co-developing dynamical system.

It is evident that there are close affinities between the framework of ecological psychology and Patten's dual, input-output conception of environment and the niche. Patten has explicitly acknowledged this connection on several occasions, though his interpretation of the connection requires some refinement. Consider the following:

> The niche-like concept of affordance was introduced into the study of animal vision by Gibson . . . and has been extended by ecological psychologists to mean the many properties of environment which permit organism requirements to be fulfilled (Turvey and Shaw 1978). This corresponds in ecology to the original "habitat niche" of Grinnell (1971) which, as Patten and Auble (1981, p. 916) have indicated, is an "input niche" restriction of input environs. The reciprocal of affordance, what the organism affords to its environment, Gibson called "effectivity" and Patten (1982) "effectance". Effectance has the reverse orientation from affordance, and may be taken to correspond to Elton's (1927) "role" or "function niche", which Patten and Auble (op. cit., p. 916) pointed out was an "output niche" restriction of output environs. (Patten 1991, 310)

There are a few critical points that can be made here. Patten makes reference to the term "effectivity", a concept in ecological psychology which I have not discussed in this chapter, but which is part of the formal framework of "neo-Gibsonian" ecological psychology of Turvey and Shaw. The idea is that for an action to be successfully completed, such as the grasping of a cup handle, two criteria need to be satisfied: i) the cup has to be "graspable"; it has to have the affordance property of graspability; and ii) the agent must be suitably equiped with hands or other appendages to be able to exploit the affordance property of the cup; that is, it must possess the corresponding "effectivity" property (having "graspers") which would enable the cup to be grasped. Patten is mistaken in his belief that the notion of "effectivity" is Gibson's; Gibson never used the term. It was introduced by Robert Shaw in his formal development of Gibson's concept of "animal-environment mutualism", and *he* took it from John von Neumann (von Neumann 1958).

I would agree that Patten's input and output environs are usefully compared with the affordance and effectivity structures of Shaw and Turvey's reformulation of ecological psychology, but their reformulation differs from Gibson's original formulation in several respects, and this complicates the comparison between Patten and Gibson. That there are clear analogies, however, is undeniable, which raises the interesting question of whether environ theory could be used as a theoretical framework for investigating the kinds of phenomena studied by ecological psychologists. I believe that it can, though a full defence of this claim cannot be carried out here.

Conclusion

In this chapter I have tried to make a case for the relevance and utility of ecological psychology to the problems of traditional ecological science. From the perspective of a unification project in ecological science, the main contribution of the chapter is the presentation of a novel conceptual framework for thinking about natural ecological systems, one that derives not from a research program in traditional ecology, but from a branch of psychology. The discussion illustrates the possibility of a productive cross-fertilization of ideas, theories and methods between ecological subdisciplines, and the plausibility of the claim that these different subdisciplines may be fruitfully regarded as engaged in a common scientific project.

My sympathies for the conceptual framework of ecological psychology are evident, but I do not wish to be interpreted as advancing the concepts of "affordances", "ecological information", and "perception-action cycles" as the *only* resources that may contribute to the development of a unified ecological science. They are intended as an example of a potentially productive union between ecological subdisciplines, to illustrate the broader thesis that such unions are possible.

The application of ecological psychology to issues in traditional ecological science presented in this chapter is a first attempt at a project that requires much more sustained analysis. The central ideas revolve around conceiving ecological psychology as offering both a novel conception of niche relations between organisms and their ecological environments, and a novel conception of the relationship between information and dynamics that appears to be a ubiquitous feature of the functioning of biological systems in relation to their physical environments. James Brown's desire for a thermodynamicized niche theory (Chapter 4) is partially fulfilled by the self-organizational approach to coordinative dynamics developed by ecological psychologists, and the concept of affordances introduces new and useful means of studying animal behaviour at both the individual and population levels.

The relationship between Gibsonian concepts and evolutionary theory is an issue that I have avoided addressing head-on. That the Gibsonian ecological environment of organisms is relevant to selection processes is unarguable, but precisely how to characterize the relationship is difficult, for it involves distinguishing selective processes at the level of behaviour that occur within the life-span of an organism (i.e. the ability of organisms to learn to use the affordances of their environment), from selective processes occurring over ecological and evolutionary time that are measured as changes in population gene frequencies. Working out these issues is a challenge for future research.

Discussion

This ends the series of chapters that make up Part Two of the dissertation. A reasonable question to ask at this point is how the general ecological science and philosophy discussed in Part Two (and in the last half of Chapter 3) might be relevant to the traditional problems of environmental philosophy discussed in Part One.

Let us recall the arguments of Part One. In Chapter 1 I divided environmental philosophy into three broad traditions, anthropocentric environmental ethics, nonanthropocentric environmental ethics, and radical environmental philosophy. In Chapter 2 I gave examples of the kinds of problems that occupy philosophers within these three traditions:

i)	Anthropocentric Environmental Ethics	the debate over limits to growth
ii)	Nonanthropocentric Environmental Ethics	the search for an ecologically-informed theory of intrinsic value in nature
iii)	Radical Environmental Philosophy	a theory of social change informed by an understanding of the ecological dimensions of human existence

In Chapter 3 I defined three types of philosophy of ecology, corresponding to three increasingly general studies of ecological phenomena:

i) Philosophy of ecology as philosophy of the special science of traditional ecology

ii) Philosophy of ecology as philosophy of general theories of biosystem-environment relations

iii) Philosophy of ecology as an ecological perspective on philosophical and scientific problems, including the ecological dimensions of human nature, social existence, history, economics, and so on.

The repeated triadic structure is purposeful. What I tried to show is that the traditional problems of environmental philosophy can be seen as problems for the various conceptions of philosophy of ecology.

In Part Two, I outlined a complex systems approach to ecological science that made contact with all three forms of philosophy of ecology. I gave reasons for why a unified ecological science would be beneficial for scientific and philosophical projects at all three levels, and discussed and gave examples of these benefits for traditional ecological science, theories of individual organism-environment relations, and general theories of system-environment relations. Thus, through its direct relevance to the philosophy of ecology, the discussion of Part 2 can be seen as an instance of precisely the kind of work that is relevant to the traditional problems of environmental philosophy.

More obvious connections can be made between traditional issues in environmental philosophy and the conception of organism-environment relations propounded in Part Two. The "ecological self", for instance, is a concept that has wide currency in nonanthropocentric environmental ethics, and radical environmental philosophy. Ecological psychology and the complex systems theory of the niche can be seen as offering a framework for a rigorous conception of an ecological self that legitimates intuitions that identify the self with a system of relations and identifications between organisms and their environments, and that locates the self within a broader eco-systemic perspective.

Ecological psychology may also be a useful resource for theories of value and evaluation. Here I will discuss an approach that I would wish to pursue in greater detail at a later date.

There is a tradition of moral epistemology that conceives moral judgments as analogous to perceptual judgments. This tradition goes by the name of "moral perception" or "moral vision", and is closely tied to the epistemological position known as "moral particularism"[9]. The general idea is that real-world moral judgments often have the phenomenological character of a perceptual judgment (you "see" the beating of elderly person behind a back alley as wrong), and these judgments will often require that one make fine discriminations within the moral situation, exercising the ability to perceive the subtleties that are morally salient to a particular context. Moral judgment is viewed as a skill that one can develop over time, through experience and practice. There is a role for general moral principles in this conception, but it is not one of deducing particular actions by applying these principles to a given situation. Rather, moral principles are conceived as heuristic devices that direct, inform, and give content to particular moral judgments, much as knowledge of general empirical principles informs, but does not entirely determine, the contents of judgments of perception.

This last point raises the issue of how precisely to draw the analogy between moral (or more generally, evaluative) judgments and perceptual judgments, for the character of the analogy will depend on how perception is construed. On traditional cognitivist models of perception, one might decompose the perception as follows:

sensation + perceptual judgment = perception

The perception of a red apple is accompanied by noncognitive sensory stimulation of redness and roundess, plus the cognitive perceptual judgment "that is a red apple", a judgment that goes beyond the impoverished information contained in sense data.

Many have argued that moral judgments ought to be analyzed in terms of *emotional* responses to particular situations, with a strong analogy between emotion and perception advocated. Most theories of emotion regard emotions are intentional states with content that, in certain cases, may be rationally

---

[9] See Murdoch 1970, Kekes 1988, and Blum 1994.

evaluable; our emotions, it is said, "make evaluative claims". The analogous decomposition of an emotion might look as follows:

feeling + evaluative judgment = emotion

Theories of emotion that analyse the cognitive content of emotional responses in terms of noncognitive bodily feelings plus a cognitive evaluative judgment ("that painting is ugly", "that movie was scary") are dominant in contemporary cognitive science.

The model of moral and value judgments that I am discussing here is based on an analogy between emotion and perception, and between the epistemology of fact and the epistemology of value. Just as factual judgments are based on the claims of perception, so evaluative judgments are based on the claims of emotion.

It should be clear where I am heading. An ecological approach to perception offers a very different model for understanding the nature of perceptual judgments and how these relate to the epistemology of fact. If we grant the analogy between perception and emotion and their respective roles in the epistemology of fact and value, then adopting an ecological approach to *emotion* would entail a correspondingly different model for understanding the epistemology of value.

Consider the differences between cognitivist and ecological approaches to perception. On the cognitivist model, perception is *of* the world only indirectly; the direct objects of perception are *representations* of the world. Consequently, the epistemology of fact is based on logical inferences from states of affairs described in representations, to states of affairs of the external world. On the ecological model, perception is of the world directly, and the epistemology of fact, at least with respect to properties of the environment that are encountered by organisms, is analyzed in terms of the sensitivity of the organism to the available ecological information.

An ecological approach to emotion should be a natural subject for Gibsonian theorists (Darwin himself believed that the purpose of emotions was to inform organisms of important relationships of the organism to its environment — "fear", for example, may inform the organism that its welfare may be threatened), but surprisingly, the study of emotion has been avoided by ecological psychologists. The contrast of such an approach with cognitivist approaches to emotion should be easy enough to see, however. Cognitivist models of emotion will admit that the content of an evaluative or emotional judgment ("that movie was scary") is best understood as attributing an evaluative property ("scariness") to the object of the emotion ("the movie"), but on the cognitivist model this object is an intentional construction, a representation. An epistemology of value must still make the logical inference from the state of affairs predicated of the representation to a state of the world, and there are many reasons (on this model) for resisting an interpretation that makes evaluative properties objective features of the world external to the perceiver. The analogy here is with "secondary" properties such as redness, which on traditional models are not regarded as properties of applies *per se*, but rather of the *appearances* of apples to observers suitably like ourselves. On a cognitivist model it would be hard to ground an objective epistemology of value on the evaluative properties predicated of the objects of emotional judgment.

An ecological approach to emotion, on the other hand, would analyze evaluative properties quite differently. Such an approach, if it were based on analogy with the ecological approach to perception, would regard emotion as the pick-up of information in the ecological environment specifying a particular class of affordances of the environment for the agent. Clarke (1984) is suggestive of the nature of these affordance properties:

> Feelings, like those of anger, are correlated with relational properties between the subject and the environment. In the case of anger, the correlation is normally with the subject's being harmed by something, and fear is normally correlated with a dispositional property of some aspect of the environment to harm the subject. [. . .] Evidence that emotional feelings should be understood as informational units similar to sensations is available from evolutionary biology. The function of emotional expression is to

> communicate information about oneself in relation to the environment. As Darwin argued, such communication provides an evolutionary advantage to social animals. If emotional expression communicates information to other members of one' s species, it is reasonable to expect that subject experience of an emotion functions to inform the subject of the same information. This sort of self-knowledge would obviously be an evolutionary advantage too. (Clarke 1984, 669)

On the ecological approach, the affordance properties specified by emotions are not in principle much different from those specified by perceptions. Most importantly, these properties are conceived as "ecologically real" features of the environment. A situation may be evaluated as "dangerous" for me, even when I am not at present in danger, or experiencing the feeling of fear.

One of the distinctive features of emotional responses is the involvement of the body. When one sees the elderly person being assaulted behind the back alley, or hears of a terrible injustice afflicted upon someone, one's whole body becomes involved in generating and defining the emotional response: the heart beat goes up, galvanic skin response changes, hormonal levels increase, etc. These bodily responses are partly constitutive of the affective perception, and hence of the evaluative claim that is being made of the object of emotional response. Bodily responses, "feelings", are usually conceived in cognitivist models of emotion by analogy with "sensations" in cognitivist models of perception, but the role of bodily feelings in the semantics of evaluative claims would, in a Gibsonian or neo-Gibsonian framework, be analyzed differently than it is in cognitivist models. On an ecological model, perception and emotion are thoroughly implicated in the corporeal, embodied reality of acting agents. The semantics of perceptual and emotional judgments is an emergent property of the organism-environment system, and is not "founded" on a base level of sensation or bodily feeling. Indeed, no sharp line can be drawn between descriptive and affective judgments on the ecological model, for all perception is the perception of value, or what is good or bad for an organism.

The model of moral judgment and value that emerges from an ecological perspective is one that distributes value throughout the ecological environment. Moral judgement involves makes claims that "this situation is wrong", or "I ought to do that". It is our affective responses to environmental situations that make these claims, and on an ecological model, these responses involve the perceptual awareness of certain kinds of affordance properties. The perception of fact is never divorced from the perception of value. The ontology of value, like the ontology of fact, is conceived within the model of ecological realism as an objective feature of the environmental situation, yet defined relationally with respect to the behavioural potentialities of agents.

The discussion given here is tentative and programmatic, and much work would have to be done to construct a defensible moral philosophy grounded on an ecological model of perception and emotion. However, I view this approach, or some variant of it, as closely related to certain feminist and ecofeminist approaches to moral theory that emphasize sensitivity and responsiveness to contextual relationships, and the significance of an embodied and relational conception of moral agency (e.g. Warren 1990).

Chapter 8

Certainty and Domain-Independence in the Sciences of Complexity

With this chapter we begin Part Three of the dissertation. The vision of ecological science articulated in Part Two draws heavily on notions of emergence, self-organization, and complexity. There is a growing interest in general theories of complex systems, but philosophers of science have only begun to study such theories. I believe that a better understanding of complex systems phenomena, and the theories that describe such phenomena, is important for progress in a general science and philosophy of ecology. The chapters in Part Three (8 and 9) are intended as a contribution to the philosophy of the complex systems sciences, and hence, to the philosophy of ecology.

Chapter 8 addresses certain epistemological and methodological questions concerning the knowledge of the physical world that the complex systems sciences give us. It was written semi-independently of the other chapters in this dissertation, and hence has a somewhat different style and tone than the other chapters. A version of this chapter has been accepted for publication in the journal *Studies in the History and Philosophy of Science*. I have added a concluding discussion that examines some connections between the issues discussed in this chapter and Gibsonian ecological psychology.

Introduction

> "... Hammond's project, " Malcolm said, "is another apparently simple system — animals within a zoo environment — that will eventually show unpredictable behavior".
>
> "You know this because of ..."
>
> "Theory, " Malcolm said.
>
> "But hadn't you better see the island, to see what he's actually done?"
>
> "No. That is quite unnecessary. The details don't matter. Theory tells me that the island will quickly proceed to behave in unpredictable fashion."
>
> "And you're confident of your theory."
>
> "Oh, yes," Malcolm said. "Totally confident." He sat back in the chair. "There is a problem with that island. It is an accident waiting to happen."

The selection is from Michael Crichton's best-selling novel *Jurassic Park* (1990, 76). Ian Malcolm is a chaos theorist, a member of a team of scientists assembled by developer John Hammond to evaluate the safety and stability of his new prehistoric theme park. Jeff Goldblum plays Ian Malcolm in the movie version. Malcolm's prediction concerning the instability of the island ecosystem is borne out, with deadly consequences for most of the secondary characters in the story.

In recent years there has been an explosion of interest in complexity and complex systems in a wide range of mathematical, natural and social sciences. Were Crichton to write Jurassic Park today he would probably have identified Malcolm as a "complexity theorist", a specialist in a variety of mathematical disciplines employable in the service of the scientific study of complex systems, such information theory, network theory, catastrophe theory, self-organization theory, nonlinear dynamics, etc. My interest in Crichton's novel is not with chaos or complexity theory *per se*, but with the nature of the science — "formal science" seems an appropriate description — which is practiced by those, like Ian Malcolm, who claim to have a knowledge of the world acquired not through the conventional (fallible, inductive) methods of natural science, but rather through the formal, deductive methods of the mathematical disciplines.

To illustrate, consider the contrast between Malcolm and the other scientists in the team sent to investigate Jurassic Park. The experts on prehistoric fauna and flora, Alan Grant and Ellie Sattler, are excited by the prospect of having their theoretical speculations confirmed or disconfirmed through direct observation. Are dinosaurs warm-blooded or cold-blooded, do they run like birds or like lizards, do they hunt alone or in groups? Grant and Sattler are models of the traditional natural scientist. One can almost see the classical inductive reasoning (or Bayesian conditionalization — pick your favorite theory of scientific methodology) grinding away in their heads as they observe, for the first time and with their own eyes, the subjects of their chosen science.

Malcolm, on the other hand, is not interested in the details of dinosaur physiology or behaviour. Yet he is confident that that the island ecosystem will exhibit some form of surprising, unpredictable behaviour that was not planned for, that disaster is inevitable, and all this on the basis of a formal analysis of a highly idealized (one must assume, given Malcolm's indifference to biological detail) mathematical model of the island ecosystem. As traditional science goes, a prediction that *something* unexpected is going to happen is pretty wishy-washy. But the novel grants that Malcolm is right, and that Malcolm *knew* that he was right. The island *was* somehow fated to exhibit unpredictable behaviour, and Malcolm's computer model *did* accurately represent (on a global scale at least) the dynamics of the island ecosystem. Malcolm's model captured certain *structural* features of the system which *necessitated* a certain qualitative (in this case, nonlinear or chaotic) behaviour.

Malcolm is a fictional character, but let us consider him seriously for a moment. In his own words, Malcolm is not a pure mathematician, but a "chaotician", a *scientist* who studies complex phenomena through the lens of his chosen discipline, nonlinear dynamics. But if Malcolm is a really a *scientist*, then what is Malcolm's science a science *of*? Grant and Sattler study extinct life-forms, but what does Malcolm study? Nonlinear dynamics is not a science of biological organisms, or atoms and molecules, or any restricted class of natural systems. It is, rather, a formal theory of a certain class of abstract mathematical objects or structures. The knowledge which Malcolm brings to an empirical investigation is a knowledge *of these structures*, and facts relating to and deducible from these structures. Jeff Goldblum could have been parachuted into any number of different sci-fi disaster movies with different scientific settings — as the scientist studying nonlinear dynamics of brain processes, or global climate change, or patterns in signals from outerspace — with little or no change to the *nature* of the contribution he would make to the problem at hand. He is *ex hypothesi* an expert on *complexity*, wherever it may be found. But what kind of a science is this?

Crichton's fictional portrayal of the application of a formal, complex systems science to real-world phenomena is stripped of all realistic detail, but for our purposes this is a virtue, for it presents a simple conception of the epistemology and methodology of the formal sciences which can focus discussion. The features of this conception are:

(1) the independence of the content of formal science from the details of the material constitution of the systems under study,

(2) the emphasis on formal structures and relations of necessity within these structures,

(3) the claim that such relations of necessity can be true of real-world systems, and

(4) the claim that, at least in certain cases, we can know with a kind of deductive certainty that such relations do indeed hold of particular real-world systems.

In a provocative article on the nature of formal science entitled *The Formal Sciences Discover the Philosopher's Stone*, James Franklin (1994) argues that, in fact, *the above four points form the methodological core of all formal science*. On Franklin's view, the kind of science practiced by Ian Malcolm is not only a conceptual possibility, but a model for the way *all* formal science is *actually* practiced. This is a striking claim, worthy of consideration if only to figure out what would motivate anyone to believe it.

In this paper I review and evaluate Franklin's conception of formal science. I show that Franklin's radical epistemological claim — that the formal sciences allow the discernment of facts about the empirical world which have the certainty of mathematical knowledge — is supported only by the most simplistic applications of formal science, and is not applicable to real-world examples of mathematical modeling of physical systems. Though his characterization of formal science as a science of mathematical structures may be appropriate in some cases, I argue that many of the sciences which Franklin calls "formal" make essential reference to physical principles which are contingently, not necessarily, true.

1. Science Without the Sweat?

To motivate Franklin's conception of formal science we shall borrow Ian Malcolm for a while and indulge in a little creative fiction of our own. Let us update Malcolm so that he is an expert not only in chaos theory, but in a wide range of formal sciences, from game theory to information theory to catastrophe theory[1] . And let us grant him the ability to make elaborate mathematical calculations on the spot, in his head.

---

[1] Franklin presents a rather long but not exhaustive list of disciplines which he wants to include in the category of formal science. These include post-World War II systems and engineering sciences such as operations research, control theory, cybernetics, information theory, and game theory; computer related disciplines like computational complexity theory, computer simulation and theoretical computer science; complexity sciences such as the theory of cellular automata, self-organizing systems, and nonequilibrium thermodynamics; mathematical branches of so-called non-physical science, such as mathematical economics or mathematical ecology; and several branches of theoretical physics, including statistical mechanics, fluid dynamics and nonlinear physics (Franklin 1994, 515-21). We shall discuss Franklin's criteria for identifying the formal sciences below.

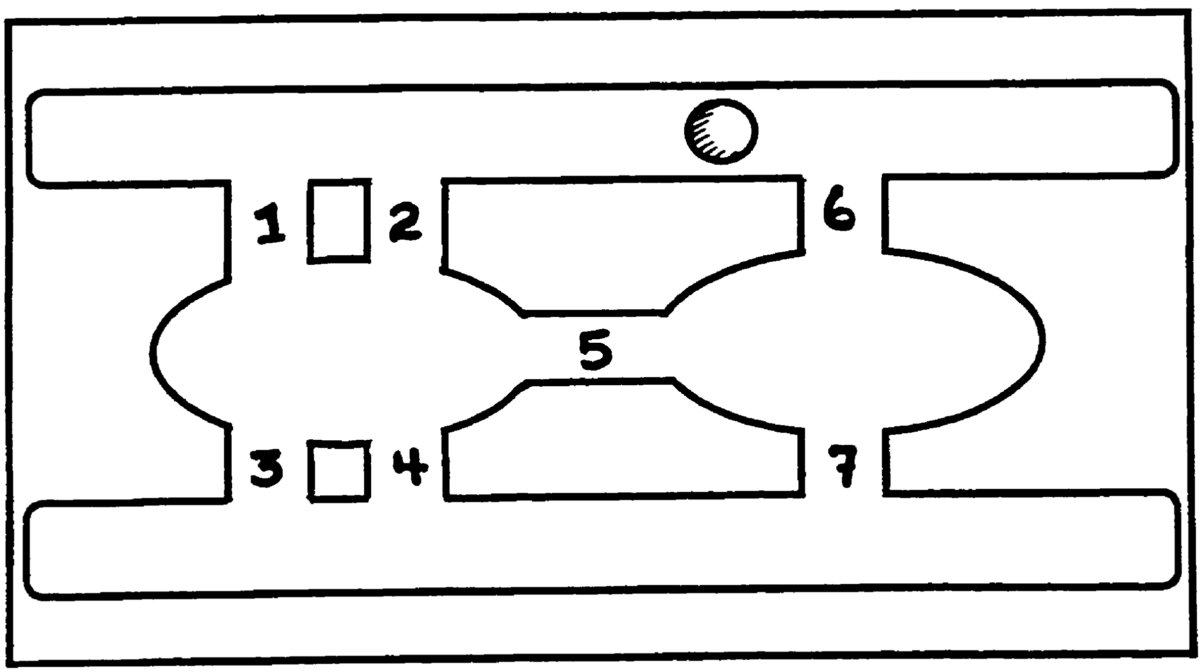
Our story begins when Ian Malcolm, Super Complexity Theorist, is invited to a potluck dinner hosted by one of his university colleagues. In attendance are a number of Natural Scientists. Much wine and cheese is consumed, and the crowd breaks up into small groups, each concerned with their own particular, vexing research problems.

On the Stairs with Jill

Malcolm walks over to the stairwell and see his host's young daughter Jill sitting at the bottom of the stairs holding a plastic toy of some kind, deep in concentration.

"What have you got there?", he asks.

"It's a puzzle that my dad gave me," Jill replies. She hands Malcolm a flat board with seven ridges in its surface, along which a small bead can roll. The ridges connect four coloured areas. "The big long ones are the mainland and the two smaller ones are islands in the middle of a river," Jill explains. "You have to find a way to roll the bead across all seven bridges without crossing any twice. I haven't figured it out yet."



Always anxious to try his hand at a brainteaser, Malcolm rolls the bead around the board, looking for a path across all seven bridges. He pauses for a moment, then his eyes widen. "Your daddy is a bit of a trickster, Jill," he says. "You can't win this game."

"Why not?", she asks.

"If you enter and leave a land area," Malcolm explains, "you use up two of the bridges. That means that, except for the two chosen for the start and finish, all the land areas have to have an *even* number of bridges leaving them, or there will necessarily be bridges left over, no matter what route is chosen. But in the puzzle all four land areas have an *odd* number of bridges leaving them, so a path going across all bridges exactly once is impossible."

Jill isn't sure she follows Malcolm's reasoning, but she grabs the puzzle and bounds up the stairs in search of her father.

In the Kitchen with Rob

Malcolm walks into the kitchen to get a bottle opener. He finds Rob, a physics student, crouched beside the sink, watching droplets of water fall from the end of the faucet. Rob says he's noticed an interesting phenomenon. He's been recording the times between water droplets and can find no discernible pattern. He suspects that the droplet times are distributed completely randomly, and is curious about the details of the physical process of drop formation which would cause such random behaviour.

Malcolm asks to see the record of droplet times, and Rob hands him a sheet of paper with a long list of numbers. Malcolm looks at the list for a while, rubs his chin, then asks Rob whether he's noticed a period-doubling pattern of droplet times at lower flow rates. Rob admits he's never paid attention to what happens at lower flow rates, and turns the faucet knob down a notch. A pattern of times emerge which repeats every eight drops. Rob turns it down a bit more, and a four-drop pattern appears. Once again, and a two-drop pattern is heard. A final turn and the droplets assume a regular, single-period beat.

"Now watch", says Malcolm, and he turns up the flow rate past the point where the random drop sequence was observed. "I'll bet you get a three-drop pattern up here", he says. A three-drop pattern is heard, and Rob is shocked.

"How did you know those droplet patterns would be there?", he asks Malcolm. "And what kind of physical process would produce such complex behaviour? It must be frightfully complicated."

"Oh no", replies Malcolm, "I'm sure it's quite simple." He explains that the droplet times for the "random" sequence weren't really random at all, but only "chaotic". "There are correlations between successive drop times, but you won't notice them unless you plot the points as a two-dimensional scatter plot, with time t_n plotted on the x-axis and time t_{n+1} plotted on the y-axis. You get a kind of parabolic ribbon structure when you plot the times this way, which indicates a quadratic relationship between successive times. Chaotic systems of this type have a characteristic period-doubling route to chaos, and intermittent windows between chaotic regions where the periods are odd-numbered."

The complex dynamics of the systems emerges from a simple, nonlinear, deterministic relationship between a small number of variables, explains Malcolm. "I suspect you could model a system like this with a simple mass-on-a-spring arrangement, letting the mass be a function of time. When a droplet fills up with water it will stretch the column of water that secures it to the faucet. When it breaks off, the column will recoil, and the time for the next droplet to form and break off will depend on the flow rate and whether the column is on the up-swing or the down-swing of the recoil when the droplet gets heavy again. That's probably where your nonlinearity enters."

Rob is thankful for Malcolm's help, and grateful that he doesn't have to bother with the detailed physics of surface tension and fluid flow to explain this curious phenomenon.

On the Patio with Linda, Harry and John

Malcolm is invited to sit down for a drink with Linda, Harry and John, who are ecologists working on forestry management problems. They tell Malcolm about their current project, which is to develop a mathematical model of spruce budworm infestations in the spruce and fir forests of eastern Canada and northeastern United States. These forests have periodically been subject to ravages by the spruce budworm caterpillar. For a number of years, a given patch of forest is seen to grow with hardly any budworm in evidence. When the trees have reached a certain level of maturity there is an explosive increase in the number of budworms and they begin to defoliate the trees. When a stand of mature trees have been sufficiently denuded over several consecutive years, they wither and die. The budworm population within the patch can no longer be sustained since its food supply becomes scarce. Their numbers decrease and then quite suddenly collapse to a low subsistence level. But the forest canopy has been opened up which allows new seedlings to grow. The forest renews itself and a new cycle begins, which eventually leads to another outbreak of insects in about thirty to seventy years.

They explain to Malcolm that they've just finished work on a mathematical model of the spruce budworm cycle which relates budworm density (B) to tree branch surface area (S) and the percentage of foliage on the trees (E). Linda hands Malcolm a sheet of paper with the following equations written on it.

$$\frac{dB}{dt} = \alpha_1 B\left[1 - \frac{(\alpha_3 + E^2)}{\alpha_2 SE^2}\right] - \frac{\alpha_4 B^2}{(\alpha_5 S^2 + B^2)}$$

$$\frac{dS}{dt} = \alpha_6 S\left[1 - \frac{\alpha_7 S}{\alpha_8 E}\right]$$

$$\frac{dE}{dt} = \alpha_9 E\left[1 - \frac{E}{\alpha_7}\right] - \frac{\alpha_{10} BE^2}{S(\alpha_3 + E^2)}$$

"All those undefined parameters, the α's, represent various intrinsic growth rates and predation rates," said John. "The model captures all the basic qualitative features of the outbreak pattern, even the sudden jumps in budworm population."

"What we want to do," said Harry, "is find a way of stabilizing *B* at a low level. We figure there has to be some combination of these parameters that will do the trick, but there are so many variables that we've just about given up hope of finding one."

"Hmm . . ." mutters Malcolm, pulling a pen out of his shirt pocket. "You want to set the right hand side to zero, right? That'll give you a big long equation in *E* and *S*, but you can eliminate *S*, and that'll give you this as the equilibrium condition, right?" he says, writing down the equation.

$$\overline{B} = \frac{-\alpha_8\alpha_9}{\alpha_7^2\alpha_{10}\left(\overline{E}^3 - \alpha_7\overline{E}^2 + \alpha_3\overline{E} + \alpha_3\alpha_7\right)}$$

"That's right!" says Linda. "But we don't know how to choose the parameters that will ensure that the equilibrium is stable."

Malcolm sighs. "You can't do it," he says. "Your stable equilibria lie on the upper and lower folds of a dual cusp catastrophe surface, and the unstable equilibria lie within the cusp region. You need to choose your α's so that the system stays out of that cusp region, but there aren't any physically realizable values for the α's that will do the trick. You can't control this system."

"Hunh?", says Linda. "Can you run that by us again?"

Malcolm explains that you can write the equation for $\overline{B}$, the steady-state condition for budworm density, as a monic with no quadratic term by introducing a new variable

$$y = \overline{B} - \frac{\alpha_2\alpha_8\overline{E}^3}{3\alpha_7\left(\alpha_3 + \overline{E}^2\right)}.$$

After a bit of manipulation, you can show that *y* satisfies the cubic equation

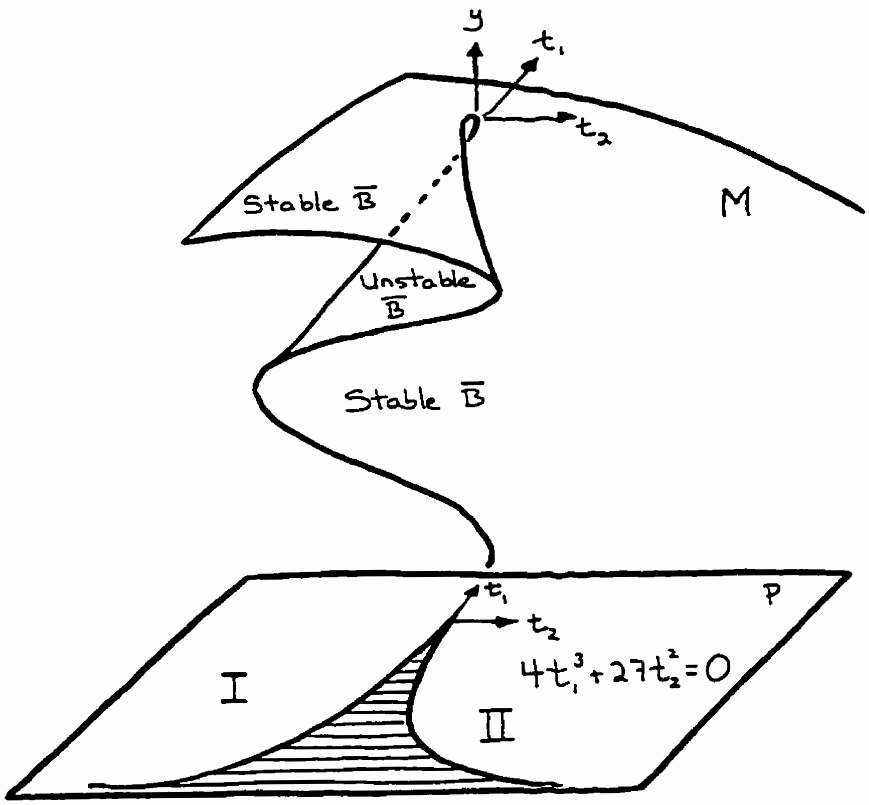
$$-\left(y^3 + t_1 y + t_2\right) = 0,$$

which a catastrophe theorist will recognize as the equilibrium equation for the standard form of the *cusp catastrophe*. The parameters t_1 and t_2 are given in terms of the original system parameters as

$$t_1 = \frac{-\alpha_8 \overline{E}^2}{\alpha_7^2}\left[\frac{\alpha_2^2 \alpha_8 \overline{E}^4}{3\left(\alpha_3 + \overline{E}^2\right)^2} - \frac{\alpha_2 \alpha_4 \alpha_7 \overline{E}}{\alpha_1\left(\alpha_3 + \overline{E}^2\right)} - \alpha_5 \alpha_8\right]$$

$$t_2 = \frac{-\alpha_2 \alpha_8^2 \overline{E}^5}{9\alpha_7^3\left(\alpha_3 + \overline{E}^2\right)}\left[\frac{2\alpha_2^2 \alpha_8 \overline{E}^4}{3\left(\alpha_3 + \overline{E}^2\right)^2} - \frac{3\alpha_2 \alpha_4 \alpha_7 \overline{E}}{\alpha_1\left(\alpha_3 + \overline{E}^2\right)} + 6\alpha_5 \alpha_8\right].$$

Geometrically, the system can be represented as a three-dimensional system, where the behavioural variable, y, is a function of two control variables, t_1 and t_2. The cusp geometry gives you generic stability conditions for systems with two inputs and one output. Malcolm sketches a diagram showing the cusp catastrophe surface:



"What you want to do is manipulate the α's to stabilize the budworm density on the lower sheet of the manifold," Malcolm explains, "but you have to stay out of the shaded cusp region, because it's unstable. The equation for this region is simple,

$$4t_1^3 + 27t_2^2 \geq 0.$$

This is the necessary condition in order to be able to stabilize the budworm densities at a low level. But if you look carefully at the physically realizable values of the α parameters, you'll see that there is no combination which will satisfy this condition."

The ecologists are stunned. "What does this mean?" asks Harry. "Is there no way to avoid these outbreaks?"

"All it means it that no amount of "knob-twisting" with the α parameters will suffice to control the system," replies Malcolm. "That doesn't mean the system can't be controlled, just that any effective scheme will have to be based on more sophisticated methods of dynamic control."

Malcolm excuses himself from the table, wishes everyone a good evening and drives home. Along the way he notices that the timing between red, green and yellow lights at a number of traffic intersections is not quite optimal, given the joint goal of maximizing traffic throughflow and minimizing energy wasted through starting and stopping. He makes a mental note to call the city transportation authorities in the morning.

2. Franklin's Account of Formal Science

Readers may recognize one or two of the applications of formal science described above. The first is widely known as the "Königsberg Bridges Problem". The citizens of Königsberg noticed that it seemed impossible to walk across all seven bridges over the river Pregel without walking across at least one of them twice. Leonhard Euler proved their conjecture correct, using the simple reasoning described. Euler's proof is now regarded as the first study in the topology of networks. James Franklin uses this example specifically to illustrate the general features of his account of formal science.

The second example is derived from Robert Shaw's classic treatment of chaotic dynamics in a dripping faucet[2]. The catastrophe-theoretic analysis of the spruce budworm outbreak is familiar to theoretical ecologists[3], though the proof that the system cannot be stabilized by parameter "knob turning" is perhaps less familiar[4]. I introduce these examples as an aid to explicating Franklin's account of formal science and to focus later discussion.

Franklin wants us to consider the nature of the contribution that a person trained in network theory, or nonlinear dynamics, or catastrophe theory, can make to our understanding of physical phenomena. In Franklin (1994), Franklin's primary concern is with the *epistemic character* of the knowledge of physical phenomena acquired through formal means, and the *method* by which this knowledge is obtained. In this section we will consider two elements of the epistemic character of formal knowledge which Franklin identifies: i) domain-independence and ii) mathematical certainty.

The reasoning which Malcolm applies in each of the above cases is, in a strong sense, *domain-independent*. In each case the system under investigation is recognized to have a formal structure which can be captured in mathematical form. Malcolm then brings his mathematical knowledge to bear on the system and deduces certain mathematical facts which are physically interpretable, and relevant to the scientific problem at hand. But in each case the mathematical reasoning involved is quite general, in that it is not tied to the particular material or ontological constitution of the system in question. The impossibility of crossing all seven bridges without crossing any twice is a restriction on *any*

---

[2] Shaw's experiment is described in Gleick (1987). For details of the analysis see Martien, Pope and Shaw (1985) and Yepez (1989).

[3] See Ludwig, Holling and Jones (1978).

[4] See Casti (1982). The diagram is redrawn from this article.

conceivable system with the appropriate network topology. Similarly, the period-doubling route chaos is a characteristic of *any* mapping with quadratic maxima, and the cusp catastrophe is a generic stability feature of *any* two-input single-output system governed by a point attractor. One can imagine the same analyses being applied to systems of radically different ontological makeup.

Second, the insights into the physical phenomena studied in the above examples appear to have the character of mathematical or deductive *certainty*. Once Malcolm realizes that Jill's game has a certain network structure, he is able to say, with certainty, that there is no solution path. On the basis of the correlations observed in Rob's water droplet data, Malcolm knows with the utmost confidence that the pattern is not random, and that it is caused by a characteristic period-doubling sequences of bifurcations. Given the equations which describe the spruce budworm outbreak, Malcolm is able to say without hesitation that no amount of parameter-twiddling will stabilize the system. Franklin believes that the knowledge of physical systems contributed by the formal sciences can have the character and the certainty of mathematical knowledge. Consequently, this knowledge will never be rendered obsolete by new scientific discoveries. The formal sciences have, in a real sense, discovered the "philosopher's stone":

> . . . the knowledge in the formal sciences, with its proofs of network flows . . . and the like , gives every appearance of having achieved the philosopher's stone; a method of transmuting opinion about the base and contingent beings of this world into the necessary knowledge of pure reason. (1994, 513)

The formal sciences may appeal, Franklin continues, to

> the many who feel that philosophers of science have chatted on to one another sufficiently about theory change, realism, induction, sociology, and so on, while real science has been producing a huge and diverse body of knowledge to which all that is totally irrelevant. (513)

Precisely how are we to understand the claim that formal knowledge has the character of mathematical certainty? Granting that mathematical reasoning about mathematical objects has a deductive character, in order for this reasoning to carry over directly to a physical system, must we not *already* be certain that a given physical system actually instantiates the appropriate formal structure?

Franklin agrees that establishing the formal structure of a physical system is necessary for our knowledge of the physical system to take on the character of mathematical knowledge. However, he argues, in many cases this is achievable. In uncomplicated cases like the Königsberg bridge problem, the formal structure is readily apparent to our perceptual faculties; we simply *look and see* how many land masses there are and how many bridges there are, and how they are connected.

How do we know that we aren't mistaken in our perceptions? Never, says Franklin, if knowledge requires "absolute" certainty — there is always the chance that we're hallucinating, or that one of the bridges is a hologram projected by an alien space-ship, or an evil demon is messing with my head. But this kind of uncertainty attends *all* perceptual knowledge. Rather, our knowledge of the network structure of the bridges has "practical" certainty, the certainty we have with respect to ordinary perceptual judgments made under ordinary viewing conditions, such as the judgment that my coffee cup is empty, or that my computer is sitting on top of my desk rather than beneath it. The assumption of "practical certainty" is required even for traditionally acquired mathematical knowledge, since the certainty obtained by following a proof of a theorem presupposes that one hasn't misread a step or been deceived at some stage in the proof.

Franklin makes much of the role of the computer in the methodology of the formal sciences. It is also possible, Franklin reminds us, to solve the Königsberg bridges problem without any mathematical ingenuity at all, by simply checking by computer whether all the possible paths which do not go over any bridge twice (there are less than a thousand of them) go over all bridges once. The result is exactly the same, and demonstrates the same impossibility with the same necessity as the earlier reasoning. Notice also that though we may not be able to "survey", through direct observation, the network structure of more complicated cases, we *can* survey the simple cases, *and we can survey the correctness of the steps in the computer algorithm which performs the calculation for the complex cases.* The computer is able to extend the practical certainty acquired through direct perception of simple cases to more complex cases because the computer program is itself a formal system which transforms inputs into outputs through a chain of necessary entailments.

At this point it becomes clear why Franklin chooses to call nonlinear dynamics or network theory a *science*, rather than a branch of applied mathematics. Franklin believes that physical systems can instantiate mathematical structures of various kinds, and that mathematical structures are proper objects of sensory experience. In this he sides with philosophers of mathematics of the *structuralist* school such as Michael Resnick (1981), who regard mathematics as a science of "structures" or "patterns", and who agree that the objects of mathematics should not be interpreted in a Platonist sense, but should be reinterpreted as things available through ordinary sense perception (Franklin 1994, 523). Formal science is *science* because it makes possible a kind of knowledge of physical systems which, like the knowledge acquired in natural science, is grounded in perception.

On the other hand, the epistemic character of formal science is *different* from that of natural science because the exclusive use of mathematical reasoning "removes, through proof, the further source of uncertainty found in the physical and social sciences, arising from the uncertainty of inductive reasoning and of theorizing" (528).

3. Reality Check

The methodology of the formal sciences is summarized by Franklin as follows:

> [1] There are connections between the parts of the system being studied, which can be reasoned about in purely logical [or mathematical] terms.
>
> [2] The complexity is, in small cases, surveyable. That is, one can have practical certainty by direct observation of the local structure. Any uncertainty is limited to the mere theoretical uncertainty one has about even the best sense knowledge.
>
> [3] Hence the necessity [of the reasoning among the connections] translates into practical certainty.
>
> [4] Computer checking can extend the practical certainty to much larger cases. (Franklin 1994, 529)

It is unfortunate that Franklin gives no examples of applications of formal science apart from the Königsberg bridge example[5]. A proposal which purports to draw a principled distinction between the category of "natural" science and the category of "formal" science, and which claims to give a characterization of the methodology of *all* formal sciences, requires at least *some* demonstration that it applies to more than the single, simple case chosen to illustrate it.

In the absence of examples provided by Franklin, let us consider the two additional examples introduced above, and see whether they fit Franklin's model. In the first example Malcolm uses chaos theory to discern a number of interesting features of the dynamics of a dripping faucet. The drip times are analyzed and correlations are observed which, when plotted in the appropriate phase space, reveal an inverted parabolic structure (which would, upon closer

---

[5] He does discuss one other example from computer science, concerning attempts to write proofs that a program is error-free ("program verification"), but his discussion of this example focuses on the question of whether mathematical properties are genuinely predicable of physical systems at all, a view which I have granted for the sake of argument.

analysis, reveal a fractal geometry). From this structure Malcolm is able to infer that the dynamics of the system is describable by the period-doubling route to chaos. He then makes a couple of predictions concerning the drip patterns that will be heard at flow rates above and below the chaotic region, which are confirmed. He later offers a hypothesis concerning the mechanism which might give rise to the observed dynamics.

Now, are there "connections between the parts of the system being studied which can be reasoned about in purely logical terms"? Yes, if we start the process of inference from the observed data and follow the steps leading to the period-doubling pattern. But in reality you need fairly precise time measurements in order to discern the correlation structure which actually governs the system dynamics. In our fictitious example we imagine Rob with a stop-watch making measurements, but one needs a laboratory setup with accurate measuring instruments to record data which actually reveal the underlying attractor structure[6]. But this point does not significantly conflict with Franklin's account if one grants that there is *some* way of acquiring data which will resolve the attractor structure. If the attractor has the characteristic inverted hump structure, then the inference to a period-doubling route to chaos is automatic.

Once Malcolm is secure in his knowledge that there is an underlying period-doubling dynamics present in the dripping faucet system, can he be as secure in his prediction that one will actually hear a periodic pattern of droplets at lower flow rates? In a realistic experiment laboratory equipment may be required to isolate the system from external influences and regulate the flow rate with sufficient precision in order to observe predicted patterns of behaviour.

---

[6] I performed this experiment in an undergraduate physics lab, and for my particular setup, period-doubling was observed at about 9 drops/sec and chaos set in around 13·drops/sec. It took a laser and a microcomputer to record the time intervals with sufficient accuracy to observe the ribbon-like structure of the underlying chaotic attractor.

Thus, Malcolm could not be secure in his prediction regarding the actual behaviour of the dripping faucet system. In our hypothetical example he just got lucky.

Nor can Malcolm be certain about his proposed mechanism for generating the nonlinearities in the system. The interaction between the spring-like dynamics of the water column and the increasing mass of the droplet is one plausible mechanism (it has the right "stretch and fold" character of all chaotic systems[7]), but it is not the only conceivable one. At best, Malcolm could be certain that *some* kind of stretch-and-fold dynamics is operating somewhere in the system. Such knowledge can be an enormous aid in mathematical modeling, and a simple mass-on-a-spring model may capture the dynamics quite well. But it in no way *guarantees* that one has isolated *the* causal mechanism which is responsible for the dynamics in this particular case.

Let us consider now the spruce budworm example. Linda, Harry and John had already developed a mathematical model for a forest patch. Malcolm was able to perform a number of formal operations on this model, reducing it to a form which allowed it to be analyzed in terms of catastrophe theory. Once the abstract form of the model was given, the impossibility of keeping the budworm density on the lower sheet of the cusp and out of the unstable cusp region followed deductively. This is clearly important information for anyone committed to the adequacy and completeness of the initial model, but it should be obvious that the construction of such models in ecology, economics, or any other area where fundamental laws are rare or non-existent (and even reliable empirical generalizations are hard to come by), is as much an art as it is a science. Simplifying assumptions and idealizations are essential to the construction of

---

[7] That is, there is a mechanism which tries to increase the value of a variable without bound, ensuring that neighbouring points in state space diverge exponentially, and another which maps the variable back onto a fixed interval in its state space, resulting in chaotic motion within the interval.

such models, and even when a good balance is achieved between empirical adequacy and analytic or computational tractability, most modelers are aware that they are dealing with mathematical cartoons of real-world phenomena, not the phenomena themselves. Malcolm's claim that the budworm outbreaks can't be controlled is entirely contingent on the acceptance of a highly idealized model of the phenomena.

As Casti (1982) states, what is really interesting about the catastrophe analysis of the model is that it showed that the number of physically meaningful parameters in a problem may be very different from the number of *mathematical* parameters needed to address the question of interest. In our example we had 10 physically important parameters (the α's) given as part of the original problem statement; however, upon carrying out the elementary analysis of the equilibrium equation for B, it turned out that the real question of interest regarding the possibility of regulating the budworm density by parametric variation came down to the interrelationship between the two mathematical parameters t_1 and t_2. Each of these parameters is a complicated algebraic combination of all ten of the physical parameters. It is very unlikely that any amount of guesswork would find that this combination of the α parameters — *and no other* — is the relevant combination for addressing the question of budworm outbreaks. The empirical significance of the catastrophe analysis is not that it rules out the possibility of managing budworm outbreaks, but that it gives us insight into *what does and doesn't count* in the analysis of the system in question.

This example illustrates a general problem with Franklin's account of methodology in the formal sciences. On Franklin's account, for knowledge of a formal structure to count as knowledge *of a physical system*, one must establish that the physical system instantiates the formal structure. But in the majority of realistic modeling situations, the models involved are simplified abstractions of the real system, and strict isomorphism between the model and the physical system is impossible to establish. Insofar as Franklin's account *requires* that such an isomorphism obtain, it rules out of consideration all but the most simple and contrived models, such as the network model for the Königsberg bridges problem.

But as a consequence of this strict requirement of isomorphism, Franklin's account makes it difficult to appreciate the diverse ways that real applications of formal science *can* contribute to our understanding of a physical problem. In both the chaos theory and catastrophe theory examples, the complex dynamics of a dripping faucet and a forest patch were found to depend on only a few parameters, effectively reducing a complex multi-dimensional system to a simple, low-dimensional system with the same qualitative dynamics as the original. Such analyses can yield significant insight into the behaviour of the original system, but they do not depend on the establishment of the structural identity of a real system and a formal system.

This is not to say that reducing the dimensionality of a problem, or constructing formal analogies which mimic the dynamics of a natural system, is the only way that formal science can contribute to our understanding of a physical system. It is to say, rather, that there are *many* ways that formal methods and formal models are used in science, and many (if not most) of them do not require that the formal model be structurally identical to a natural system.

4. A More Charitable Interpretation

At this point we should pause and consider whether we have interpreted Franklin correctly, for it seems too obvious a fact that the formal sciences do not always operate with physical systems which are known to instantiate a formal structure. Does his account of formal knowledge really require such a close relationship between model and the world? The emphasis which he places on "practical certainty" would seem to indicate that he does require it, but there is evidence in his article which supports a more charitable and plausible interpretation.

Franklin addresses the model-reality gap problem in the last section of his paper where he considers the role of experimentation in the formal sciences:

> Real certainty for armchair work — surely this is too rosy a picture of the formal sciences? If it were right, it ought to be possible to issue real-world predictions by computer, without needing to do any experiments. Anyone who has worked in applied mathematics knows it is rarely like this. It is well known that fitting a realistic mathematical model to actual data is in general difficult. Sometimes, as in meteorology and macroeconomics, it is virtually impossible. . . . Everyone agrees that formal work can proceed with the usual necessity of mathematics, provided one keeps within the model. The important point is that there is wide variability in the certainty in deciding whether the real world has the structure described by the model. The model-reality gap may be wide or narrow. (532)

Franklin even admits that his examples are tailored to fit his methodological model:

> The examples above were chosen near the opposite extreme, even, so it was argued, to the extent that there was no gap [between model and reality] at all. What structure a system of bridges or a computer program has is open to perceptual inspection, with the practical certainty that attends unimpeded sense perception. So all the hard work is in the mathematics, and the results are directly applicable, again with practical certainty. (533)

But if the "real certainty" characteristic of formal knowledge is applicable only to a very small class of systems, then why advertise it as a general feature of all formal science? Some insight into this question may be gained by considering several comments that Franklin makes regarding the formal status of various branches of theoretical physics. These comments suggest a different interpretation of the essential character of formal science.

> In retrospect, certain aspects of theoretical physics have a character recognizably like the formal sciences. Statistical mechanics, going back to Maxwell and Boltzmann, looks at how macroscopic properties of gases, like pressure and temperature, arise as global averages of the movements of the individual particles. The emphasis is not on details about the properties of the particles themselves, but on the transition from local to global properties.

> The same is true of fluid dynamics, especially in the very difficult study of turbulent fluids. The organization of the fluid flow into eddies and smoke rings is plainly not to be explained by examining the individual atoms more closely. Non-linear physics treats more generally the ways in which complicated global structures can arise from simple local interactions. (521)

Franklin is contrasting theoretical speculation concerning the natures of the component parts or hypothetical constituents of a system, with the explanation of system properties and behaviours which arise as collective phenomena or as mathematical consequences of underlying dynamics. The move from microscopic to macroscopic properties in statistical mechanics proceeds in a purely formal way, and can be applied to a diverse range of systems as long as properties of systems at the microlevel relate to properties at the macrolevel in the appropriate way. Similarly, certain phenomena, such as the transition from laminar to turbulent flow in fluid dynamics, are generic properties of a certain class of nonlinear dynamical systems, and do not depend on the detailed structure of the microconstituents.

While the *existence* of these formal properties is contingent on the existence of system components of a certain kind, the *relationships between* formal properties remain a matter of necessity:

> Whether the kinetic theory of gases is true is a contingent fact, not easily established. But it is in fact true, and the way temperature arises from the random motion of gas particles is a matter of necessity. Though it is harder than in the case of the bridges to determine if things have the properties, there is real necessity in the *connections* of the properties. Being provable, it is a stronger necessity than nomic or Kripkean necessities. (533)

In light of these comments, I offer the following reconstruction of Franklin's account of formal science:

(1) Natural systems possess formal, mathematical properties, which are deductive consequences of the natures and arrangements of the hypothetical constituents of the system.

(2) Because these formal, mathematical properties are provable, they can be known with deductive certainty *on the assumption that the hypothetical constituents of the system exist and have the natures presumed in* (1).

(3) For certain systems we *can* have practical certainty that the relevant constituents exist and possess the properties as given in (1). This practical certainty is grounded in the fact that *when* structural relationships *are* instantiated in physical systems, they *may* be directly accessible to perception.

(4) For many systems we cannot be certain that the assumptions necessary for the deduction of formal properties obtain, either because the system is too complex or because the assumptions are of a theoretical nature, inaccessible to the senses via direct observation. In such cases one does *not* have practical certainty about the formal properties of the system.

(5) The distinctive nature of the formal sciences is this: *they tell us what the formal, domain-independent properties of a system are or would be, given certain assumptions about the natures and arrangements of the hypothetical constituents of the system.*

(1) makes an ontological claim about the reality of mathematical properties, which Franklin defends on pp. 523-526. (2) and (3) together assert an epistemological claim about the kind of knowledge that these properties make possible; this is the main focus of Franklin's paper. (4) simply admits what we all know to be the case, and which Franklin acknowledges on p. 533. The only claim that is applicable to the formal sciences *as a whole* is (5), and this, I contend, is what Franklin intends as the essential feature of formal science which distinguishes it from natural science; it is what is meant by saying that formal science, like any branch of mathematics, is a science of "relations", "pattern" or "structure" (1994, 525).

Franklin's article gives the impression that he regards the epistemological claim — that the formal sciences offer "practical certainty" about real-world systems — as the central feature which distinguishes formal from natural science, but one must conclude that he simply misrepresents his position, or is not clear on his position himself. Regardless, the summary given above is the most charitable and, I believe, the most defensible formulation of Franklin's views.

5. A World Full of Structures

Franklin's account of formal science raises some interesting questions concerning the nature of formal constraints and their operation in the world. Consider once again the Königsberg bridges problem. The citizens of Königsberg could not find a path across all the bridges which did not cross one bridge twice. Why not? What prevented them from finding such a path? The natural answer is that the network structure of the bridges imposed a *formal constraint* which all paths through the network were required to satisfy. And this same network structure was responsible for Jill's frustration with the game that her father had given her. This kind of structural constraint is not *universal* in scope, for it applies only to systems with a given network topology, but it *is* strictly domain-independent, applicable to any conceivable *type* (physical, biological, artificial, social) of system.

Franklin adopts a structuralist philosophy of mathematics, a view which regards mathematical structures as real, genuine properties of physical systems. On a structuralist account, the network topology of the Königsberg bridges is a real property of *that* physical system. As one contemplates the many different kinds of formal structure that are conceivably instantiated in the world, this view naturally leads to an expansion and diversification of the formal "ontology" of the world. The world appears densely populated with formal structures which constrain phenomena in a myriad of different ways. Beads are constrained to follow certain paths and not others in children's games. Dripping water is constrained to burst into chaotic rhythms at the turn of a faucet knob. Spruce budworm populations are constrained to explode and shrink in rapid, discontinuous jumps.

When presented in this light, a *science* of formal constraints doesn't seem so odd. Processes and events in the world are governed by physical laws of various kinds, but they are also governed by purely structural, formal constraints which operate at all spatial and temporal scales. Understanding how these formal constraints operate in the world is a legitimate scientific pursuit, and it may well have a distinctive character from the traditional natural and social sciences. Ian Malcolm may be a fictional character, but the traits which mark and distinguish him from his fellow natural scientists — a focus on mathematical theories and computer models; relative indifference to the details of the material constitution and causal mechanisms at work in specific natural systems; a degree of certainty about the possibility or impossibility of the occurrence of certain phenomena that is rarely observed in traditional, empirically-oriented natural science — are not fictions, but inherent characteristics of a science which specializes in formal structure.

6. Principle Theories and Formal Constraint

All this talk of structural constraints on events or processes may bring to mind the distinction introduced by Einstein between "principle" theories and "constructive" theories. Constructive theories postulate "hypothetical constituents" which are used to "build up a picture of more complex phenomena out of the materials of a relatively simple formal scheme" (Einstein 1919, 228). The Kinetic Theory of Gases, for instance, conceived a gas as composed of hypothetical constituents called "atoms" or "molecules", which were modeled as elastic spheres or point centers of force, colliding with one another and with the sides of the container which contained the gas. The aim of a constructive theory is to reduce a wide class of diverse systems to component systems of a particular kind.

"Principle" theories, on the other hand, have potentially universal application. Principle theories specify principles or laws which impose structural constraints on the interactions or processes described by lower-level constructive theories. Einstein's favorite example of a principle theory is Classical Thermodynamics, where all physical processes are stipulated to satisfy conservative (First Law) and dissipative (Second Law) constraints. Einstein regarded Newtonian Mechanics and the Special and General theories of Relativity as principle theories as well.

The constraints which principle theories impose are often described as formal or mathematical constraints on the structure of spatial and temporal events (Bub 1974, 142). Thus, Newtonian mechanics imposes the inhomogeneous Galilean group as the symmetry group of free motions; Einstein's principle of relativity asserts that the symmetry group of free motion is the Poincaré group (with an associated modification in the space-time structure), and so forth.

Given the previous discussion of formal science as a science of mathematical structure, it is tempting to say that formal sciences do on the small scale what principle theories do on the large scale; i.e. specify formal structures which processes and events in the world must satisfy. The traditional principle theories, one might suggest, are distinguished simply by their near-universal scope and the fundamental character of their domains.

There is a certain appeal to this view, but one must avoid conflating constraints imposed by physical principles and constraints imposed by purely mathematical or logical principles. Physical principles are contingently true, and contraints imposed by these principles have the status of contingent truths, not necessary truths. Consider the derivation of the Ideal Gas Law, $PV = nRT$, within the Kinetic Theory of Gases. The Kinetic Theory asserts that a gas is really composed of tiny molecules which move rapidly about, bouncing off each other and the walls of their container. By itself the molecular hypothesis is insufficient to derive any phenomenological macroscopic laws. Only after the motions of the molecules are constrained by the contingently true laws of Newtonian Mechanics (a principle theory) is it possible to derive the Ideal Gas Law. So constrained, the relationship between microstates and macrostates of a gas emerges as a purely formal relationship, with macrostates appearing as time averages of microstates.

Furthermore, constraints imposed by principle theories manifest themselves in the interaction laws of constructive theories, which in turn specify the kinds of forceful interactions (mechanical, gravitational, electromagnetic, etc.) which are observed in the world. The Law of Action-Reaction, for example, is a constraint on forceful interactions (or perhaps, a constraint on what sorts of interactions are to count as true forces). Mathematical constraints typically do not manifest themselves as forceful interactions or as constraints on forceful interactions The little bead in Jill's Königsberg bridges game was not *forcefully* prevented from following a path which crossed all the bridges without crossing any one twice.

This distinction between formal and physical constraints is important, for it requires us to distinguish two different kinds of domain-independence. A formal theory in the strict mathematical sense will be domain-independent because the theory only makes claims about the formal properties of a mathematical or logical structure. The theorems of such theories, such as network topology or graph theory, are literally not *about* physical systems at all. A physical theory may be domain-independent in a different sense. Principle theories, for example, state physical principles and general laws which are postulated to apply to all physical processes, interactions or systems, without reference to specific causal mechanisms at the "ground" level. Domain-independence results from the fact that a large, potentially universal class of phenomena are constrained by the principles of the theory. In this case the theory has a physical domain, but the domain is so large that it cuts across conventionally defined scientific domains.

Franklin doesn't acknowledge these two different kinds of domain-independence in his account of formal science, but he should, because some of the sciences which he wishes to call "formal" are really physical theories whose domain-independence is of the latter variety. Consider the following two "domain-independent" claims:

> (A) There is no path through a graph with an odd number of nodes which does not cross at least one node twice.

> (B) The ratio of the magnitude of indirect to direct flows in a network increases with increasing (a) system size (number of components), (b) system connectivity (density of interactions), (c) compartment storage (flow impedance), (d) feedback and nonfeedback cycling, and (e) strength of direct flows. In fact, as a network becomes larger and more complex, the contribution of the indirect flows tends to exceed the contribution of the direct flows.

(A) is a theorem of graph theory, or "network topology". It is a purely mathematical result. (B) is a theorem of *network ecology*, a subdiscipline within theoretical ecology which studies the network structure of complex ecological systems. The result given in (B) is known as the *Dominance of Indirect Effects* (Higashi and Patten 1989). It asserts that as a network grows in complexity, indirect feedback effects will come to dominate the activity of any given node in the network. But (B) is not a purely mathematical result. The statement of the result makes essential reference to "flows", "cycling", and "interactions". The network that is being described in (2) is a *physical* network of flows of material or energetic substance. In order to derive (2) one needs to assume that every transfer is subject to mass-balance, energy conservation and energy dissipation constraints, which are contingent physical constraints ("principle theory" constraints, the theory in this case being Thermodynamics). The Dominance of Indirect Effects is a physical hypothesis which, if true, is applicable to systems as diverse as computer networks, neural networks, cellular metabolism, economic systems and ecological systems. (B) is domain-independent in the physical sense described above, not in the purely formal, mathematical sense. It has a physical domain, but the domain is so broad that it cuts across traditional scientific boundaries.

Franklin's long list of "formal sciences" is a heterogeneous mixture of mathematical and physical theories which exhibit different kinds of domain-independence. The field of cellular automata may be a formal science in the strict mathematical sense, but theories of self-organization and nonequilibrium thermodynamics, such as Prigogine's theory of dissipative structures and "order through fluctuations" (Prigogine 1980), certainly are not. Even within a field one can distinguish the different kinds of domain-independence. The theory of dynamical systems originated in classical physics, and most of the classical theorems of dynamical systems theory apply to Hamiltonian systems with potentials whose derivatives can be interpreted as real physical forces. But more general and abstract dynamical systems can also be studied (cellular automata, for example), and the theorems of this field are best seen as pieces of pure and applied mathematics[8]. A proper understanding of the complex systems sciences will require a more careful analysis of how formal and physical contraints combine to produce the complex phenomena which we observe.

Conclusion

In this paper I reviewed James Franklin's approach to "formal science" as presented in his "The Formal Sciences Discover the Philosopher's Stone" (Franklin 1994). Despite appearances to the contrary, Franklin's emphasis on the "practical certainty" made possible by formal science is not the feature which he is using to distinguish formal science from natural science. Rather, Franklin is using the criterion of "domain-independence" to distinguish the formal from the natural sciences. I gave a more charitable reconstruction of Franklin's conception of formal science as a science of mathematical structure, but showed that not all of the complex systems sciences are "formal" in the strict mathematical sense. Many complex systems sciences are a hybrid mix of formal and physical principles, and their domain-independence is of a different kind than is found in purely mathematical theories. More work needs to be done before we have a clear understanding of how these mathematical and physical principles interact to generate explanations of physical phenomena.

---

[8] See, for example, Hirsch and Smale (1974).

Discussion

The following discussion draws out some connections between the issues addressed in Chapter 8 and Gibsonian and neo-Gibsonian ecological psychology.

According to ecological psychology, you see affordances by detecting information in the ambient energetic array that specifies behaviourally relevant properties of the environment. Most of the analyses of ecological information given in Chapter 7 identify that information with an invariant, and sometimes quite abstract, mathematical structure defined over a phase space defined over the coupled organism-environment system. Now, Franklin's structuralism seems to shares an interesting feature with the ecological approach to perception — namely, that both require the notion that mathematical properties be epistemically available to the senses of a perceiving agent. Is there any interesting connection here?

I believe there is, though my intuitions are not clear on the matter. The issue is as much a problem for the philosophy of mathematics as it is for the philosophy of science, and a solution will presuppose certain views in the philosophy of mathematics. Franklin himself, as we have seen, is a mathematical structuralist, and he views the commitment to structuralism as an essential feature of his overall account, since structuralism is one of the few philosophies of mathematics that regards mathematical structures as genuine properties of natural systems that are epistemically accessible to us through ordinary sense perception. On Franklin's account, you can *see* mathematical properties of physical objects and systems.

Now, orthodox Gibsonianism rejects the notion that perception is *of* the invariant mathematical structures that specify affordance properties, but neo-Gibsonians like Turvey believe that detection of such structures should count as perception. This raises an interesting issue for the problem of the nature of affordances and ecological information. What philosophy of mathematics is most accommodating to this requirement? There are several different forms of structuralism in the mathematical and philosophical literature. Some theorists wish to talk about mathematical structures only within the framework of some existing overarching mathematical theory, such as set theory or category theory (Mclarty 1993). Others are happy to introduce structures *sui generis*, as patterns or universals existing in their own right (Resnik 1981), while some prefer to eliminate all talk of structures apart from the systems of objects that exemplify them (so-called "eliminative structuralism") (Benaceraff 1965; Hellman 1996).

Another approach to structuralism that is virtually ignored in the philosophy of mathematics literature is particularly interesting with respect to the issues discussed here. I call this approach "naturalized structuralism", and associate it with the work of Piaget (1971), Kitcher (1983), and Hooker (1995). It is a form of empiricist constructivism that asserts that mathematical knowledge arises ultimately from rudimentary knowledge acquired by perception and exploratory activity in the developing human. In Kitcher's version, mathematics is conceived as an idealized science of operations (physically initially, but later cognitive and symbolic) that we can perform on objects in our environment. The analysis of abstract mathematical notions such as "collection", "order", and "correlation", for example, is carried out with reference to the idealized operations of "collecting", "ordering" and "correlating" of an ideal agent. This is not to suppose that there *is* such an ideal agent, but rather that

> mathematical truths are true in virtue of stipulations set down, specifying conditions on the extensions of predicates which actually are satisfied by nothing at all but are approximately satisfied by operations we perform (including physical operations). (Kitcher 1983, 110)

Hooker gives an example of how a mathematical concept might be learned, taken from Piaget's account of the development of the species of structure known as a "group":

> The child first learns to spatially displace objects in single moves [the group operation], but then learns to perform displacements serially [group composition] and to reverse the operation [group inverse], so completing the construction of the abstract

> displacement group and generalizing the concept of spatial displacement. (Hooker 1995, 268)

Naturalized structuralism simplifies foundational questions concerning the origins of mathematical knowledge by avoiding any gap between mathematics and the world from the outset, and building the account of mathematical knowledge into a general, naturalized psychology and epistemology.

Is there a connection between naturalized structuralism and the problems of ecological perception? In Kitcher's mind, at least, there is. The account of mathematics in terms of an idealized science of operations evokes, for Kitcher, the Gibsonian notion of affordances:

> I have tried to remain neutral wherever the development of my theory permitted. Nevertheless, it is true that the theory I propose can easily be recast in the favored terminology of a currently popular psychological theory, the approach of "ecological realism" which stems from the work of J. J. Gibson and his students. Some of the central ideas of ecological realism can be used to add further detail to my account of mathematical knowledge. From a different perspective, my account may be seen as resolving a problem for ecological realism, the problem of how to fit mathematical knowledge into the ecological approach. (1983, 11)

> The constructivist position I defend claims that mathematics is an idealized science of operations which we can perform on objects in our environment. Specifically, mathematics offers an idealized description of operations of collecting and ordering which we are able to perform with respect to any objects. If we say that a *universal affordance* is an affordance which any environment offers to any human, then we may state my theory as the claim that mathematics is an idealized science of particular universal affordances. In this form, the theory expresses the widespread utility of mathematics, and given the ecological realist claim that affordances are the objects of perception, it is also easy to see how mathematical knowledge is possible. (1983, 12)

The *science* of mathematics is an object of knowledge only for humans (or other creatures capable of making the appropriate conceptual generalizations), but on this account, awareness of universal mathematical properties need not be restricted to humans. Behavioural sensitivity to mathematical properties should be a phenomenon common to all perceiving agents. Hooker's discussion of the Piaget example suggests that the account of mathematical knowledge is embedded in a broader, developmental theory of organism-environment relations, and does not require that perceiving agents be competent adult humans. Through its *behaviour*, the child demonstrates that it has internalized the formal concept of spatial displacement, but this concept is not symbolically represented in its conscious thought processes. A raccoon may be just as mathematically competent as a child in this respect.

Another feature of the present account of mathematical knowledge that is congenial to our discussion is the tight connection that is postulated between perception and action. Just as information specific to behavioural potentialities is generated by action in the Gibsonian framework, so too is mathematical knowledge specific to behavioural potentialities generated by action. This connection between perception and action, or more abstractly, between information and dynamics, has frustrated the attempts of ecological theorists to formalize this relationship within a mathematical framework. But perhaps such a formalization is impossible, given the naturalistic, dynamical roots of mathematical concepts and properties postulated by this version of naturalized structuralism. If mathematics is ultimately given a naturalistic and dynamical treatment in terms of the pick-up of particular universal affordances, then the attempt to capture this pick-up in formal mathematical terms becomes curiously self-referential, conjuring the image of a dog chasing its tail. Perhaps this is the source of the difficulty that theorists have experienced in trying to formalize the concepts of ecological psychology.

These comments are intended merely as suggestive for future research; I am not defending naturalistic structuralism here. Though attracted to naturalistic approaches in epistemology generally, my intuitions on the merits of naturalized approaches to mathematical knowledge are unsettled. But that considerations of the philosophy of mathematics might be relevant to the ecological science and philosophy I have been advocating in this dissertation is itself an interesting observation, and supports my call for a general philosophy of ecology that is not shy of crossing disciplinary boundaries in the pursuit of a unified and consistent theoretical framework.

Chapter 9

Does Complex Systems Ecology Require a New, Fourth Law of Thermodynamics?

Introduction

Sven Jørgensen's (1997) *Integration of Ecosystem Theories: A Pattern,* is a survey of developments in theoretical ecosystem ecology, or, as I have used the term in the current dissertation, "complex systems ecology". In the book, Jørgensen reviews various theoretical approaches to the description and analysis of ecosystem structure and development, including the application of network theories, catastrophe theory, chaos theory and fractals. However, Jørgensen is mostly concerned with sketching the outlines of a unified theoretical framework for ecosystem theory, and hence devotes the most space to what he (and many other ecosystem theorists) considers the best candidate for such a framework, *thermodynamics.*

In the final chapter of the book, Jørgensen presents a tentative, unifying framework for organizing the different theoretical approaches described in earlier chapters. The central principle of this framework is a posited "fourth" law of thermodynamics, which he prefers to call the "ecological law of thermodynamics". He states the law as follows:

> A system that receives a through-flow of exergy (high quality energy) will have a propensity to move away from thermodynamic equilibrium, and, if more combinations of components and processes are offered to utilize the exergy flow, the system has the propensity to select the organization that gives the system as much exergy as possible. (1997, 345)

In this chapter I examine the use of thermodynamics within complex systems ecology, and discuss the meaning of, and motivation for, the above formulation of an "ecological" law of thermodynamics. My concern is not to refute the notion that such a law may be applicable to far-from-equilibrium complex systems, but rather to make what I hope are some useful recommendations for how to think about such a law. I will argue that, even if we grant that such a law is a valid and useful description of processes that govern the development of complex ecological systems, it is inappropriate to think of it as a law of *thermodynamics*.

I begin with a survey of the laws of classical equilibrium thermodynamics, then proceed to analyze Jørgensen's proposed ecological law of thermodynamics. I then return to the discussion began in Chapter 5 concerning "principle", "constructive" and "phenomenological" theories, and apply these concepts to theories in complex systems ecology. My position is that Jørgensen's law is better understood as an expression of a phenomenological law of complex systems, and that thermodynamics and network theory are the principle and constructive theories of complex systems ecology, respectively. Consequently, complex systems ecology does not need a new, fourth law of thermodynamics, not because there are no laws that govern the behaviour of complex systems, but because it is incorrect to think of complex systems phenomena as issuing directly from thermodynamic principles.

1. The Laws of Equilibrium Thermodynamics

Putting aside technical qualifications for the moment, we can state the three better-known laws of equilibrium thermodynamics as follows:

1. First Law: *The change in the internal energy of a system, defined as the difference between the heat it absorbs and the work it performs, is the same for all transformations between a given state and a final state.*

This is an expression of the law of energy conservation, and is often expressed as the principle that, for an isolated system (one that does not exchange energy with its surroundings), total internal energy is a conserved quantity.

2a. Second Law (Clausius): *It is impossible to construct a device that operates in a cycle and whose sole effect is to transfer heat from a cooler body to a hotter body.*

This is just one formulation of several equivalent formulations of the second law of thermodynamics, and is known as the "Clausius" statement of the second law. Another well-known formulation is due to Kelvin and Planck:

2b. Second Law (Kelvin-Planck): *It is impossible to construct a device that operates in a cycle and produces no other effect than the production of work and exchange of heat with a single reservoir.*

The Kelvin-Planck statement of the second law is often paraphrased as "there is no such thing as a perpetual motion machine". A third statement of the second law makes use of the concept of "entropy". In thermodynamics, entropy is defined in terms of the heat energy that is generated when a thermodynamic system moves from one state to another at a constant temperature:

2c. Second Law (entropy law): *The entropy change of an isolated system is always greater than or equal to zero.*

When an isolated system reaches thermodynamic equilibrium, there is no longer any change of thermodynamic state, and entropy reaches a maximum value.

The third law of thermodynamics expresses a relationship between entropy production and the Kelvin temperature scale:

3. Third Law: *At absolute zero, 0 K, for any pure chemical compound, entropy production is zero.*

Some authors talk about a "fourth" law of thermodynamics that is otherwise, and more commonly, known as the so-called "zeroth" law. This law establishes the existence of an empirical temperature function for thermodynamic systems, via the following principle:

0. "Zeroth Law": *If two bodies are in thermodynamic equilibrium with a third body , then they are in thermodynamic equilibrium with each other.*

These are the laws of classical equilibrium thermodynamics. They are the foundation of all applications of thermodynamics to equilibrium systems, and describe relationships between energy, heat and work that, to our best knowledge, apply to all real-world energetic processes.

2. Complex Systems Phenomenology

Jørgensen's fourth, ecological law of thermodynamics is intended to describe the response of systems when they are pushed away from thermodynamic equilibrium, and hence is not meant as an addition to the theoretical corpus of equilibrium thermodynamics. Two distinct types of claim are being made in Jørgensen's formulation:

> i) it offers an abstract characterization (in terms of such concepts as "exergy", "components", "selection", etc.) of a set of observed, phenomenological regularities in the organization and behaviour of complex systems that are driven far from equilibrium by thermodynamic gradients;
>
> ii) it asserts that these phenomenological regularities ought to be understood as a direct consequence of thermodynamic imperatives.

The critical point that I wish to make about Jørgensen's formulation has to do with the second claim, but in order to make the point, we will need to take a closer look at the first claim, and the particular characterization that Jørgensen gives of the phenomenology of complex systems.

What are the observed phenomenological regularities that Jørgensen's law attempts to capture? At a general level these include brute facts such as that nature is structured in hierarchical levels that can sometimes be decomposed into weakly interacting subsystems, and that levels of organization seem to develop and co-evolve with the entities that reside at that level (Wimsatt 1996, 242). One mechanism by which hierarchical organizations can be constructed is via the spontaneous emergence of order in phase transitions, or self-organizing phenomena. An often-discussed example is the Bénard transition, which occurs when a layer of heated fluid develops convection cells at a critical value of the imposed temperature gradient. Before the transition, the dissipation of heat occurs through random collisions of molecules in the fluid (conduction), but at a certain temperature the collection of molecules begins to move in an organized manner in the form of macroscopic convection cells. Jørgensen's law offers a characterization of the relationship between certain thermodynamic properties of the Bénard system and the spontaneous emergence of order at the macroscopic scale.

Another simple example is observed in the behaviour of a simple child's toy, a "tornado in a bottle". One can buy a two-way screw top that will connect two 2-litre pop bottles together at the neck, making an hourglass shape. If one of the bottles is filled with water and the apparatus turned so that the water-filled end is upright, water will trickle through a hole in the screw top into the bottle below. The process of draining the bottle is slow, and takes a couple of minutes. Now, if one gives the apparatus a circular twist, imparting angular momentum to the water in the top end, a whirlpool will form in the top bottle, and the water will drain out of the top end much more quickly (a matter of 10-15 seconds). The funnel shape that emerges in the top bottle is reminiscent of a tornado funnel. But why does a macroscopic like the whirlpool emerge at all, and what is the connection between this emergence and the rate at which water is drained from the bottle? Jørgensen's law subsumes this case, and similar phenomena, under a common thermodynamic characterization.

With respect to ecosystem phenomenology, recall the discussion of Chapter 5. Patterns of ecosystem development include the tendency for ecosystems to i) capture, store and cycle more energy and matter, ii) have species occupy higher average trophic levels with greater trophic efficiencies and longer food chains, and iii) develop more articulated food webs, as ecosystems move from immature to mature stages. Jørgensen's law is intended to describe and explain all these phenomena as well, as well as more specific manifestations of these general phenomena in specific cases.

Returning to our "tornado in a bottle" example, what is the connection between the formation of the whirlpool and the rate of drainage of the top bottle? An explanation that appeals to thermodynamic concepts might go as follows.

When all the water is in the bottom bottle, the system is at equilibrium, and no change is observed or expected. When the bottle is inverted, the system is pushed far from equilibrium, and a gravitational potential energy gradient is established whose magnitude is measured by the distance between the water levels in the top and bottom bottles. According to (another formulation of) the second law of thermodynamics, all isolated systems (we can assume the bottles form an isolated system for the moment) tend toward thermodynamic equilibrium, a state in which all potential energy gradients are zero; hence, there is a flow of water from the top to the bottom which acts to reduce the potential energy gradient.

What is interesting from a complex systems perspective is the correlation between the emergence of an ordered macroscopic structure (the whirlpool), and the increased drainage rate, which can be interpreted as an increased rate of potential energy dissipation. The whirlpool is what is known as a "dissipative structure", a macroscopic ordered state that facilitates the dissipation of potential energy, and which exists only as long as, and in virtue, of the presence of a potential energy gradient. The whirlpool makes it possible for the system to drain in 10 to 15 seconds, and disappears once equilibrium is regained.

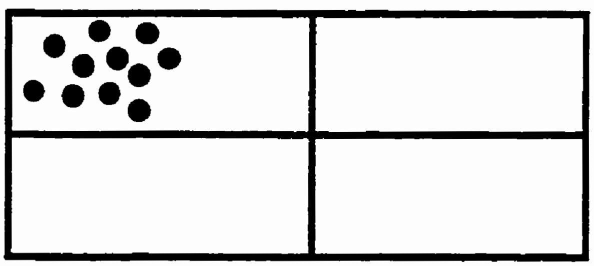
3. Ecology and Nonequilibrium Thermodynamics

Very few (if any) systems we encounter in the world are at thermodynamic equilibrium. Living systems and ecosystems are far-from-equilibrium dissipative structures, and classical equilibrium thermodynamics is of little help in understanding the particular thermodynamic properties of these systems. Jørgensen's ecological law of thermodynamics is an attempt to formalize a relationship between the generation of complex macroscopic structures and the thermodynamic properties of open, far-from-equilibrium systems. To understand this proposed law, we need to introduce some terminology, and review some of the history of appeals to nonequilibrium thermodynamics in ecology. The key terms in Jørgensen's ecological law of thermodynamics that will require elaboration are the concepts of "exergy" and "selection".

Exergy

"Exergy" is a term used most often in engineering thermodynamics, and denotes the amount of *work* a system can perform when it is brought into thermodynamic equilibrium with its environment. As such it is a measure of the distance from thermodynamic equilibrium of a system relative to its environment. Exergy has units of energy, and can be viewed as a generalized thermodynamic potential[1]; hence, a dissipative process is one that dissipates exergy.

Jørgensen also uses the exergy concept to talk about the amount of structure and potential energy that is *stored in* organized macroscopic states. Consider by way of example a simple box with two partitions, and a gas that has been introduced into a corner of one of the subcompartments:

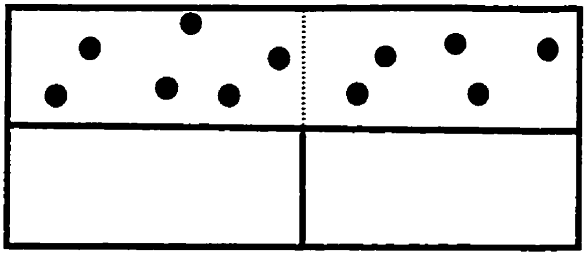


The gas has a high potential energy relative to its environment (the subcompartment), and has the potential to do work through expansion (a positive exergy). Once the gas is distributed homogeneously throughout the subcompartment the potential energy gradient is dissipated, and no more work is possible (exergy is zero). But what happens if one of the partitions is

---

[1] Evans (1969) shows how exergy differences can, in certain circumstances, be identified with better known thermodynamic work potentials, such as the Gibbs free energy, the Helmholz free energy, and enthalpy.

removed? The reconfiguration makes possible further work within the system, as the gas expands to fill the new volume:



Thus, a different source of exergy is contained in the *structural constraints* that constitute the organization of the system. Were these structural constraints to be removed, further work energy would be made available, and the quantity of this work energy is a measure of the degree of structure within the system.

Let us now recall Jørgensen's ecological law of thermodynamics:

> A system that receives a through-flow of exergy (high quality energy) will have a propensity to move away from thermodynamic equilibrium, and, if more combinations of components and processes are offered to utilize the exergy flow, the system has the propensity to select the organization that gives the system as much exergy as possible. (1997, 345)

The expression "through-flow of exergy" refers to the externally imposed potential energy flow. It is the exergy of this flow that is *dissipated* by the formation of an organized, macroscopic structure like the whirlpool. But one can also refer to the exergy *stored within the macroscopic structure itself*, and this exergy is *increased* as the macroscopic structure develops. It is this stored exergy that is referred to in the phrase "that gives the system as much exergy as possible".

Several comments can be made at this point. First, note both the difference and the similarity between "exergy-talk" and the more familiar "entropy-talk" that one often hears with regards to far-from-equilibrium thermodynamic processes. In the case of the exergy of the focal system, an *increase* in exergy corresponds to a *decrease* in entropy (i.e. an increase in order and organization). Similarly, the exergy of the external potential energy gradient (the exergy of the "environment") *decreases*, corresponding to an *increase* in entropy. The two forms of exergy/entropy are related to one another hierarchically. A developing system (the focal system) extracts exergy from the next higher level (its environment). Some of the available energy is used by the system to create ordered structure which contains available energy at the focal level. The bulk is dissipated to the next lower level (the microscopic). Overall, exergy (entropy) is dissipated (increases), in conformity with the second law of thermodynamics.

Second, there are important connections between the different types of exergy discussed above and information-theoretic descriptions of physical systems. The relationship between information, entropy, exergy and energy is a large and confusing topic, since both information and entropy have been defined in various nonequivalent ways in thermodynamics, statistical mechanics, and mathematical information theory. I will make only two observations here. First, recall the discussion of information theory in Chapter 5, and its application to the representation of ecological networks. The information-theoretic measures of structural organization that were introduced in that chapter (in terms of the "mutual information" of a network) may be viewed as measures of *structural exergy*, since they represent the degree to which the direction of flows is constrained by network organization. I will expand on this point later in the current chapter. Second, the interpretation of exergy as the energy available to do work is consistent with Leon Brillouin's identification of "physical information" with the opposite-signed quantity of thermodynamic entropy, so-called "negentropy" (Brillouin 1962), though it must be remembered that thermodynamic entropy and energy do not have same dimensions, and hence cannot be strictly identified. Exergy, on the other hand, does have the units of energy, which is one of the sources of its utility in formalizing these relationships.

Selection

What does Jørgensen mean by "the propensity to *select* the organization that gives the system as much exergy as possible"? Why should systems maximize energy storage, and what is the mechanism by which they are selected? These questions cannot be properly addressed without some familiarity with the background of attempts to formulate principles of nonequilibrium thermodynamics, and of the attempts to apply these to biological and ecological systems.

Maximum Entropy (Prigogine)

The first such attempt is known as "irreversible thermodynamics", developed by Ilya Prigogine (1947) from theoretical work initiated by Lars Onsagar (1931). The theory applies to systems that are near enough to equilibrium that the relationships between thermodynamic potentials and their corresponding induced fluxes of matter and energy can be treated as linear. Onsagar observed that a thermal gradient imposed on a homogeneous mixture results not only in a flow of heat through the medium, but also in the differential migration of one or more chemical species in the mixture. There is thus a coupling between mass diffusion and heat flux. Prigogine recognized that this effect is similar to the Le Chatelier-Braun principle, which says that any perturbation to a factor contributing to equilibrium induces a compensating change in an opposing factor. Prigogine formulated a unifying description of how such coupled ensembles of flows behave near equilibrium. He showed that for an arbitrary collection of processes near equilibrium, *the entropy produced by the collection of flows is maximized.*

Ecosystem ecologists were interested in the collective organization exhibited by near-equilibrium systems, and the simplicity of a single "goal function" that governed the whole process. However, it was recognized early on that linear irreversible thermodynamics had little applicability to the nonlinear, far-from-equilibrium phenomena that characterized biological and ecological systems.

For a while Prigogine worked to develop a nonlinear description of how systems behave farther from equilibrium (Glansdorff and Prigogine 1971), but he eventually came to believe that no purely phenomenological theory of nonequilibrium thermodynamics was possible, and turned his attention to the application of nonlinear dynamics to statistical descriptions of thermodynamic systems. He coined the term "dissipative structure" to describe far-from-equilibrium systems, but sought explanations of the emergence of such structures in terms of symmetry-breaking in the micro-level dynamics of constituent particles (so-called "order through fluctuations").

Maximum Power (Lotka, Odum)

Alfred J. Lotka (1925) introduced the "maximum power principle". This principle states that in the competition for material and energetic resources, those natural systems will prevail which maximize the rate at which energy is converted into work, i.e. maximize power output. Ecosystem ecologist Howard Odum has applied the maximum power principle to ecological systems of varying compositions and scales. The rationale for this principle can be illustrated by the example of fossil fuel power generation (Jørgensen 1997, 90). The upper limit of efficiency for any thermodynamic engine such as a turbine is determined by the Carnot efficiency. A steam turbine could run at 80% efficiency, but it would need to operate at a nearly infinitely slow rate to achieve this efficiency. Or a steam turbine could run very quickly and run very inefficiently. Actual operating efficiencies for modern steam-powered generators are closer to 40%, roughly half their Carnot efficiency. Operating at this efficiency maximizes the useful power output of the generator. Similarly, it is argued, complex natural systems metabolize, grow and reproduce by converting free energy into work, and systems that operate near the intermediate efficiency regime will out-compete those that operate at lower rates with high efficiency, or higher rates with low efficiency.

Unlike Prigogine's maximum entropy principle, the maximum power principle was not derived from a formal theory of nonequilibrium thermodynamics. It, like many of the principles discussed in this section, was simply postulated as a plausible candidate for a nonequilibrium thermodynamic goal function. The arguments for these various candidate principles have rested almost entirely on claims for their consistency with observed phenomena, and their success at explaining and predicting these phenomena.

Maximum Entropy (Swenson)

Rod Swenson has attempted in recent years to resuscitate a principle of maximum entropy production for far-from-equilibrium thermodynamic systems (Swenson 1991, 1997). As we have seen, the spontaneous emergence of order in far-from-equilibrium systems is correlated with increased entropy production and potential energy dissipation. This is consistent with the second law of thermodynamics, but the second law says nothing about which out of a set of available paths a system will take to increase its entropy:

> The answer to the question is that *the system will select the path or assembly of paths out of otherwise available paths that minimizes the potential or maximizes the entropy at the fastest rate given the constraints.* This is a statement of the law of maximum entropy production, the physical principle that provides the nomological basis . . . for why the world is in the order production business. (Swenson 1997, 83)

The similarity between Swenson's maximum entropy principle and Jørgensen's ecological law of thermodynamics is evident. The differences involve a choice between entropy and exergy as the relevant thermodynamic quantities. One major objection to Swenson's principle is that, strictly speaking, classical thermodynamic entropy is defined only for systems at equilibrium. For this reason, many ecosystem theorists prefer to formulate nonequilibrium thermodynamic principles in terms of surrogate quantities such as exergy, which are well-defined for systems far from equilibrium.

Maximum Exergy Dissipation (Schneider and Kay)

Eric Schneider and James Kay (1994) posit that systems evolve in a way that facilitates the degradation of exergy at the fastest rate possible:

> The thermodynamic principle which governs the behaviour of systems is that, as they are moved away from equilibrium, they will utilize all avenues available to counter the applied gradients. As the applied gradients increase, so does the system's ability to oppose further movement from equilibrium. (1994, 29)

The first sentence states that a system will spontaneously organize in a way that minimizes an applied potential gradient (the exergy of the environmental gradient). The dissipative structure that emerges acts to return the system to equilibrium by facilitating the dissipation of exergy. If there is more than one way of achieving this end, the system will "select" that form of organization that is more efficient at dissipating exergy.

The last sentence adds a new element to the characterization of nonequilibrium thermodynamic principles. It states that as a system is pushed farther and farther from equilibrium (the applied gradients increase), it will spontaneously organize in such a way that the new form of organization is more efficient at dissipating exergy than the previous form of organization, with the consequence that the new form of organization is more resistant to further movement away from equilibrium than the previous form of organization. The image that comes to mind is of a spring whose restoring force increases as it is stretched farther and farther from its equilibrium point.

Maximum Exergy Storage (Jørgensen and Meyer)

Jørgensen and Meyer (1979) formulated a principle of nonequilibrium thermodynamics that referred not to exergy *dissipation* in imposed environmental gradients, but to exergy *storage* within dissipative structures themselves. They claimed that systems generally act to store within themselves as much exergy as possible. Jørgensen's (1997) ecological law of thermodynamics is basically a reformulation of this principle that makes explicit which exergy process, internal or external, is the selectively-relevant one when multiple developmental paths are available to the system. For Jørgensen, if two pathways differ in the amount of exergy that is drawn into the system, the pathway that draws in the most exergy is the one that will be selected. This is in contrast to Schneider and Kay, who argue that the selectively-relevant process is external exergy dissipation. They claim that if two possible developmental pathways are available to the system, the pathway that is most efficient at dissipating the external potential energy gradient is the one that will be selected.

Are the principles of maximum exergy dissipation and maximum exergy storage consistent with one another? Certainly both external exergy dissipation and internal exergy storage can increase as systems are driven farther from equilibrium, but will the developmental pathways that maximize external exergy dissipation be *the same* as the ones that maximize external exergy storage? The question is difficult to answer on theoretical grounds alone, and is complicated by the fact that most real-world biophysical systems go through developmental stages characterized by differing types of thermodynamic behaviour. During the stages of rapid growth that characterize early successional stages in ecosystems (and infancy to maturity in organisms), the exergy extracted from environmental gradients is used mostly for accumulation of biomass, and as biomass accumulates, more exergy is required for maintenance of organizational structure, resulting in a greater channeling of exergy into storage and greater dissipation of environmental exergy. But once a system has developed sufficient biomass, the amount of exergy that it is possible to capture becomes limited, and further development of the system will involve increases in dissipative efficiency without a corresponding increase in biomass. From the perspective of internal exergy storage, the move from growth to maturity involves a shift from increasing internal potential energy stored in biomass, to increasing organization through the articulation of structural constraints (i.e. increasing *structural* exergy). Increasing structural organization can result in increasing dissipative power through greater cycling and retention of stored matter and energy.

Thus, the two maximization principles appear to be consistent, but over the course of development there is a change in the way that the system functions to dissipate externally applied gradients, from earlier stages where increasing dissipation is achieved by extracting energy from the environment and channeling it into biomass accumulation, to later stages where increasing dissipation is achieved through the development of material and energetic cycles that are more efficient at dissipating energy.

To sum up, Jørgensen's ecological law of thermodynamics is one of several "goal function" or "maximization principle" approaches to nonequilibrium thermodynamics that are currently the focus of some attention in the ecological literature. What all of these principles have in common is the assertion that the development of complex, organized structures in far from equilibrium systems is a *means by which* systems regain equilibrium as quickly as possible. That is, they exist *in virtue of* their dissipative properties, and ought to be viewed as manifestations of a fundamental thermodynamic imperative. This thermodynamic imperative is related to, but is not identical with, the second law of thermodynamics. The second law states that all isolated systems tend to equilibrium, but it does not say how the approach to equilibrium will proceed. Jørgensen's law, and the others described above, add a "selective" component to the approach to equilibrium; of a variety of developmental pathways that might be available to a system, the ones that are realized are those that maximize some thermodynamic quantity.

4. Types of Theory in Complex Systems Ecology

In this section I discuss reasons for resisting the characterization of Jørgensen's law, or the other laws discussed above, as thermodynamic laws. The basis for this resistance is a recognition that thermodynamics is a type of physical theory that, by itself, simply cannot do the job that Jørgensen and other complex systems ecologists want it to, and further, that theoretical explanations of complex systems phenomenology necessarily make reference to *non*thermodynamic physical theories.

Along with his distinction between "principle" and "constructive" theories (see the discussion of Chapter 8, section 6), Einstein introduced another distinction, between "theoretical" physics and "phenomenological" physics (Einstein 1936)[2]. The laws of phenomenological physics are empirical regularities that are observed to hold true of a certain class of phenomena. The Ideal Gas Law, $PV = kT$, is a phenomenological law. The law simply states that the product of the observed temperature and volume of a gas is proportional to the observed temperature of the gas. The Ideal Gas Law says nothing about *why* pressure, volume and temperature should be related in this way. Kepler's laws of planetary motion are another example. They accurately describe the paths of the planets around the sun, but they don't explain *why* planets should travel in ellipses, *why* planets should sweep out equal areas in equal times (the Areal Law), or *why* the periods of planets should vary as the 3/2 power of their radii (the Harmonic Law). Laws such as these, while practically useful for certain purposes, are purely descriptive and theoretically uninteresting. They can't be used to deduce other known relationships between variables, nor deduce new relationships that can be tested experimentally.

---

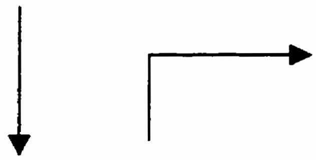
[2] I am grateful to Francisco Flores for helpful discussions on Einstein's philosophy of science. See Flores 1998 for a detailed analysis of the principle/constructive theory distinction in Einstein's work, and an application of the distinction to space-time theories.

According to Einstein, the task of *theoretical* physics is to *explain* these phenomenological laws. It is within the category of theoretical physics that Einstein situates "principle" and "constructive" theories. Principle theories are composed mainly of definitions of physical terms and principles or postulates that describe general characteristics of all natural processes. These principles function in explanations of phenomena *by imposing constraints that must be satisfied by the behaviour of all objects or processes in the world.*

Constructive theories, on the other hand, explain phenomenological laws by postulating "hypothetical constituents" that are used to "build up a picture of the more complex phenomena out of the materials of a relatively simple formal scheme" (Einstein 1919, 228). As discussed in Chapter 8, the kinetic theory of gases is an example of a constructive theory. The kinetic theory gives a simple description of a molecule of gas, either as a tiny sphere, a point source of force, or as a weakly-interacting sphere. The molecules are allowed to interact and collide with one another, and by averaging over the interactions one can construct a description of a macroscopic gas. Yet in order to derive $PV = kT$, *the interactions of the molecules must be constrained by a principle theory*, in this case, Newton's laws of motion. Newton's first law restricts the allowable motions of the molecules so that, between collisions, the molecules must travel in straight lines with uniform velocities. The principle of conservation of momentum, a deductive consequence of Newton's laws, imposes additional constraints, and so on. Only when these constraints are satisfied can the Ideal Gas Law be deduced.

I offer the following schematic representation of the relationship between levels of theory and types of explanation for the ideal gas law:

Principle Theory (Newton's Laws)



Phenomenological Regularity
(Ideal Gas Law)

Constructive Theory (Kinetic Theory of Gases)

The principle theory, Newton's laws, constrains the behaviours of the hypothetical constituents postulated by the constructive theory, the kinetic theory of gases. This in turn allows the deduction of a phenomenological relationship between thermodynamic variables, the Ideal Gas Law.

Now, Einstein regarded the laws of classical thermodynamics as a paradigm example of a principle theory of theoretical physics. Recall the formulations presented in section 1:

1. First Law: *The change in the internal energy of a system, defined as the difference between the heat it absorbs and the work it performs, is the same for all transformations between a given state and a final state.*

2a. Second Law (Clausius): *It is impossible to construct a device that operates in a cycle and whose sole effect is to transfer heat from a cooler body to a warmer body.*

2b. Second Law (Kelvin-Planck): *It is impossible to construct a device that operates in a cycle and produces no other effect than the production of work and exchange of heat with a single reservoir.*

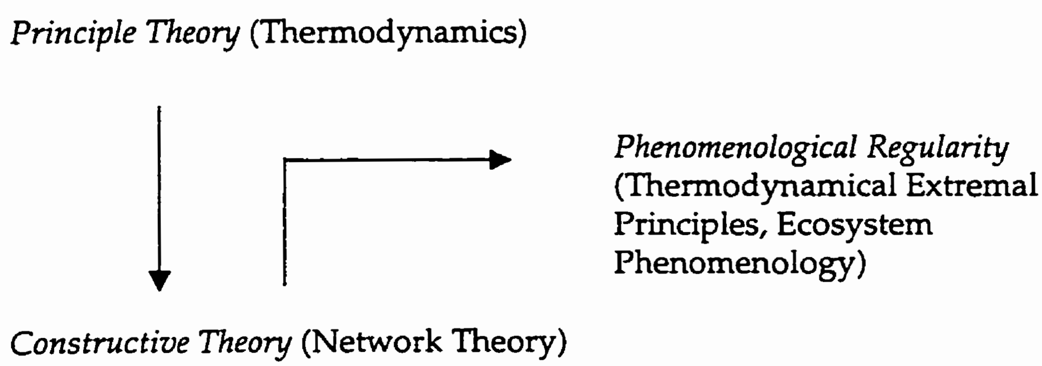
2c. Second Law (entropy law): *The entropy change of an isolated system is always greater than or equal to zero.*

3. Third Law: *At absolute zero, 0 K, for any pure chemical compound, entropy production is zero.*

These laws express constraint relations that must be satisfied by any physical process. They do not issue in any predictions of observable phenomena other than those that are immediately subsumed by the above definitions (knowing the second law of thermodynamics, and assuming that no external work is being done on my cup of coffee, and that it is not in contact with a heat reservoir, I can predict that it will be colder one minute from now).

Thus, one reason for resisting the characterization of Jørgensen's law as a "fourth" law of thermodynamics is that thermodynamic laws, as they are commonly understood, simply are not the sort of law that can function in the way that Jørgensen, and other thermodynamically-inclined complex systems ecologists, want them to. Just as Newton's laws cannot serve, by themselves, to explain the Ideal Gas Law, so thermodynamic laws cannot, by themselves, explain the observed phenomenological regularities that characterize far-from-equilibrium systems.

One might respond by saying that Jørgensen's law is a different type of thermodynamic law, and should not be expected to apply to physical systems in ways exactly analogous to the classical equilibrium laws. This is a plausible line of defense, but I would suggest that a more appealing approach is one that is consistent with established understanding of thermodynamic concepts and principles. To this end, I offer a proposal for reconceiving the relationship between thermodynamics and complex systems phenomenology that makes use of the principle/constructive theory and theoretical/phenomenological physics distinctions introduced above. Consider the following schematic relationship:



I believe that theories in complex systems ecology are decomposable in the way suggested by the diagram. What we observe in complex systems far from equilibrium are hierarchically organized structures, exhibiting a complex dynamics, growing, developing and differentiating over time, dissipating (and internalizing) large amounts of available energy. These are the *phenomenological* features that a theory of complex ecosystems tries to explain.

These theories then postulate a class of idealized, hypothetical constituents, *networks* (including network components and their relations), as the entities out of which ecosystems are constructed. We postulate flows through these networks, and network theory allows us to trace the histories of these flows as they propagate through the network. *Network theories are the constructive theories of complex systems ecology.*

The observed phenomenological regularities cannot be generated on the basis of network flows alone, however. To even apply the *concepts* of energy and entropy to the flows requires appeal to definitions of these terms derived from thermodynamics. Thermodynamic constraints must then be applied to the flows, ensuring conservation and dissipation in accordance with the first and second laws of thermodynamics. Subject to these constraints, theories in complex systems ecology show how network flows begin to self-organize, harnessing greater and greater amounts of available energy, increasing energy and material throughput, cycling, and so on. New levels of organization arise naturally as symmetry-breaking and self-organization continue, with the end product being a hierarchically organized array of levels of organization exhibiting the gross features of biological and ecological organization we observe in the world. *Thermodynamics is the principle theory for complex systems ecology.*

The explanation and derivation of complex systems phenomenology involves the interaction of two types of physical theory, one contributing a set of concepts and relations that state how thermodynamic concepts and quantities are to be applied to physical systems, and one that states how physical system are organized, and how this organization develops over time when subject to thermodynamic constraints. Thus, complex systems phenomena *are* thermodynamic phenomena, but they are not *solely* thermodynamic phenomena. They are also "systems" or "network" phenomena.

5. Networks and Nonequilibrium Thermodynamics

Network concepts can be found in most of the formulations of nonequilibrium thermodynamic principles discussed in section 3. Jørgensen's own formulation makes explicit reference to "components and processes":

> A system that receives a through-flow of exergy (high quality energy) will have a propensity to move away from thermodynamic equilibrium, and, if more combinations of components and processes are offered to utilize the exergy flow, the system has the propensity to select the organization that gives the system as much exergy as possible. (1997, 345)

Physical networks are defined as sets of components related by flows of energy, matter and information ("processes").

Complex systems theorists who specialize in network analysis are less prone to treat network structure as subordinate to thermodynamic principles in the characterization of far-from-equilibrium systems. For example, Howard Odum, ecology's strongest advocate for the universal significance and applicability of Lotka's maximum power principle, has also developed an elaborate network formalism for representing ecosystems. He states that Lotka failed to realize that

> [t]he principle of maximum power and its corollaries concern a system's network organization. Consequently, they cannot be expressed with single equations of classical thermodynamics, which concern only one energy transformation step at a time. A network language is required. (Odum 1995, 311)

Odum believes that all the thermodynamic properties that are particular to ecosystems are *a function of their network structure.*

Network theorist Robert Ulanowicz echoes this view with respect to his own, network- and information theory-based approach to ecosystems phenomenology:

> [A] key postulate in the development of the current thesis should be understood; to thermodynamically describe an ecosystem, it is sufficient to quantify the underlying networks of material and energy flows. A more general form of the postulate would read: *the networks of flows of energy and material provide a sufficient description of far from equilibrium systems.* (Ulanowicz 1986, 30)

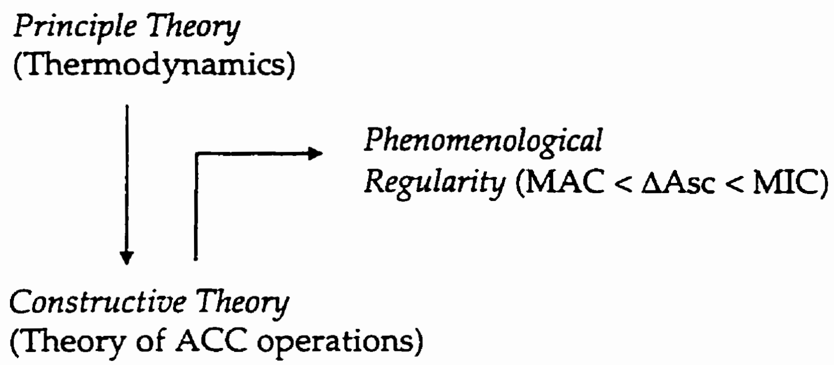
We are familiar with Ulanowicz's "ascendency" approach to ecosystem organization from Chapter 5. Let us use Ulanowicz's theory to illustrate the potential of the principle/constructive theory distinction to represent theories in complex systems ecology.

We saw in Chapter 5 how positive feedback among network components may function as an agent of growth and development in ecosystems. In this case the phenomenological regularity to be explained is "growth and development", which in Ulanowicz's theory is described quantitatively as the pattern of increase over time of the "ascendency" (Asc) of the ecosystem network; i.e. the product of total system throughput (TST) and average mutual information (AMI).

Ulanowicz argues that *positive feedback* or *autocatalytic* cycles (I'll use the abbreviation "ACC") are the primary agents that drive the increase in network ascendency. ACC's may be regarded as the "hypothetical constituents" of Ulanowicz's *constructive* network theory. The core of Ulanowicz's theory is a description of how changes in the connectivity and throughput of a system are constrained to remain somewhere in between the two extremes of maximal and minimal connectance, between total randomness and total order. We'll call these maximal and minimal states MAC and MIC for short. Ulanowicz (1986) shows how the basic ACC operations of growth enhancement, selection and competition (see chapter 5) are sufficient to produce a pattern of increasing Asc that replicates the pattern of growth, maturity and senescence observed in ecosystems.

The *principle* theory for this approach, as always, is thermodynamics, since conservation and dissipation are essential constraints on the network formalism.

Our picture of the structure of Ulanowicz's "ascendency" theory looks something like this:



This characterization of ascendency theory abstracts away from the details of the theory and highlights the relationships between physical principles and formal structures that function together to generate a phenomenological regularity.

Conclusion

In this chapter I have argued that Einstein's principle/constructive theory distinction can be usefully employed for understanding the structure of contemporary theory in complex systems ecology. Thermodynamics is a principle theory which by itself is inadequate to account for structural and functional regularities observed in complex, hierarchically organized systems such as ecosystems. Network theories are the constructive theories of systems ecology, theories of the hypothetical constituents of ecosystems which by themselves are not sufficiently constrained to model complex systems phenomena. When thermodynamic imperatives constrain network models, one can generate (within theoretical models) many of the gross features of complex ecosystem phenomenology.

Complex systems ecologists may be divided on the issue of the priority of thermodynamic over network principles, but I believe the debate can be diffused by recognizing the necessity, and complex interaction, of both elements for the description and explanation of complex systems phenomena.

Conclusion

In this dissertation I have argued for the viability of a new approach to the investigation of scientific and philosophical problems, one that is best described as "ecological". I have tried to defend the plausibility of an expanded ecological science that encompasses traditional ecology as well as the various ecological research traditions that one finds in psychology, the social sciences, and philosophy.

The dissertation is divided into three Parts. Part One (chapters 1, 2 and 3) is a survey and critique of environmental philosophy as the field is currently conceived and practiced. I argued that environmental philosophy is handicapped by a failure to acknowledge the centrality of ecological themes in its core philosophical problems, that it conceives itself as a species of ethical, social and political philosophy when in fact its core philosophical problems are best understood in relation to nonnormative issues concerning the nature and severity of the ecological crisis, and the ecological dimensions of human nature and human activity in the world. Thus, I concluded that environmental philosophy should reconceive itself in a fashion that highlights these ecological themes, that interprets environmental philosophy as a true philosophy of *system-environment relationships*, i.e. a philosophy of ecology.

In constructing a science and philosophy of ecology, there are many resources upon which one can draw. There is traditional ecological science, the science of natural ecological communities that is most often taught in university biology and ecology departments. There are also a large number of ecological research traditions in fields outside of traditional ecology, such as ecological psychology, ecological economics, and ecological anthropology, as well as ecological traditions in philosophy, that conceive the phenomena in their respective domains as in one way or another dependent on interactions between systems and their environments. The vision of ecological science that is presented at the end of Chapter 3 is one that conceives all of these ecological traditions as subdisciplines within a broader, shared scientific and philosophical enterprise, an ecological approach to "natural philosophy".

My particular choice for an ecological framework is one that allows both the scientific and philosophical ecological traditions to engage in productive dialogue. In the chapters of Part Two (4, 5, 6 and 7) I sketched such a framework, the elements of which involve i) the application of concepts and theories drawn from the *complex systems sciences* to the study of ecological and evolutionary phenomena, and ii) a conception of the *ecological niche* that is sufficiently general to apply to individual organisms as well as to populations and species.

In Chapter 4 I argued that these two elements are important to the project of unifying the various subdisciplines of traditional ecology. The main ecological subdisciplines are divided between "demographic"/"evolutionary" approaches, and "physiological"/"systems-oriented" approaches. Complex systems approaches to ecology and evolution offer the promise of a framework for relating broad-scale physiological and demographic processes, and the niche concept may function as a formal device for linking ecological processes at the level of individual organisms to population and ecosystem-level processes. A goal for a unified ecological science, I argued, is to develop a complex systems approach to the ecological niche.

Chapters 5 and 6 were devoted to complex systems theories in ecology and the niche concept, respectively. In Chapter 5 I surveyed the theoretical components of what I call "complex systems ecology", or CSE. CSE is a development of ecosystem and systems ecology that draws on information theory, network theory, thermodynamics, and hierarchy theory, to articulate a comprehensive theory of the dynamics of complex ecological systems. I introduced the elements of network theory to illustrate how ecological systems are represented in the network theories of Robert Ulanowicz and Bernard Patten, and to introduce some concepts and formalism that would appear again in later chapters.

In Chapter 6 I surveyed the classical niche concepts of Grinnell, Elton, Hutchinson and MacArthur, and introduced a network-theoretic niche concept based on Bernard Patten's "environ theory", which Patten believes offers a unifying framework that subsumes all the classical niche concepts. A notable feature of Patten's niche concept is its dual input-output conception of the niche environment.

Chapter 7 was an important one for the dissertation, for it introduced the concepts of *ecological psychology*, a nontraditional ecological discipline, and argued for the utility of such concepts for the advancement of a unified ecological science. I showed how the concepts of "affordance" and "ecological information" can be applied to problems in behavioural, population and ecosystem ecology, and highlighted the remarkable similarities between Patten's input-output niche concept and the neo-Gibsonian notions of perception-action cycles and affordance-effectivity structures. I suggested that a synthesis of Gibsonian concepts, dynamical systems approaches to motor coordination, and Patten's environ theory, offers a tantalizing (if still undeveloped) framework for understanding perception and action in ecological terms. In a concluding discussion I suggested several ways in which the ecological framework developed in Part Two may be applied to the traditional normative problems of environmental philosophy.

The final two chapters (Part Three) investigated conceptual issues relating to the foundations of the complex systems sciences. Chapter 8 looked at the nature of the "domain-independence" exhibited by the complex systems sciences, and concluded that, contrary to the view of James Franklin, these sciences should not be understood as purely mathematical sciences that study abstract formal structures. Rather, the complex systems sciences explain phenomena by constructing models that embody both formal and physical constraints on the behaviour of physical systems. In a concluding discussion I related one of the issues raised in the chapter — the question of whether mathematical structures are epistemically accessible to the senses — to the

Gibsonian problem of understanding the perceptual control of action via the detection of invariant structures in the ambient energetic array, and speculated that a "naturalized" mathematical structuralism might shed some light on the peculiar difficulties involved in understanding how ecological information relates to the dynamics of movement.

Chapter 9 examined a claim that has been made by several complex systems ecologists, that the application of thermodynamic principles to the explanation of complex systems phenomena requires that one postulate a new, fourth, or "ecological" law of thermodynamics that describes how systems respond when they are driven far from thermodynamic equilibrium. I argued that, as a "principle" theory in Einstein's sense of that term, thermodynamic constraints cannot function in the way that complex systems ecologists would like them to. What is really going in appeals to new "thermodynamic" principles in complex systems ecology almost always involves, I argued, a tacit appeal to an underlying network representation of ecological systems. I concluded that Einstein's distinctions between "principle" and "constructive" theories, and between "theoretical" and "phenomenological" physics, may be applied to explanations of phenomena in complex systems ecology. Thermodynamics acts as a principle theory, constraining the dynamics of formally represented networks (the constructive theory), and the resulting network theory is what generates the observed phenomenology. I used Robert Ulanowicz's "ascendency" approach to ecosystem phenomenology to illustrate how these distinctions may be applied to particular theories in complex systems ecology.

At a recent meeting of the International Society for Environmental Ethics (ISEE) at the annual Eastern division meeting of the American Philosophical Association, I attended three separate workshops hosted by the ISEE. The first workshop was on the role of ecology in environmental ethics, where a panel debated the merits of appealing to ecology to support ethical claims concerning the environment. The second workshop was an "author meets her critics" panel, and the subject of discussion was Kristin Shrader-Frechette and Earl McCoy's (1993) book *Method in Ecology: Strategies for Conservation*, which discusses the potential of ecological theory to serve the needs of conservation biology. The third workshop was another author-meets-critics panel, the subject in this case being David Abram's (1996) *The Spell of the Sensuous*, a remarkable book, written from the theoretical standpoint of a phenomenologist trained in the tradition of Husserl and Merleau-Ponty, that defends the reality and cogency of "animism" in the psychological experiences of pre-literate peoples.

I made two observations while attending these meetings that had an influence on my then-nascent dissertation proposal. The first was that the audiences in the three workshops did not overlap considerably. There was a different crowd of people interested in Abram's book than were interested in Shrader-Frechette and McCoy's book, and a different one again in the panel on ethics and ecology. Philosophers of science showed up for the methodology in ecology workshop but didn't bother with the animism workshop, and vice versa. Applied ethicists participated in the ethics workshop but weren't interested in either of the others. Yet all three workshops were sponsored by the same organization, the ISEE.

The second observation was that issues of ecological theory and natural complexity arose in all three workshops. Shrader-Frechette is a critic of ecosystem approaches to ecological management and conservation issues, and part of her objections involve criticism of the explanatory and predictive power of theoretical ecology, including network and thermodynamic approaches. In the ethics and ecology workshop there was a discussion of the use of biological and ecological theory in articulating life-based and ecosystem-based approaches to environmental ethics. And David Abram discussed a tantalizing theory relating the psychological experiences of pre-literate people who live in close contact with nature, to the finely-grained, local ecological knowledge that is required in order to survive and thrive in such environments.

It struck me at the time that the central topic in each of the workshops was the nature of ecological phenomena and ecological theory, and that what separated the various speakers and audiences was a lack of awareness of the commonalities in the problems that were being addressed, and of the potential for answers in one area to illuminate answers in another area. The fact that all three workshops fell under the rubric of "environmental philosophy" suggested that these common themes were not completely unrecognized, but still, only one or two people attended all three meetings.

The current dissertation is motivated by a perception of unity underlying apparent disunity in the ecological disciplines, and is intended as a contribution to a philosophy of ecology that reveals this unity.

BIBLIOGRAPHY

Abram, D. 1996. *The Spell of the Sensuous: Perception and Language in a More-Than-Human World*. New York: Pantheon Books.

Ahl, V., and Allen, T. F. H. (1996) *Hierarchy Theory: A Vision, Vocabulary, and Epistemology*. New York: Columbia University Press.

Allee, W. C., Emerson, A. E., Park, O., Park, T., and Schmidt, K. P. (1949) *Principles of Animal Ecology*. Philadelphia: Saunders.

Allee, W. C., and Park, T. (1939) "Concerning Ecological Principles" *Science* 89: 166-9.

Allen, C., Bekoff, M., and Lauder, G. (1997) *Nature's Purposes: Analyses of Function and Design in Biology*. Cambridge, MA: MIT Press.

Allen, T. F. H., and Hoekstra, T. W. (1992) *Toward a Unified Ecology*. New York: Columbia University Press.

Allen, T. F. H., and Starr, T. B. (1982) *Hierarchy*. Chicago: Chicago Press.

Amir, S. (1994) "The Role of Thermodynamics in the Study of Economic and Ecological Systems" *Ecological Economics* 10: 125-142.

Andrewartha, H. G. (1961) *Introduction to the Study of Animal Populations*. Chicago: University of Chicago Press.

Andrewartha, H. G., and Birch, L. C. (1954) *The Distribution and Abundance of Animals*. *Chicago*: University of Chicago Press.

Augustinovics, M. (1970) "Methods of International and Intertemporal Comparison of Structure". In A. P. Carter and A. Brody (eds.) *Contributions to Input-Output Analysis* Vol I. Amsterdam: North Holland, pp. 249-69.

Badiner, A. H. (1990) *Dharma Gaia: A Harvest of Essays in Buddhism and Ecology*. Berkeley: Parallax Press.

Barwise, J., and Perry, J. (1983) *Situations and Attitudes*. New York: MIT Press.

Baumol, W. J. and Oates, W. E. (1988) *The Theory of Environmental Policy*, 2nd Edition. New York: Cambridge University Press.

Beek, P. J., and van Wieringen, P. C. W. (1994) "Perspectives on the Relation Between Information and Dynamics: An Epilogue". *Human Movement Science* 13: 519-33.

Begon, M., Harper, J. L., and Townsend, C. R. (1990) *Ecology: Individuals, Populations, and Communities*, Second Edition. Boston: Blackwell.

Benzoni, F. (1996) "Rolston's Theological Ethic". *Environmental Ethics* 18 (4): 339-52.

Bernstein, N. (1967) *The Coordination and Regulation of Movements*. Elmsford, NY: Pergammon Press.

Blum, L. A. (1994) *Moral Perception and Particularity*. Cambridge: Cambridge University Press.

Boden, M. (ed.) (1996) *The Philosophy of Artificial Life*. London: Oxford University Press.

Boltzmann, L. (1905) *Populare Schriften*. Leipzig: J. A. Barth.

Bookchin, M. (1982) *The Ecology of Freedom: The Emergence and Dissolution of Hierarchy*. Palo Alto, CA: Cheshire Books.

Bookchin, M. (1990) *The Philosophy of Social Ecology*. Montreal: Black Rose Books.

Bormann, F. H., and Likens, G. E. (1979) *Pattern and Process in a Forested Ecosystem*. New York: Springer-Verlag.

Bootsma, R. (1988) *The Timing of Rapid Interceptive Actions: Perception-Action Coupling in the Control and Acquisition of Skill*. Amsterdam: Free University Press.

Bradie, M. (1994) *The Secret Chain: Evolution and Ethics*. Albany, NY: SUNY Press.

Brillouin, L. (1962) *Science and Information Theory*, second edition. New York: Academic Press.

Brooks, D. R., Collier, J., Maurer, B. A., Smith, J. D. H., and Wiley, E. O. (1989) "Entropy and Information in Evolving Biological Systems". *Biology and Philosophy* 4: 407-32.

Brown, J. H., Marquet, P. A., and Taper, M. L. (1993) "Evolution of Body Size: Consequences of an Energetic Definition of Fitness". *American Naturalist* 142: 573-84.

Brown, J. H. (1994) "Organisms and Species as Complex Adaptive Systems". In Jones and Lawton (ed.) *Linking Species and Ecosystems*. pp. 16-24.

Brown, J. H. (1995) *Macroecology*. Chicago: Chicago University Press.

Bub, J. (1973) *The Interpretation of Quantum Mechanics*. Dordrect, Holland: Reidel.

Buege, D. J. (1997) "An Ecologically-Informed Ontology for Environmental Ethics". *Biology and Philosophy* 12: 1-20.

Burns, T. P., Patten, B. C., and Higashi, M. (1991) "Hierarchical Evolution in Ecological Networks: Environs and Selection". In M. Higashi and T. P. Burns (eds) *Theoretical Ecosystem Ecology: The Network Perspective*. Cambridge: Cambridge University Press.

Burton, G. (1992) "Nonvisual Judgment of the Crossability of Path Gaps". *Journal of Experimental Psychology: Human Perception and Performance* 18: 698-713.

Burton, G., Turvey, M. T., and Solomon, H. Y. (1990) "Can Shape be Perceived by Dynamic Touch?". *Perception & Psychophysics* 5: 477-87.

Callicott, J. B. (1985) "The Metaphysical Implications of Ecology". *Environmental Ethics* 8: 301-16.

Callicott, J. B. (1987) "The Conceptual Foundations of the Land Ethic". In J. B. Callicott (ed) *The Conceptual Foundations of the Land Ethic*. Madison: University of Wisconsin Press.

Callicott, J. B. (1996) "Do Deconstructive Ecology and Sociobiology Undermine Leopold's Land Ethic?". *Environmental Ethics* 18 (4): 353-72.

Capra, F. (1996) *The Web of Life: A New Scientific Understanding of Living Systems*. New York: Anchor Books.

Casti, J. L. (1982) "Catastrophes, Control and the Inevitability of Spruce Budworm Outbreaks". *Ecological Modelling* 14: 293-300.

Casti, J. L. (1992) *Reality Rules I: Picturing the World in Mathematics*. New York: Wiley Inter-Science.

Clark, A. (1997) *Being There: Putting Brain, Body, and World Together Again*. London: MIT Press.

Clark, J. (1998) "What is Social Ecology?". In M. Zimmerman et al. (eds.) *Environmental Philosophy: From Animal Rights to Radical Ecology*. Upper Saddle River, NJ: Prentice Hall.

Clarke, S. (1986) "Emotions: Rationality Without Cognitivism". *Dialogue* 25:663-674.

Clements, F. E. (1916) *Plant Succession: An Analysis of the Development of Vegetation*. Washington: Carnegie Institute of Washington.

Code, L. (1996) "What is Natural About Naturalized Epistemology?". *American Philosophical Quarterly* 33:1-22.

Colwell, R. K. (1992) "Niche: A Bifurcation in the Conceptual Lineage of the Term". In E. F. Keller and E. A. Lloyd (eds.), *Keywords in Evolutionary Biology*. pp. 241-48. Cambridge: Harvard University Press.

Costanza, R., and Wainger, L. (1991) *Ecological Economics: The Science and Management of Sustainability*. New York: Columbia University Press.

Costanza, R., Segura, O., and Martinez-Alier, J. (eds.) (1996) *Getting Down to Earth: Practical Applications of Ecological Economics*. Washington D.C.: Island Press.

Coveney, P., and Highfield, R. (1995) *Frontiers of Complexity: The Search for Order in a Chaotic World*. New York: Fawcett Columbine.

Crichton, M. (1990) *Jurassic Park*. New York: Ballentine Books.

Daly, H. E. (1996) "Consumption: Value Added, Physical Transformation, and Welfare". In Costanza et al. 1996. pp. 49-59.

Darwin, C. (1881) *The Formation of Vegetable Mould Through the Action of Earthworms*. London: John Murray.

Dennett, D. C. (1995) *Darwin's Dangerous Idea: Evolution and the Meaning of Life*. New York: Simon and Shuster.

Depew, D. J., and Weber, B. H. (1995) *Darwinism Evolving: Systems Dynamics and the Geneology of Natural Selection*. Cambridge, MA: Bradford/The MIT Press.

de Shalit, A. (1995) *Why Posterity Matters*. New York: Routledge.

Devall, B. (1980) "The Deep Ecology Movement". *Natural Resources Journal* 20: 299-313.

Dretske, F. I. (1981) *Knowledge and the Flow of Information*. Cambridge, Mass: MIT Press.

Einstein, A. (1919) "What is the Theory of Relativity?" in Einstein, A. (1954), pp. 227-232.

Einstein, A. (1936) "Physics and Reality". In Einstein, A. (1954), pp. 290-323.

Einstein, A. (1954) *Ideas and Opinions*. New York: Crown.

Elliot, R. (1982) "Faking Nature". *Inquiry* 25: 81-93.

Elton, C. (1927) *Animal Ecology*. London: Sidgwick & Johnson.

Evans, R. B. (1969) *A Proof that Essergy* [sic] *is the Only Consistent Measure of Potential Work* [thesis]. Hannover, NH: Dartmouth College.

Faber, M., Manstetten, R., and Proops, J. (1996) *Ecological Economics: Concepts and Methods*. Brookfield, Vt.: E. Elgar.

Fitzpatrick, P., Carello, C., Schmidt, R. C., and Corey, D. (1994) "Haptic and Visual Perception of an Affordance for Upright Posture". *Ecological Psychology* 6: 265-87.

Fodor, J. (1980) "Methodological Solipsism Considered as a Research Strategy in Cognitive Psychology". *Behavioural and Brain Sciences* 3: 63-110.

Forbes, S. A. (1887) "The Lake as a Microcosm". *Bulletin Social Association of Peoria*, Illinois. 1887: 77-87.

Franklin, J. (1994) "The Formal Sciences Discover the Philosophers' Stone". *Studies in the History and Philosophy of Science* 67: 286-294.

Freese, L. (ed.) (1992) *Advances in Human Ecology*. Vol 1. London: JAI Press Inc.

Ghiselin, M. T. (1987) "Species Concepts, Individuality, and Objectivity". *Biology and Philosophy* 2: 127-44.

Gibson, J. J. (1950) *The Perception of the Visual World*. Boston: Houghton Mifflin.

Gibson, J. J. (1966) *The Senses Considered as Perceptual Systems*. Boston: Houghton Mifflin.

Gibson, J. J. ([1979] 1986) *The Ecological Approach to Visual Perception*. Boston: Houghton Mifflin.

Glansdorff, P. and Prigogine, I. (1971) *Thermodynamic Theory of Structure, Stability, and Fluctuations*. New York: Wiley-Interscience.

Gleick, J. (1987) *Chaos: Making a New Science*. New York: Viking Penguin Inc.

Godfrey-Smith, P. (1996) *Complexity and the Function of Mind in Nature*. New York: Cambridge University Press.

Golley, F. B. (1987) "Deep Ecology from the Perspective of Ecological Science". *Environmental Ethics* 9: 45-55.

Good, J., and Still, A. (1989) "Ecological Psychology as a Theory of Social Cognition". In A. Gellatly, D. Rogers, and J. Sloboda (eds.), *Cognition and Social Worlds*. New York: Oxford University Press.

Goodpaster, K. E. (1978) "On Being Morally Considerable". *The Journal of Philosophy* 75 (6): 308-325.

Gotelli, N. J. (1995) *A Primer of Ecology*. Sunderland, Mass: Sinauer Associates Inc.

Grandy, R. E. (1987) "Information-Based Epistemology, Ecological Epistemology and Epistemology Naturalized". *Synthese* 70: 191-203.

Grant, P. (1986) *Ecology and Evolution of Darwin's Finches*. Princeton: Princeton University Press.

Grinnell, J. (1917) "The Niche Relations of the California Thrasher". *Auk* 34: 364-82.

Griesemer, J. R. (1992) "Niche: Historical Perspectives". In E. F. Keller and E. A. Lloyd (eds.), *Keywords in Evolutionary Biology*. pp. 231-40. Cambridge, MA: Harvard University Press.

Grove, R. (1990) "The Origins of Environmentalism". *Nature* 345: 11-14.

Haeckel, E. (1866) *Generelle Morphologie der Organismen: Allgemaine Griendzge der organischen Formen-weissenschaft, mechanisch begründet durch die von Charles Darwin reformite Descendenz-Theorie,* 2 vols. Berlin: Reimer.

Hagen, J. B. (1989) "Research Perspectives and the Anomalous Status of Modern Ecology". *Biology and Philosophy* 4: 433-55.

Haken, H. (1977) *Synergetics: An Introduction.* New York: Springer-Verlag.

Hannon, B. (1973) "The Structure of Ecosystems". *Journal of Theoretical Biology* 41: 535-46.

Harper, J. L. (1977) "The Contributions of Terrestrial Plant Studies to the Development of Ecological Theory" in C. E. Goulden (ed) *Changing Scenes in the Natural Sciences,* 1776-1976. Philadelphia: Academy of Natural Sciences.

Haskell, E. F. (1940) "Mathematical Systematization of 'Environment', 'Organism' and 'Habitat'" *Ecology* 21: 1-16.

Hellman, G. (1996) "Structualism Without Structures". *Philosophia Mathematica* 3: 100-123.

Higashi, M., and Burns, T. P. (eds.) (1991) *Theoretical Studies of Ecosystems: The Network Perspective.* Cambridge: Cambridge University Press.

Higashi, M., Burns, T. P., and Patten, B. C. (1989) "Food Network Unfolding: An Extension of Trophic Dynamics For Applications to Natural Ecosystems". *Journal of Theoretical Biology* 140: 243-61.

Higashi, M, and Patten, B. C. (1989) "Dominance of Indirect Causality in Ecosystems". *American Naturalist* 133: 288-302.

Higashi, M., Patten, B. C., and Burns, T. P. (1991) "Network Trophic Dynamics: An Emerging Paradigm in Ecosystem Ecology". In M. Higashi and T. P. Burns (eds) *Theoretical Ecosystem Ecology: The Network Perspective.* Cambridge: Cambridge University Press.

Hirsch, M. W., and Smale, S. (1974) *Differential Equations, Dynamical Systems, and Linear Analysis.* New York: Academic Press.

Hooker, C. A. (1995) *Reason, Regulation and Realism: Toward a Regulatory Systems Theory of Reason and Evolutionary Epistemology*. New York: SUNY Press.

Hull, D. L. (1978) "A Matter of Individuality". *Philosophy of Science* 45: 335-60.

Hull, D. L. (1987) "Geneological Actors in Ecological Roles". *Biology and Philosophy* 2: 168-83.

Hutchinson, G. E. (1957) "Concluding Remarks". *Cold Spring Harbor Symposium on Quantitative Biology* 22: 415-27.

Iberall, A. (1972) *Toward a General Science of Viable Systems*. New York: McGraw-Hill.

Johnson, L. (1995) "The Far-from-Equilibrium Ecological Hinterlands" in Patten and Jorgensen 1995. pp. 51-103.

Johnson, L. E. (1991) *A Morally Deep World: An Essay on Moral Significance and Environmental Ethics*. New York: Cambridge University Press.

Jones, C. G., and Lawton, J. M. (eds) (1995) *Linking Species and Ecosystems*. New York: Chapman and Hall.

Jørgensen, S. E. (1997) *Integration of Ecosystem Theories: A Pattern*. Second Revised Edition. Dordrecht: Kluwer Academic Publishers.

Jørgensen, S. E., and Meyer, H. (1979) "A Holistic Approach to Ecological Modelling". *Ecological Modelling* 7: 169-89.

Kadar, E., and Effken, J. (1994) "Heideggerian Meditations on an Alternative Ontology for Ecological Psychology: A Response to Turvey's (1992) Proposal". *Ecological Psychology* 6(4): 297-341.

Katz, E. (1997) *Nature as Subject: Human Obligation and Natural Community*. Lanham: Rowman and Littlefield.

Kay, B. (1988) "The Dimensionality of Movement Trajectories and the Degrees of Freedom Problem: A Tutorial". *Human Movement Science* 7: 343-64.

Kekes, J. (1988) *The Examined Life*. University Park, PA: Pennsylvania State University Press.

Kelso, J. A. S., and Schoner, G. (1988) "Self-Organization of Coordinative Movement Pattern". *Human Movement Science* 7: 27-46.

Kitcher, P. (1983) *The Nature of Mathematical Knowledge*. New York: Oxford University Press.

Krebs, C. J. (1978) *Ecology: The Experimental Analysis of Distribution and Abundance*, Second Edition. New York: Harper & Row.

Kolasa, J. and Pickett, S. T. A. (1992) "Ecosystem Stress and Health: An Expansion of the Conceptual Basis" *Journal of Aquatic Ecosystem Health* 1: 7-13.

Kolasa, J., and Pickett, S. T. A. (eds.) (1991) *Ecological Heterogeneity*. New York: Springer-Verlag.

Kugler, P., Kelso, J. A. S., and Turvey, M. T. (1980) "On the Concept of Coordinative Structures as Dissipative Structures: I. Theoretical Lines of Convergence. In G. E. Stelmach and J. Requin (ed.). *Tutorials in Motor Behaviour*. Amsterdam: North Holland Publishing Co.

Kugler, P., and Turvey, M. T. (1987) *Information, Natural Law, and the Self-Assembly of Rhythmic Movement*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Kugler, P., and Turvey, M. T. (1988) "Self-Organization, Flow Fields, and Information". *Human Movement Science* 7: 97-129.

Lee, D. N. (1980) "The Optic Flow Field: The Foundation of Vision". *Philosophical Transactions of the Royal Society* B290: 169-79.

Lee, D. N. (1981) "Plummeting Gannets: A Paradigm of Ecological Optics". *Nature* 293: 293-94.

Lee, D. N., Reddish, P., and Rand, D. T. (1991) "Aerial Docking by Hummingbirds". *Naturwissenschaften* 78: 526-27.

Lee, D. N., Davies, M., Green, P., and van der Weel, F. (1993) "Visual Control of Velocity of Approach by Pigeons When Landing". *Journal of Experimental Biology* 180: 85-104.

Lehmann, S. (1981) "Do Wildernesses Have Rights?". *Environmental Ethics* 3: 131.

Leopold, A. (1949) *A Sand County Almanac*. New York: Oxford University Press.

Lewin, R. (1992) *Complexity: Life at the Edge of Chaos*. New York: Macmillan.

Likens, G. E. (1992) *Excellence in Ecology, 3: The Ecosystem Approach: Its Use and Abuse*. Oldendorf/Luhe, Germany: Ecology Institute.

Lombardo, T. J. (1987) *The Reciprocity of Perceiver and Environment: The Evolution of James J. Gibson's Ecological Psychology*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Lotka, A. J. (1922) "Contribution to the Energetics of Evolution". *Proceedings of the National Academy of Science* 8: 147-55.

Lotka, A. J. (1925) *Principles of Physical Biology*. Baltimore: Williams & Wilkins.

Lovelock, J. E. (1988) *The Ages of Gaia*. New York: W. W. Norton.

Ludwig, D., Holling. C, and Jones, D. (1978) "Qualitative Analysis of Insect Outbreak Systems: The Spruce Budworm and Forest". *Journal of Animal Ecology* 47: 315-332.

Maelzer, D. A. (1965) "Environment, Semantics, and System Theory in Ecology" *Journal of Theoretical Biology* 8: 395-402.

MacArthur, R. H. (1968) "The Theory of the Niche". In R. C. Lewontin (ed.), *Theoretical and Mathematical Biology*. pp. 159-76. New York: Blaisdell.

Margalef, R. (1968) *Perspectives in Ecological Theory*. Chicago: University of Chicago Press.

Marr, D. (1982) *Vision*. San Francisco: W. H. Freeman.

Martien, P., Pope, S. C., and Shaw, R. S. (1985) "The Chaotic Behaviour of a Leaky Faucet". *Physics Review Letters* A 110: 399-404.

Mason, H. L., and Langenheim, J. H. (1957) "Language Analysis and the Concept of Environment" *Ecology* 38: 325-340.

Maturana, H., and Varela, F. (1987) *Tree of Knowledge*. Shambala

Mayr, E. (1975) *Evolution and the Diversity of Life*. New York: Harvard University Press

McIntosh, R. P. (1987) "Pluralism in Ecology" *Annual Review of Ecology and Systematics* 18: 321-341.

McFarland, D. J. (1996) "Animals as Cost-Based Robots". In M. Boden (ed) *The Philosophy of Artificial Life*. pp. 179-205.

McLarty, C. (1993) "Numbers Can Be Just What They Have To". Nous 27: 487-98.

Meadows, D. H., Meadows, D. L., Randers, J., and Behrens, W. W. (1972) *The Limits to Growth: A Report to the Club of Rome's Project on the Predicament of Mankind*. 2nd Ed. New York: Universe Books.

Merchant, C. (ed.) (1994) *Ecology*. Atlantic Highlands, NJ: Humanities Press International Inc.

Michaels, C. F., Carello, C. (1981) *Direct Perception*. Englewood Cliffs, NJ: Prentice Hall.

Miller, A. (1978) *A Planet to Choose: Value Studies in Political Ecology*. New York: Pilgrim Press.

Millikan, R. G. (1984) *Language, Thought, and Other Biological Categories*. Cambridge: MIT Press.

Moran, E. F. (ed.) (1990) *The Ecosystem Approach in Anthropology: From Concept to Practice*. Ann Arbor: University of Michigan Press.

Murdoch, I. (1970) *The Sovereignty of Good*. London: Routledge & Kegan Paul.

Myers, N. and Simon, J. (1994) *Scarcity or Abundance? A Debate on the Environment*. New York: W. W. Norton.

Naess, A. (1973) "The Shallow and The Deep, Long-Range Ecology Movement: A Summary". *Inquiry* 16: 95-100.

Nicolis, G., and Prigogine, I. (1989) *Exploring Complexity: An Introduction*. New York: W. H. Freeman.

Noble, W. (1981) "Gibsonian Theory and the Pragmatist Perspective". *Journal for the Theory of Social Behaviour* 11: 65-85.

Norton, B. (1991) *Toward Unity Among Environmentalists*. New York: Oxford University Press.

Niven, B. S. (1982) "Formalization of the Basic Concepts of Animal Ecology" *Erkenntnis* 17: 307-320.

O'Connor, J. (1991) "Socialism and Ecology". *Capitalism, Nature, Socialism* 2 (3): 1-12.

Odum, E. P. (1969) "The Strategy of Ecosystem Development". *Science* 164: 262-70.

Odum, E. P. (1971) *Fundamentals of Ecology*, Third Edition. Philadelphia: Saunders.

Odum, H. T., and Pinkerton, R. C. (1955) "Time's Speed Regulator: The Optimum Efficiency for Maximum Power Output in Physical and Biological Systems". *American Scientist* 43: 331-43.

Odum, H. T. (1983) *Systems Ecology: An Introduction*. New York: John Wiley & Sons.

Odum, H. T. (1995) "Self-Organization and Maximum Empower". In C. A. S. Hall (ed.) *Maximum Power: The Ideas and Applications of H. T. Odum*. Niwot, CA: University Press of Colorado. pp. 311-31.

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

Onsagar, L. (1931) "Reciprocal Relations in Irreversible Processes". I. *Physics Review* 37: 405-26.

Owen, J. (1980) *Feeding Strategies*. Chicago: University of Chicago Press.

Oyama, S. (1985) *The Ontogeny of Information: Developmental Systems and Evolution*. New York: Cambridge University Press.

Pahl-Wostl, C. (1995) *The Dynamic Nature of Ecosystems: Chaos and Order Entwined*. New York: John Wiley & Sons.

Parker, G. and Maynard Smith, J. (1990) "Optimality Theory in Evolutionary Biology", *Nature* 348: 27-33.

Pattee, H. H. (ed.) (1973) *Hierarchy Theory: The Challenge of Complex Systems*. New York: Braziller.

Patten, B. C. (1978) "Systems Approach to the Concept of Environment" *American Zoologist* 21: 845-52.

Patten, B. C. (1982) "Environs: Relativistic Elementary Particles for Ecology" *American Naturalist* 119: 179-219.

Patten, B. C. (1985) "Energy Cycling in the Ecosystem". *Ecological Modeling* 28: 1-71.

Patten, B. C. (1991) "Network Ecology: Indirect Determination of the Life-Environment Relationship in Ecosystems". In Higashi and Burns (eds.) *Theoretical Studies of Ecosystems: The Network Perspective.*

Patten, B. C., and Auble, G. T. (1981) "System Theory of the Ecological Niche". *American Naturalist* 117: 893-922.

Patten, B. C., Bosserman, R. W., Finn, J. T., and Case, W. G. (1976) "Propagation of Cause in Ecosystems". In *Systems Analysis and Simulation in Ecology*, B. C. Patten (ed.), vol. 4, pp. 457-579. New York: Academic Press.

Patten, B. C., and Jørgensen, S. E. (eds) (1995) *Complex Ecology: The Part-Whole Relation in Ecosystems*. Englewood Cliffs, NJ: Prentice Hall PTR.

Patten, B. C., and Odum, E. P. (1981) "The Cybernetic Nature of Ecosystems". *American Naturalist* 118: 886-95.

Patten, B. C., Straskraba, M, and Jørgensen, S. E. (1997) "Ecosystems Emerging: 1. Conservation". Ecological Modelling (in press).

Pepper, D. (1996) *Modern Environmentalism: An Introduction*. London: Routledge.

Peters, R. H. (1991) *A Critique for Ecology*. Cambridge: Cambridge University Press.

Piaget, J. (1971) *Mental Imagery in the Child: A Study of the Development of Imaginal Representation*. New York: Basic Books.

Pickett, S. T., Kolasa, J., and Jones, C. G. (1994) *Ecological Understanding: The Nature of Theory and the Theory of Nature*. New York: Academic Press.

Pierce, G. J., Ollason, J. G. (1987) "Eight Reasons Why Optimal Foraging Theory is a Complete Waste of Time". *Oikos* 49: 111-25.

Plumwood, V. (1991) "Nature, Self, and Gender: Feminism, Environmental Philosophy, and the Critique of Rationality". *Hypatia* 6.

Port, R. F., and van Gelder, T. (1995) *Mind as Motion: Explorations in the Dynamics of Cognition*. Cambridge: MIT Press.

Prigogine, I. (1947) *Etude Thermodynamique des Processus Irreversibles*. London: Pitman.

Prigogine, I. (1980) *From Being to Becoming: Time and Complexity in the Physical Sciences*. San Francisco: W. H. Freeman & Co.

Reed, E. S. (1996) *Encountering the World: Toward an Ecological Psychology*. New York: Oxford University Press.

Regan, T. (1983) *The Case for Animal Rights*. Berkeley: University of California Press.

Reichle, D. V., O'Neill, R. V., and Harris, W. F. (1975) "Principles of Energy and Material Exchange in Ecosystems" in W. H. van Dobben and R. H. Lowe-McConnell (eds) *Unifying Concepts in Ecology*. pp. 27-29. The Hague: Dr W. Junk.

Resnick, M. (1981) "Mathematics as a Science of Patterns: Ontology and Reference". *Nous* 15: 529-550.

Richards, O. W. (1939) "An American Text Book". Book review of A. S. Pearse, *Animal Ecology*. *Journal of Animal Ecology* 8: 387-88.

Rolston, H. (1993) "Challenges in Environmental Ethics", in Zimmerman et al. (eds.) *Environmental Philosophy: From Animal Rights to Radical Ecology*. Englewood Cliffs, NJ: Prentice Hall.

Russell, P. (1991) *The Awakening Earth: The Global Brain*. London: Arkana.

Saarinen, E. (ed.) (1982) *Conceptual Issues in Ecology*. Boston: Kluwer.

Salthe, S. (1994) *Evolution and Development*. Cambridge, MA: MIT Press.

Sanders, J. T. (1993) "Merleau-Ponty, Gibson, and the Materiality of Meaning". *Man and World* 26: 287-302.

Sanders, J. T. (1997) "An Ontology of Affordances". *Ecological Psychology* 9(1): 97-112.

Sayre, K. M. (1976) *Cybernetics and the Philosophy of Mind*. London: Routledge and Kegan Paul.

Schmidt, R., Carello, C., and Turvey, M. T. (1990) "Phase Transitions and Critical Fluctuations in the Visual Coordination of Rhythmic Movements Between People". *Psychological Review* 94: 84-106.

Schneider, E. D., and Kay, J. J. (1994) "Life as a Manifestation of the Second Law of Thermodynamics". *Math. Comput. Modelling* 19: 25-48.

Scriven, T. (1997) *Wrongness, Wisdom, and Wilderness: Toward a Libertarian Theory of Ethics and the Environment*. Albany, NY: SUNY Press.

Sesardic, N. (1995) "Recent Work on Human Altruism and Evolution". *Ethics* 106: 128-57.

Shannon, C. E., and Weaver, W. (1949) *The Mathematical Theory of Communication*. Urbana: University of Illinois Press.

Shaw, R. (1987) "Behaviour with a Purpose". *Contemporary Psychology* 32: 243-45.

Shaw, R. and Kinsella-Shaw, J. (1988) "Ecological Mechanics: A Physical Geometry of Intentional Constraints. In P. Kugler (ed.), *Self-Organization in Biological Workspaces*. Berlin: Springer-Verlag.

Shaw, R., Kugler, P., and Kinsella-Shaw, J. (1990) "Reciprocities of Intentional Systems". In R. Warren and A. Wertheim (eds.), *Perception and Control of Self-Motion*. pp. 579-619. Hillsdale, NJ: Lawrence Erlbaum.

Shaw, R. E., Flascher, O. M., and Kadar, E. E. (1995) "Dimensionless Invariants for Intentional Systems: Measuring the Fit of Vehicular Activities to Environmental Layout". In J. Flach, P. Hancock, J. Caird, and K. Vicente (eds.), *Global Perspectives on the Ecology of Human-Machine Systems*, Vol I. pp. 293-357. Hillsdale, NJ: Lawrence Erlbaum.

Shrader-Frechette, K., and McCoy, E. D. (1993) *Method in Ecology: Strategies for Conservation*. New York: Cambridge University Press.

Simberloff, D. (1980) "A Succession of Paradigms in Ecology: Essentialism to Materialism and Probabilism" *Synthese* 43: 3-39.

Simone, J. L. (1981) *The Ultimate Resource*. Princeton: Princeton University Press.

Simon, J. L. (1996) *The Ultimate Resource 2*. Princeton: Princeton University Press.

Singer, P. (1990) *Animal Liberation*, 2nd Ed. New York: Avon Books.

Sober, E. (1980) "Evolution, Population Thinking, and Essentialism". *Philosophy of Science* 47: 350-83.

Solomon, H. Y. and Turvey, M. T. (1988) "Haptically Perceiving the Distance Reachable with Hand-Held Objects". *Journal of Experimental Psychology: Human Perception and Performance* 14: 404-27.

Steiner, D. and Nausser, M. (eds.) (1993) *Human Ecology: Fragments of Anti-Fragmentary Views of the World*. London: Routledge.

Sterelny, K. (1990) *The Representational Theory of Mind*. Oxford: Basil Blackwell.

Swenson, R. (1991) "Oder, Evolution, and Natural Law: Fundamental Relations in Complex Systems Theory". In C. Negoita (ed.), *Cybernetics and Applied Systems*. pp. 125-48. New York: Marcel Dekker.

Swenson, R. (1997) "Evolutionary Theory Developing: The Problem(s) with *Darwin's Dangerous Idea*". *Ecological Psychology* 9(1): 47-96.

Tansley, A. G. (1935) "The Use and Abuse of Vegetational Concepts and Terms" *Ecology* 16: 284-307.

Taylor, P. W. (1981) "The Ethics of Respect for Nature". *Environmental Ethics* 3: 197-218.

Taylor, W. P. (1939) "What is Ecology and What Good is It?" *Ecology* 17: 333-46.

Thero, D. P. (1995) "Rawls and Environmental Ethics" *Environmental Ethics* 17 (1): 93-106.

Tilly, L. J. (1968) "The Structure and Dynamics of Cone Spring". *Ecological Monographs* 38: 169-197.

Turvey, M. T. (1992) "Affordances and Prospective Control: An Outline of the Ontology". *Ecological Psychology* 4: 173-87

Turvey, M. T., and Carello, C. (1995a) "Dynamic Touch". Chapter 11 in W. Epstein and S. Rogers (ed). *Perception of Space and Motion*. San Diego: Academic Press Inc. pp. 401-490.

Turvey, M. T., and Carello, C. (1995b) "Some Dynamical Themes in Perception and Action". In Port and van Gelder, *Mind as Motion: Explorations in the Dynamics of Cognition*. pp. 373-401.

Turvey, M. T., and Shaw, R. E. (1979) "The Primacy of Perceiving: An Ecological Reformulation of Perception for Understanding Memory". In L-G Nilsson (ed.) *Perspectives on Memory Research: Essays inHonour of Uppsala University's 500th Anniversary*. Hillsdale, NJ: Laurence Erlbaum and Associates.

Ulanowicz, R. E. (1986) *Growth and Development: Ecosystems Phenomenology*. New York: Springer-Verlag.

Ulanowicz, R. E. (1997) *Ecology, The Ascendent Perspective*. New York: Columbia University Press.

Van DeVeer, D., and Pierce, C. (eds.) (1998) *The Environmental Ethics and Policy Book: Philosophy, Ecology, Economics*, 2nd Edition. Belmont, CA: Wadsworth.

Van Dyne, G. (1969) *The Ecosystem Concept in Natural Resource Management*, Chapt. X. New York: Academic Press.

van Gelder, T., and Port, R. F. (1995) "It's About Time: An Overview of the Dynamical Approach to Cognition". In Port and van Gelder (1995) *Mind as Motion: Explorations in the Dynamics of Cognition*. pp. 1-44.

Van Valen, L. M. (1976) "Energy and Evolution". *Evolutionary Theory* 7: 93-106.

von Bertalanffy, L. (1968) *General System Theory: Foundations, Development, Applications*. New York: Braziller.

Von Neumann, J. (1958) *The Computer and the Brain*. New Haven: Yale University Press.

Wagner, H. (1982) "Flow Field Variables Trigger Landing in Flies". *Nature* 297: 147-48.

Warren, W. H., Young, D. S., and Lee, D. N. (1986) "Visual Control of Step Length during Running Over Irregular Terrain". *Journal of Experimental Psychology: Human Perception & Performance* 12: 259-66.

Warren, K. J. (1990) "The Power and Promise of Ecological Feminism". *Environmental Ethics* 12 (2): 125-146.

Weber, B. H., Depew, D. J., Dyke, C., Salthe, S. N., Schneider, E. D., Ulanowicz, R. E., and Wicken, J. S. (1990) "Evolution in Thermodynamic Perspective: An Ecological Approach". *Biology and Philosophy* 5: 374-405.

Weber, B. H., and Depew, D. J. (1996) "Natural Selection and Self-Organization: Dynamical Models as Clues to a New Evolutionary Synthesis". *Biology and Philosophy* 11: 33-65.

Weigert, R. G. (1988) "The Past, Present, and Future of Ecological Energetics". In L. R. Pomeroy and J. J. Alberts (eds.) *Concepts of Ecosystem Ecology: A Comparative View*. pp. 29-55. New York: Springer-Verlag.

Westra, L. (1994) *An Environmental Proposal for Ethics: The Principle of Integrity*. Lanham, MI: Rowman & Littlefield.

Wheeler, M. (1996) "From Robots to Rothko". In M. Boden (ed) *The Philosophy of Artificial Life*. pp. 209-36.

Wimsatt, W. C. (1994) "The Ontology of Complex Systems: Levels of Organization, Perspectives, and Causal Thickets". *Canadian Journal of Philosophy*. Supplemental Volume 20.

Wittbecker, A. E. (1990) "Metaphysical Implications from Physics and Ecology". *Environmental Ethics* 12: 275-82.

Wuketits, F. M. (1990) *Evolutionary Epistemology and Its Implications for Humankind*. Albany, NY: SUNY Fress.

Yepez, N. (1989) "Chaos in a Dripping Faucet" *European Journal of Physics* 10: 99-105.

Zimmerman, M. E., Callicott, J. B., Sessions, G., Warren, K, and Clark, J. (eds.) (1998) *Environmental Philosophy: From Animal Rights to Radical Ecology*, 2nd Edition. Upper Saddle River, NJ: Prentice Hall.

IMAGE EVALUATION TEST TARGET (QA-3)

150mm

6"

APPLIED IMAGE . Inc
1653 East Main Street
Rochester, NY 14609 USA
Phone: 716/482-0300
Fax: 716/288-5989