2475/3926

MONASH UNIVERSITY
THESIS ACCEPTED IN SATISFACTION OF THE
REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY
ON.........[redacted]...........
....

Sec. Research Graduate School Committee

Thinking Locally

A disunified methodology of science

Jeremy P. Aarons

B.Sc (Hons.), B.A. (Hons.) (Monash)

Submitted for the degree of PhD.

Department of Philosophy, Monash University.

30 March 2001

For I am building in the human understanding a true model of the world, such as it is in fact, not such as a man's own reason would have it to be; a thing which cannot be done without a very diligent dissection and anatomy of the world. But I say that those foolish and apish images of worlds which the fancies of men have created in philosophical systems must be utterly scattered to the winds.

Francis Bacon, *The New Organon*, Book 1, CXXIV
(Translated by Spedding, Ellis & Heath)

"The Hedgehog and the Fox", illustrated by Michael Ayrton
(Ayrton 1977)

Table of Contents

Summary

This thesis is a defence of the validity and importance of bottom-up, *localised*, methods in science. By a *localised methodology* I mean a scientific methodology that is concerned with understanding and revealing the entities and causal relations in a particular situation, with a particular problem at hand, and thus particular explanatory or predictive aims. Instead of appealing to generalisations or laws in explanatory schemata, a localised methodology is based on the uncovering and modelling of local causal and stochastic processes, involving locally specified capacities, properties and kinds. Accepting the validity of such approaches allows scientific discovery to be achieved via process-driven modelling, from the bottom-up, rather than via models structured from the top-down. A localised methodology can deliver powerful predictions and detailed explanations, by rejecting the central importance of fundamental laws, being open to the possibility of disunity, and focussing on solving particular problems in particular contexts rather than developing generally applicable theories.

My views are motivated by taking seriously the knowledge acquired in theories and modelling in biology, especially conservation ecology. Such complex sciences do not fit the standard view that philosophers have of science, a view that has been dominated by physics as a paradigm of 'hard' science. Yet there are many successful methods being used in conservation ecology today, essentially based on bottom-up, localised modelling. The example I focus on is Population Viability Analysis (PVA), a technique designed to determine the survival possibilities for a particular endangered species.

In defending a localised methodology I make use of many of Nancy Cartwright's insights concerning the role of causation in scientific modelling. I apply these insights to the PVA approaches to conservation ecology. Through such an investigation I scrutinise and clarify Cartwright's views on modelling, causation, and laws of nature. In doing so I show how Cartwright's antirealism about fundamental laws, and her claim that modelling and experiment allow us to uncover the causal capacities in nature, fit extremely well with the bottom-up modelling approaches I endorse.

Declaration

This thesis contains no material which has been accepted for the award of any other degree or diploma in any university or any other institution; and to the best of my knowledge it contains no material previously published or written by another person except where due reference is made in the text.

Signed: [redacted]

WITH COMPLIMENTS

MONASH UNIVERSITY

Research Services

Signature:

Originals Sighted By: Date:
[redacted] 7.11.02
MONASH RESEARCH GRADUATE SCHOOL
Research Services

MONASH RESEARCH GRADUATE SCHOOL
PO Box 3A
Monash University
Victoria 3800, Australia
Telephone: +61 3 9905 3009
Facsimile: +61 3 9905 5042
Email: mrgs@adm.monash.edu.au
www.monash.edu.au/phdschol

Acknowledgements

Many wonderful and generous people have helped influence, assist and encourage me in completing this thesis.

Firstly, I wish to express thanks to the late John McGechie, who inspired me during my undergraduate years, and always encouraged me to continue further study in the philosophy of science. Without his encouragement I may never have returned.

I owe an enormous debt of gratitude to John Bigelow, who supervised my work for the majority of my candidature. John has provided me not only with immense encouragement and support, but has also been a wonderful role model as a dedicated, enthusiastic, and tirelessly inspirational intellectual. I also owe a huge thanks to Graham Oppy, who acted as my supervisor for the final stages of writing up. While this project would not have begun without John Bigelow, it wouldn't have been completed without Graham's kind and supportive help. Jo Asscher was also of invaluable help, providing an extremely detailed commentary and criticism of Chapter Four. Also a huge thanks to Kerry Wardlaw and Jacqui Broad, who both conscientiously proof-read the entire thesis.

Numerous other people have also helped inspire and clarify my ideas. In particular, Dale Jamieson led me into the philosophy of ecology, Kim Sterelny dazzled me with the philosophy of biology, and Steve Clarke introduced me to the work of Cartwright and Duhem. The environment within the Department of Philosophy at Monash University has been a most stimulating one, with many people providing me with helpful feedback. In particular I would like to thank Richard Holton, Karen Green, and Dirk Baltzly for their contributions. I must also thank Cassandra Star, who encouraged me to participate in the Ecopolitics XI conference at Melbourne University, where I had the opportunity to present my work to a diverse audience of non-philosophers. Thanks also to Hugh Possingham, both for the kind permission to use his population modelling software, and for providing me with copies of some of his recent papers, including a number of preprints.

Finally, I must thank all my supportive friends and family, especially all those who lived with me while I worked on this project. Undoubtedly, my greatest debt is to my parents and grandparents. Without their unfailing support, encouragement, and pride, this work would certainly not have been possible.

Chapter 1

Local Thinking, Ecology and Disunified Science

1. *Two Approaches to Science*

1.1 Introduction

"Think globally, act locally" goes the environmental catchcry: a potent message aimed at making us aware of the broader impact of our actions and inspiring us to act on a small scale, within our own homes and local communities, to improve the world at large. Such thinking helps us to see the importance of implementing recycling, restricting water usage, and reducing overall consumption in our day-to-day living. It helps us to look at the impact of our diets on the environment, reassess our farming techniques and to look carefully at our use of natural resources. Thinking globally we can see that the widespread consumption of meat in developed countries has a severely detrimental effect on the environment, since much of the excess land clearing worldwide is for livestock grazing. We can also see that our high consumption of seafood is having a devastating effect on marine ecosystems. To combat these problems we can act locally, switching to a more sustainable diet, reducing or even eliminating unnecessary consumption of destructive food sources such as meat, fish, and environmentally hazardous crops. Similarly, we can modify our consumption patterns, refusing to purchase rainforest timbers and boycotting corporations that are responsible for widespread environmental vandalism. We can reassess urban development, question our ever-increasing reliance on the automobile, and look at ways of limiting the urban sprawl of big cities. We can also reassess our reliance on large scale, highly polluting power generation, and look carefully at the alternatives.

Overall, as a maxim about practical and political action, "Think globally, act locally" delivers a powerful environmental message. Such a message, however, is *not* the topic of my discussion here, although much of what I say is deeply motivated by environmental concerns. For instead of ethical and political issues, I am concerned with the *epistemological* and *methodological* issues facing scientists, especially those studying biological and environmental phenomena. In this context, as a maxim about *science*, "thinking globally" is deeply problematic, especially when it comes to disciplines like the environmental sciences. When it comes to a scientific outlook on the world, thinking globally is not the best attitude to take. Instead of thinking globally, we should think *locally*.

I begin here by describing the dominant, 'global view' on science and contrasting it with what I term the 'local view'. In the second section I present evidence that the global view is indeed the dominant view on science. As part of this I argue both that its assumptions are present in significant philosophical accounts of science, and that one popular alternative to the dominant 'received' view on scientific theory, the so-called *semantic account* of scientific theories, is also commonly infected with the same problematic assumptions of the global approach. In the subsequent section I take a closer look at the question of just why the global view has such a strong intuitive appeal in mathematics and theoretical physics, looking at the significance of ideals such as simplicity, unity and elegance. I also look briefly at the state of physicists' search for a Grand Unified Theory of nature. Finally, I end with a brief overview of my positive view on science, which is explored and developed throughout the rest of this thesis.

1.2 The Dominant View: Global, Universal Science

The concept of *thinking locally* is best understood in contrast with the *global* view on scientific knowledge, which is the dominant view in contemporary philosophy of science. This global view has been endorsed explicitly by most philosophers of science, and is implicit in nearly every account of science today. For now I will briefly describe this view, leaving it until section two of this chapter to elaborate on the claim that the global view is indeed the dominant view in the philosophy of science.

The global view states that we should develop scientific theories that are universal, expressing fundamental laws of nature, and applicable independent of location in space and time. Science is seen as the quest for the ultimate rules according to which the universe runs. By use of these fundamental laws we can explain and predict events, manipulate and control nature, and discover novel facts about nature. These laws describe the fundamental workings of the world, at the level of the basic building blocks of the constituents of the world, and are applicable at any time in any place in the universe. In principle, all other higher order sciences are reducible to the language of physics, although this reduction may be somewhat messy and unworkable to our finite minds. However, on this dominant view, the realms of scientific theory are coherent as a whole and in principle expressible in one basic language –

we may be forced by our cognitive limitations to use unrealistic, idealistic theory, but we can rest assured that there is order in the universe and that if our theory works it somehow reflects that order.

More formally, the dominant view says that scientific theories are formal systems defined by the fundamental laws, which form the explanatory and predictive base of the theory. This view, sometimes termed the "received view"[1], has been developed over the last century and is most clearly expressed in the work of Carl Hempel (1948; 1962a; 1962b; 1965). Pierre Duhem (1906) argued that scientific theories are essentially logical structures, with the laws of nature forming the axioms of the theory. These laws are true generalisations that are not merely accidentally true, but are necessary: they are part of the underlying structure of the way the world must be. They are also *universal*, in that they apply anywhere in the universe, a point emphasised by Smart (1963; 1968). Hempel (1948) argued that scientific explanation is just a matter of logical deduction from these laws: knowledge of the fundamental laws plus knowledge of initial conditions gives us the explanandum by simple deduction. According to Hempel's Deductive-Nomological (D-N) model of explanation the fact to be explained, the explanandum, is explained by appealing to knowledge of certain initial conditions along with specific laws of nature, which state that the explanandum had to occur given those initial conditions:

Initial Conditions:	C
Law:	$C \rightarrow E$
Explanandum:	E

Thus, for example, we can explain why Halley's comet returns every 76 years just by citing Newton's laws of motion along with some data on the comet's velocity. Also, by appealing to facts about the Earth, Moon and Sun, and applying the laws of physics, we can explain why there are two tides per day, why there are spring and neap tides, etc. The laws of physics are extremely powerful tools, for they reveal to us the underlying patterns and symmetries of the universe, and allow us to understand a wide range of phenomena as the effects of a few simple rules. In fact, according to the D-N model of explanation a science is *only* explanatory if it contains general laws of the form $C \rightarrow E$. In this sense such an approach

[1] The term "received view" was first coined by Putnam, 1962.

can be termed *top-down*, since the overarching laws form the basis for explanation and prediction.[2]

In summary, according to this global view, scientific theories:

- are universal structures: sets of fundamental laws;
- involve universal approaches;
- are 'top-down': the fundamental laws form the explanatory base (i.e. explanation is based on deduction from fundamental laws).

The global view was developed primarily with theories in physics in mind as the paradigms of scientific theories; most philosophers of science were either physicists themselves or philosophers with a background in physics. Few considered issues arising out of the biological sciences, and those who did generally considered biology a weak science because it did not have general laws (Smart 1968) or was seen not to be clearly testable (Popper 1959). Thus the fact that the global view is biased against sciences such as biology is clear, since in biology there are few, if any, universally applicable laws of nature.[3] More importantly for my purposes, many areas of biology are not at all concerned with discovering general laws, and instead are aimed at understanding phenomena in quite specific contexts. Nevertheless, I claim, this doesn't imply that biology is theoretically weak. All it shows is that biology has a different theoretical structure to that valorised by the global view.

---

[2] Interestingly, in Hempel's (1948) formulation he is not so strict as to insist that the laws are genuine laws of nature, only that they are reasonably robust generalisations. This weaker formulation, however, doesn't alter the fact that Hempel's D-N schema is essentially a top-down approach, although it may make it more amenable to particular areas of biology in which there are reasonably robust generalisations.

[3] There are those who defend the existence of laws in biology, most notably Ruse (1973). However, candidates for biological laws are still quite rare, and whether they are genuine laws of nature is still a controversial issue. In particular, they appear to be quite different to the types of laws that have traditionally been accepted in physics (Smart 1963). Possible laws in biology can be divided into the following categories:

a) Descriptive generalisations: e.g. "Von Baer's laws of embryology" (Sterelny & Griffiths 1999, p. 365); "Cope's Law": that body size tends to increase in phyletic sequences; "Williston's Law": that large numbers of similar elements tend to be reduced to fewer differently specialised units.

b) Mathematical tautologies: e.g. "Hardy-Weinberg Law" (Sober 1993, pp.71-2): "If there are p A genes and q a genes at some locus in a population, then the frequencies of the three genotypes AA, Aa, and aa will be p^2, $2pq$, and q^2, respectively."

c) Historical laws: e.g. "Dollo's Law" (Gould 1970); 'Process' Laws (McIntyre 1997; Kauffman 1995)

However each of these categories appear to fall short of being genuine laws of nature, because they lack necessity or universality. (For more on this, including a defence of a modified conception of laws in biology, see Brandon 1997, Sober 1997, and Waters 1998.) In any case, the debate over the existence of laws in biology is one I largely sidestep in this thesis, since whether or not such laws exist is not crucial to my argument. All I emphasise is that many areas of biology do not utilise laws.

Thus I argue we should reject a picture of science that only incorporates the global view. At present there are a few who defy the norm, and I see my work here as adding to an ever-increasing body of work challenging the dominant, global view. Such a view is not only untenable for science as a whole, it is also based on what is arguably a false view of physics. For, as Cartwright argues (1983; 1989a; 1999), even the most fundamental realms of physics do not adhere very well to the global view. If Cartwright is correct then the philosophy of science surely needs a major revamp. Yet even if she is wrong about fundamental physics, there is still a clear need to revise our conception of science to accommodate other sciences: we need to admit the *local* as having genuine explanatory and predictive import.

1.3 The Local View

What I term the 'local view' of scientific theories contrasts with the global view, in that there is no emphasis on the existence of globally applicable laws in the structure of the theory. Instead, theories are merely locally applicable models based on relevant causal and stochastic processes. By a *localised methodology* I mean a scientific methodology that is concerned with understanding and revealing the entities and causal relations in a particular situation, with a particular problem at hand, and thus particular explanatory or predictive aims. Instead of an appeal to generalisations or laws in explanatory schema, a localised methodology is concerned with local causal relations between local entities and properties, with explanations appealing to (singular and complex) processes at the local level. In contrast to the top-down approach of the global view a localised methodology is *bottom-up*, since explanation and prediction are derived from base level processes and properties.

Thus, according to the local view, theories:

- are based on locally specific models derived from causal and stochastic processes;
- involve 'case study' oriented approaches (Yin 1994);
- are 'bottom-up' structures: explanations based on local causal and stochastic facts.

I clarify these ideas over the remainder of this chapter, in Chapter Two where I discuss Pierre Duhem's philosophy of science, and in later chapters where I present tangible examples to clarify the distinction between a global and a local approach to science.

Such a methodology is fundamental in many areas of investigation, such as medical research, biology and engineering, and possibly even economics, psychology, psychoanalysis and history. Here I am concerned primarily with the biological sciences, in particular with ecology, more specifically conservation ecology. But my views have far broader implications in terms of how science is viewed, and how it should be conducted. I see this work as being a defence of the methodology and scientific value of sciences that are traditionally seen as weaker than sciences such as physics. In particular, I defend the methodology of many realms of biology, concentrating specifically on the so-called 'special science' of ecology.

My starting point is to take ecology seriously as a science, to consider that the knowledge we obtain from ecology is potentially just as powerful as knowledge from other scientific disciplines, and that the methodology of ecology is as genuinely scientific as that of other sciences. If we take the theories and models of ecology seriously then it is clear that the views of standard (global) philosophy of science, concerning the nature of scientific explanation, the role of laws and generalisations, and the general aim of the enterprise of science, must be mistaken. This is because there are few, if any, laws in ecology that are analogous to the paradigm laws of physics. There are also few, if any, theories in ecology that are applicable in more than a few limited contexts, let alone globally applicable. Thus, as the philosophy of science and scientific explanation now stands, basing the strength of science in its ability to uncover laws, it appears that sciences such as ecology do not stand up very well as compared to a 'hard' science such as physics. I consider this more a problem with the philosophy of science than a problem with ecology, for biology has provided us with a wealth of important knowledge concerning the world, knowledge which in many cases seems just as real and reliable as that provided by physics. Thus the need to revise the dominant view.

The general point here is that by looking at the methodology of ecology it is clear that a conception of knowledge as being *localised* is important. This is a point I explore in Chapter Three, where I look seriously at a number of models used in ecology: in particular at a method called Population Viability Analysis (PVA) in population ecology, which provides a way of ranking various conservation strategies in a particular situation. Through this example I argue that a localised view of science is not only possible, it is just as powerful, and in many cases far more powerful, than the global, universal view of science.

In Chapter Four I apply a localised view to respond to one of the central criticisms levelled at the biological sciences. This is the claim that biological kinds are not *natural kinds* at all, but are somewhat arbitrary groupings that do not reflect any underlying division in nature. If this is the case then, at best, the theories in biology can only reflect the particular way we divide up the world to suit our particular interests, since there are no true divisions in nature at the level of our theories in biology. I respond to this by arguing that biological kinds are indeed *natural kinds*, though not in the universal sense usually associated with kind terms in physics. Here I show that a localised conception of natural kinds is not only possible, but that it is actually used in many areas of science, and is the only means of overcoming certain definitional problems, such as the definition of species.

Linked to this localised conception of natural kinds is a localised conception of causation and laws. In Chapter Five I bring these themes together by focusing on the work of Nancy Cartwright. Cartwright's approach to epistemology, with its non-Humean ontology of causal capacities and its primacy of scientific methodology as an essential part of theory, fits well not only as a justification of methodology in biology, but also as a potential unifying theme for sciences in general. Thus the pluralism I advocate concerning the types of theories within sciences, the methodologies, the explanatory aims and ontologies can be encompassed within a broadly unified view of science, which sees the business of science as discovering the causal capacities of the structures in nature.

By arguing for a localised conception of science I am not trying to fully undermine the standard global approach to science. I see the standard approach to be an important and essential part of the ragged patchwork of scientific theories. There just seems to be no scope within traditional views of scientific theories to accommodate sciences of localised knowledge. By defending a localised methodology I intend to place many research programmes on a far firmer philosophical grounding. At present, although there is much work being done in this way, such work may come under criticism for not living up to certain ideals of what a scientific theory should be. For example, conservation ecology in general may be deemed weak because it cannot come up with laws or generalisations that are useful for achieving desired aims. Although I argue this is true for a certain global approach to population conservation, such a criticism seems misplaced when aimed at a local approach to conservation ecology. Thus I am not arguing for the strong thesis that all science is local, or even that all science *should* be local, but merely that a localised methodology is on as firm a philosophical footing as the traditional view of science, and in many cases is a far more fruitful approach to particular problems. In particular, I argue that a localised methodology provides a far more fruitful approach than a global approach for dealing with problems concerned with species conservation and protection.

2. *The Dominance of the Global View*

2.1 Casting Aside the Local

The local has been cast aside in the philosophy of science, so much so that generally its absence has been barely noted. With almost all philosophy of science being motivated by physics as the exemplar of science, only global theorising has been viewed as genuine explanatory science. The physics biased philosophy of science has typically valorised the global view and entirely ignored the local view as a strong or even valid scientific methodology, classing bottom-up approaches as merely historical, non-general, contingent, particular (Smart 1963; 1968), 'weak' (Duhem 1906), and mere application *viz* engineering as opposed to true science. This "physics envy" implicit in the dominant accounts of science has relegated non-global approaches to science to being subordinate or inferior to physics, the "queen of the sciences". Even the category of "special sciences", reserved for sciences such as biology, chemistry, economics and social science, has a derogatory feel to it, not dissimilar to the way the word "special" is sometimes used to refer to intellectually handicapped people.

An example of the global bias in the philosophy of science can be seen in the work of Karl Popper. The entire framework of Popper's (1959) influential analysis of science is deeply embedded in the global view. Popper's approach essentially follows David Hume's famous rejection of induction, but with an interesting twist. The problem of induction as stated by Hume was to do with the nature of individual belief: of how we can rationally justify a belief about unknown events from observing a constant conjunction of like events. Hume concluded that we can't, that "there can be no *demonstrative* arguments to prove, *that those instances of which we have had no experience resemble those of which we have had experience.*"[4] Popper reformulated this problem to be one about the rationality of accepting scientific *laws*, moving the problem from one about ordinary knowledge to one about objective scientific knowledge of a particular type – from a problem about individual belief to a problem about theory acceptance. The problem of induction then becomes one of how

[4] Hume 1888, p. 89.

we can justify a generalization from finite experience to a scientific law. Popper states the problem as follows:

> *Are we rationally justified in reasoning from instances or from counterinstances of which we have had experience to the truth or falsity of corresponding laws, or to instances of which we have had no experience?* [5]

Popper's "solution" is that we can never justify induction as a mode of scientific inference and thus we must abandon it. He then goes on to claim that although we can never accept the truth of a scientific law, we can deductively falsify a law. Here falsification involves deductive logic: we look for the logical consequences of a law, and test whether we observe that result. For example, to test the statement that the speed of light in a vacuum is a constant invariant of any reference frame we test whether the predicted effects of time dilation occur. If we don't observe such a result we have deductively falsified the theory.

Importantly for my purposes, the entirety of Popper's discussion concerns the acceptance or rejection of scientific theory that is based on the global view. Popper's conception of falsification centres on the use of deductive logic to test *laws*. Without global laws Popper's methodology doesn't even get started, for the global laws are what are tested. In disciplines where laws or robust generalisations are few and far between there isn't much scope for applying Popper's demarcation criterion.

Of course Popper's views have been thoroughly scrutinised and criticised, with the so-called Duhem-Quine thesis spelling the end of such naïve conceptions of falsification.[6] Such criticisms, however, generally still embody the assumption that laws are the central component of scientific theory; they just disagree on what counts as corroboration or falsification. Still, some progress away from the global view was made through criticism of Popper's views. For example Thomas Kuhn (1977) goes some way to rejecting a global view, when he discusses the importance of "exemplars" and "models" in the formation of the "disciplinary matrix", a more sophisticated version of what he called a "paradigm" in his earlier work (Kuhn 1970).[7] Here Kuhn plays down the importance of lawlike generalisations, even saying that "The example of taxonomy suggests that a science can exist with few,

[5] Schilpp (ed.) 1974, p. 1020.

[6] Chalmers 1999 provides an excellent summary of these issues.

[7] See Kuhn 1977, Chapter 12, "Second Thoughts on Paradigms".

perhaps with no, such generalizations".[8] His proposal is that what drives scientific problem solving is not always the mere application of generalised rules, but may be due to what he terms "a learned perception of similarity."[9] Thus Kuhn has moved away from a strictly global view, and seems to have embraced a view that is compatible with a case-by-case approach characteristic of a bottom-up localised approach to science.

However, whether Kuhn has fully embraced the local view is difficult to judge. On one hand, he explicitly asserts that "symbolic generalizations", the term he uses to refer to universal laws, are one of the core components of the disciplinary matrix. Thus global laws do still seem to play a central role in Kuhn's view of science. But on the other hand, the role that these laws play is perhaps problem-specific to some degree, thus quite different to the role laws play in a global approach. As Hoyningen-Huene (1989) states about applying Kuhn's methodology:

> A problem situation can only fall within the intended sphere of application of a formulation of a law or theory if the empirical concepts employed in this formulation have referents in the problem situation. Furthermore, the law or theory can only be specified for a given situation, and the resulting assertions compared with observations, if the referents of the empirical concepts employed in the law or theory have actually been identified in that situation.[10]

Thus it is difficult to tell just how globally applicable Kuhn takes his "symbolic generalizations" to be, since whether a law is applicable in a given problem situation depends upon the particulars of that situation. This may be compatible with the global view if all that is at stake is the *relevance* of a law for a particular problem: on this interpretation all laws are globally valid, but whether they are applicable depends upon the problem at hand. But it presents difficulties for the global view if Kuhn's position implies that laws are only *valid* depending upon the particular context. What Kuhn fully intended is difficult to tell, especially given that Kuhn largely shied away from further discussion of "disciplinary matrix" in his later work. Thus it may have been that Kuhn was pioneering a localised view of science, but it is hard to draw a definite conclusion here. Instead I leave the ultimate interpretation of Kuhn on this issue as an open question, for that is not my project here.

---

[8] Kuhn 1977, pp. 298-9.
[9] *Ibid.*, p. 318.
[10] Hoyningen-Huene 1989, pp. 102-3.

Significantly, there are other obstacles the opponent of the global view faces, for which Kuhn's perspective can provide us with only limited help, even if we interpret him as endorsing a localised approach. For, as mentioned in section 1.2 above, the concept of scientific explanation has traditionally been linked to knowledge of global laws of nature, and the present view on what constitutes explanation is almost universally held to rely on an appeal to generalisations or laws. This is the view that all science should aspire to the same explanatory schemata as physics, as exemplified by Hempel's explanatory schema where particular matters of fact are explained as the result of fundamental laws of nature on some set of initial conditions. Given this view on scientific explanation, and given the poverty of reasonably robust generalisations, let alone exceptionless laws of nature, in many of the 'special sciences' such as biology and especially ecology, it seems that these sciences as they exist cannot explain much if we take this view of what a scientific theory is.

My approach is to reject the current picture of science and scientific explanation, to reject the global view in favour of a more flexible approach that incorporates *localised* science. It may be true that theoretical structures in physics (and other so-called 'hard' sciences) do tend to fit the global view better than the local view. However, given the strengths and successes of various approaches to biology that do not fit the global view, there is clearly a desperate need to revise the philosophy of science to accept the local view as giving us genuine knowledge. Philosophy shouldn't *decide* what good science is, it should *describe* what good science is. This is not to say that the philosophy of science can't be normative. The point here is that the normative claims in the philosophy of science should respect and reflect the successful methods of good science. This is akin to the "naturalistic" approach to the philosophy of science taken by Ronald Giere, as exemplified in his discussion of the problem of theory choice in science:

> All of [the previous] approaches [to scientific theory choice] assume the more general principle of rationality that scientists generally strive to make a rational choice, however this is defined. Other than this general principle, philosophical accounts of theory choice make scant reference to the actual flesh-and-blood scientists who do the choosing. The approach is almost always "top down". A naturalistic approach to theory choice is explicitly "bottom up". It begins with real agents facing various choices in the course of their actual scientific lives. It assumes that choosing theories is not too dissimilar from choosing anything else, and then looks at how humans in fact make choices.[11]

---

[11] Giere 1999, pp. 169-70.

Looking at how scientists actually work, the tools they use, and the basis for the judgements they make is especially important when we consider relatively young sciences such as ecology, together with the relatively new methods of scientific analysis that have become available to us with the very recent advances in technology. Our ways of looking at the complexity of the world have changed, through the developments of probability and statistics, and with the massive explosion of computational power made possible by the microprocessor revolution. We now no longer need to develop elegant and simple theories to understand and manipulate the world, for we can represent vastly complex systems in more and more accurate ways through computer modelling. This has not only changed the way science is done, with the old-style laboratory of bells and smells being replaced by the virtual laboratory, but has changed what science can hope to achieve. In virtual laboratories we can now not only perform experiments which are too hazardous or ethically problematic in reality, such as nuclear fission experiments[12], but we can perform experiments that would be otherwise impossible, such as looking at the long-term effects of global catastrophes.[13]

The power of these methods, which has only begun to be tapped, presents a real challenge to the view of science still largely dominant amongst philosophers, for these methods are not simple or elegant. In fact the methods and models of "virtual science" are proliferating at an extraordinary rate, becoming more and more complex, and if anything, are increasing the barriers between different realms of science rather than unifying science. Clearly science is not what it used to be. Yet the ideas of the past are still dominant amongst philosophers of the present, who always take a while to catch up to what the scientists have been doing and saying.

Over recent years some philosophers have attempted to look closely at more contemporary science. They are largely philosophers of science who have been concerned with the actual grubby practice of science, as it is done in the laboratory and in the field. These so-called "new experimentalists"[14] look closely at the nature of experiment, and are more concerned with scientific rationality in the small, local, experimental context rather than in the broad theoretical sense that dominated philosophy of science in the 1960s and 1970s (as found in the work of Popper, Kuhn, Lakatos and Feyerabend). The approach is exemplified in the

[12] With the use of Monte Carlo simulations, as discussed by Galison 1987, p. 265, and in more detail in Galison 1997, pp. 689-780.

[13] These and other examples were presented by Professor Mary O'Kane at the *Lady Masson Memorial Lecture*, October 1997, University of Melbourne.

[14] A term introduced by Ackermann, 1989.

work of Hacking (1983), Cartwright (1983, 1989a, 1999), Galison (1987, 1997) and is discussed in Ackermann (1989), Mayo (1996) and Chalmers (1999). Yet, interestingly, these writers still focus their investigations on physics, and have little or nothing to say about biology in particular, something I intend to make up for here. However, before launching into that project I wish to take a closer look at the dominance of the global view. In the next section I present another interesting and significant piece of evidence that the global view has quite deep roots, even amongst philosophers who perhaps should have good reason to question such a view.

2.2 Philosophy of Science on Trial

> *Q:* *What is science?*
>
> *A:* *Science is an attempt to understand the physical world, primarily through law, that is, through unbroken natural regularity.*
>
> Michael Ruse, under cross-examination during the 1981 Arkansas trial over the constitutional validity of Act 590. [15]

One significant piece of evidence for the dominance of the global view comes from the legal case *McLean v. Arkansas*. On March 19th 1981 the Governor of Arkansas signed into law Act 590, "The Balanced Treatment for Creation-Science and Evolution-Science Act". This act required that all teachers in public schools in the state of Arkansas must talk of Creation-science as well as evolution when discussing the origins of life. On May 27th 1981 a group of twenty-three Arkansas citizens and organizations mounted a challenge to the Act's constitutional validity in the U.S. District Court. The *McLean v. Arkansas* trial began on December 7th 1981 and lasted two weeks, with the court's decision handed down on January 5th 1982. In his *Opinion*[16] the presiding judge, William R. Overton, held Act 590 to be unconstitutional, a violation of the 'establishment of religion' clause of the First Amendment that guarantees the separation of Church and State.[17]

Judge Overton's *Opinion* was largely influenced by the testimony of Michael Ruse, one of the most prominent and respected philosophers of biology. Ruse has written widely on

[15] Ruse, M. "A Philosopher's Day in Court". In Ruse (ed.) 1988, p. 26.

[16] Overton 1982.

[17] For details and commentary on the case see La Follette (ed.) 1983 and Ruse (ed.) 1988.

conceptual issues in biological theory, and is the founding editor of the journal *Biology and Philosophy*, the major journal in the field. As such he was called as an expert witness by the plaintiffs for their case against the so-called "balanced treatment" for creation-science. As part of his testimony Ruse was asked to define what science is. The aim of this question was to show that so-called creation-science is in no way scientific, and thus has as its principal aim the promotion of religious doctrine. Since the Establishment Clause of the First Amendment disallows the teaching of religion in U. S. public schools, this would then show Act 590 to be constitutionally invalid. Thus Ruse's definition of science was a central component of the case against creationism.

When asked to define science, this is what Ruse said in the courtroom:

> the most important characteristic of modern science is that it depends entirely upon the operation of blind, unchanging regularities in nature. We call those regularities "natural laws".[18]

This definition was re-emphasised in his cross-examination, quoted at the beginning of this section. And again, to emphasise that Ruse was not just trying to present a simplified analysis in the courtroom, in his later commentary on the trial he explicitly states:

> I believe the key distinguishing factor about science to be its appeal to and reliance on *law*: blind, natural regularity. Everything else follows from this notion: explanation, prediction, testing, confirmation, falsifiability, tentativeness.[19]

Judge Overton, in his *Opinion*, used this definition to justify the assertion that creation-science "is simply not science".[20] Overton states:

> [Creation science] is not science because it depends upon a supernatural intervention which is not guided by natural law. It is not explanatory by reference to natural law, is not testable, and is not falsifiable.[21]

[18] Ruse, M. "Witness Testimony Sheet: *McLean v. Arkansas*." In Ruse (ed.) 1988, p. 301.
[19] Ruse, M. "A Philosopher's Day in Court." In Ruse (ed.) 1988, p. 21.
[20] Overton 1982. In Ruse (ed.) 1988, p. 318.
[21] *Ibid.*

Ruse's definition is as clear a statement of the global view, based on Hempel's D-N schema, as is possible: the basis of science are laws of nature, which are essentially universal. Given the serious problems concerning the existence of laws in biology, you would expect a philosopher of biology to present a somewhat different view on the nature of science. You certainly wouldn't expect a philosopher of biology to defend a view of science that excludes much of biology from being genuine science. Yet here we have a distinguished philosopher of biology asserting that laws of nature form the basis of genuine science, clearly asserting the global view: a strong piece of evidence for the dominance of the global view.

More significantly, even in the aftermath of the trial, nobody raised an objection to this global aspect of Ruse's characterisation of science. Even Larry Laudan, Ruse's major critic concerning his testimony, essentially fails to question this part of Ruse's definition. In Laudan's (1982) critique he does raise a number of objections to Ruse's characterisation of science, including an objection to the centrality of law in scientific theory. Laudan points out that if laws are an essential part of science then we should have to say that Darwin was unscientific. This is because Darwin developed his theory of natural selection without laws, long before the genetic principles that underlie his theory were formulated. Thus, according to Laudan, good science can be done without the knowledge of the laws that support the theory:

> For centuries scientists have recognized a difference between establishing the existence of a phenomenon and explaining that phenomenon in a lawlike way. Our ultimate goal, no doubt, is to do both. But to suggest, as the *McLean* Opinion does repeatedly, that an existence claim (e.g., there was a worldwide flood) is unscientific until we have found the laws on which the alleged phenomenon depends is simply outrageous.[22]

However, although Laudan argues that good science can be done without the explicit use of laws, here he still asserts that the "ultimate goal" of science is to derive laws, and to explain phenomena "in a lawlike way". The implication is that such laws form the basis of our scientific theories. Thus, although Laudan is on the right track in criticising the necessary use of laws in science, here he implicitly adopts a global view concerning the foundations of scientific theory.

[22] Laudan 1982. In Ruse (ed.) 1988, pp. 353-4.

This issue of how to define science isn't just a matter of abstract academic debate, since it is clearly significant for the result of the Arkansas trial. For if the rejection of creation-science is based on a false view of the standards a theory must achieve in order to be genuinely scientific, then this rejection cannot be sustained. Proving that creation-science doesn't fit a false view of science proves nothing about its validity as a science. Laudan touches on this issue in his commentary on the trial, concluding that

> The victory in the Arkansas case was hollow, for it was achieved only at the expense of perpetuating and canonizing a false stereotype of what science is and how it works.[23]

In fact, the situation is probably even worse for the anti-creationists. For it allows a clear and strong response by the creationists: that there was no proof given in the trial that creation-science is not scientific. Yet in the *McLean v. Arkansas* case this line of argument was never taken by the defence. In one important sense this wouldn't have helped the overall creationist case very much, for Judge Overton concluded on independent grounds that Act 590 was clearly "passed with the specific purpose by the General Assembly of advancing religion".[24] That fact alone was grounds enough to conclude that Act 590 was a violation of the Establishment Clause. Nevertheless, it would have been a significant victory for the creationists if they could have defended the scientific merit of creation-science. Such a result could have paved the way for a new Act, that would appear entirely secular, with the worthy aim of promoting a valuable and scientific education in creation-science. Thus, what was under scrutiny in the bulk of the trial, the validity of creation science as a science, was not only extremely significant at the time, but continues to be an important issue.

Luckily for Ruse, and the other critics of creation-science, the definition of science presented in the trial was never challenged; the validity of science in general, and evolutionary theory in particular, was never thoroughly questioned. As far as the trial went, the relevant question was whether the teaching of creation-science amounted to the teaching of religion, and not whether the theory of evolution was a valid scientific theory. Questioning the validity of evolutionary theory was not itself directly relevant to the case. However, Ruse's characterisation of science *was* relevant to the case, and one means of criticising this conception would have been to show that it didn't apply to evolutionary theory. For, given Ruse's characterisation of science, it would have been an extremely difficult task to defend

[23] *Ibid.*, p. 355.
[24] Overton 1982, p. 315.

the validity of evolutionary theory, let alone the rest of biology. Henceforth, it would have been far more difficult to discredit evolution-science. At the least, defending evolutionary theory would have involved a great deal of technical philosophical debate, as well as detailed description and analysis of evolutionary theory, in order to show that evolutionary theory fits Ruse's model of science.

Such a task was certainly not beyond Ruse. In fact, he was one of the best people at that time to defend a global axiom-based view of evolutionary theory.[25] In his *The Philosophy of Biology* (1973) Ruse explicitly defended such an axiomatic 'hypothetico-deductive' account of evolutionary theory. This involved arguing that evolutionary theory is basically of the same logical form as physics, with its explanatory and predictive power being derived from generally applicable evolutionary laws such as Mendel's Laws and the Hardy-Weinberg Law. Thus Ruse's approach fits in squarely with the dominant, "received view" of scientific theories, an approach which explicitly embraces the global view and casts the local aside. Yet Ruse's views were not at all unanimous. Ruse had several significant critics from amongst other philosophers investigating the structure of biological theory, who presented an alternative definition of scientific theory, the "semantic view" of theories.

2.3 Global Thinking in the Semantic Account of Theories

In the years after the Arkansas trial Ruse's axiomatic view of biology came under great scrutiny by philosophers of biology such as Lloyd (1988) and Thompson (1989). These philosophers claimed that the "received view" endorsed by Ruse doesn't fit well with evolutionary theory, and that an alternative view of evolutionary theory, the "semantic view", should be adopted. My aim here is not to adjudicate in this debate. For, even if Lloyd and Thompson are correct, the question remains whether the semantic account of theories is compatible with a localised view, or whether it still embodies the fundamental assumptions of the global view. If the semantic account of theories generally incorporates the same assumptions regarding global theorising as the standard syntactic account, then for my purposes it does no better at adequately describing the structure of scientific theory, and should be rejected. On the other hand, if the semantic view is compatible with a localised methodology then perhaps it has the potential to be a powerful account of scientific theory.

[25] Ruse was not alone in defending an axiomatised view of evolutionary theory. For example, Mary Williams (1970) had also defended an axiomatisation of the theory of natural selection, although Ruse was critical of her approach because it made no mention of genetics, which he saw as being a central part of evolutionary theory.

The "received view" is explicitly based on the existence of global laws of nature, adopting what has been called the *syntactic view* of scientific theories. This approach is a remnant of the positivist program in philosophy, as it views a scientific theory as being formalizable as a set of axioms which express the fundamental laws. The theories are formalizable in first order logic, constructible out of observational and theoretical terms, with the theoretical terms of the theory being connected to the world via correspondence rules. Other laws, that are derived from the axiomatic laws, are also included in the theory. The relationship between theories in different domains is given by various 'bridge laws', which express how the terminologies and laws of two theories can be intertranslated. This "received view" is most explicit in the work of Carnap (1966) and Hempel and is epitomised by E. Nagel's *The Structure of Science* (1961). Despite coming under severe criticism since the 1950s, this view is still largely the dominant account of scientific theories, and is central to the claims of those who champion a global approach to methodology. Although papers from a symposium on the structure of scientific theories in 1969 (Suppe 1977) were highly critical of the "received view", and the work of Feyerabend, Kuhn and Lakatos in the 1960s and 1970s presented damning critiques of that approach, many of its ideals still remain dominant today, although the challenges to this view are becoming more powerful and more widespread.

Over more recent years some philosophers, often motivated by looking closely at biology, have challenged the "received view", arguing instead for what they call the "semantic view" of theories.[26] The semantic view essentially states that scientific theories are not formalised sets of laws together with correspondence rules, but instead are *classes of* (set theoretic, formal) *models* that are ideally isomorphic to the phenomena. However, for some versions of the semantic view (e.g. Giere 1999) the relationship may be somewhat weaker than an isomorphism. There are a number of different formulations of the semantic approach, yet all have in common both the assumption that scientific theories are formalizable in the language of set theory (as does the syntactic approach), and the view that there is no single formalised axiomatization of scientific theories.[27] The claim is that the semantic view represents more accurately the way scientists actually work, and also gives a better account of more complex theoretical frameworks.

How much the semantic view has superseded the syntactic view in the philosophy of science is not too clear, especially when we move away from formalised accounts of theories and

[26] Suppe 1977, 1989; Lloyd 1988; van Fraassen 1980, 1989; Thompson 1989; Rosenberg 1993; Giere 1999.
[27] Thompson 1989, pp. 73-4.

look at more general accounts of scientific theories. For example, Philip Kitcher in *The Advancement of Science* (1993) deliberately avoids talk of the semantic view, seeing it as inessential to his project. Kitcher says: "I have not found it helpful to use the standard terminology employed by proponents of the semantic conception in stating my views, but it is possible that everything I say about Darwin could in fact be translated into their idioms."[28] Similarly, Sterelny and Griffiths' (1999) thorough "introduction" to the philosophy of biology makes no mention whatsoever of the semantic view – the issue seems to be of no relevance to their investigations, although they could plausibly echo Kitcher's sentiment.

However, these views aside, the semantic view has received widespread endorsement amongst philosophers of biology as a viable alternative to the syntactic view. As well as Lloyd and Thompson, Dupré (1993) implicitly appeals to the semantic view in his endorsement of theoretical pluralism, and Rosenberg (1994) explicitly appeals to the semantic view to support his "instrumentalism". There also seems to be no other viable account of biological theory, or scientific theory in general, to rival the syntactic view other than the semantic view. Clearly the semantic view is still of significance, even if mention of it has largely disappeared from the literature in recent years. Thus it is worth taking a more detailed look at the semantic view, in particular that version of the semantic view advocated by biologically minded philosophers. The claim made by Lloyd and Thompson is that the semantic account is more amenable to biology than the received view. However, it is not clear that such a view is compatible with the kind of localised methodology I wish to defend here. That is, it is not clear whether the models that form the basis of the semantic view require the existence of global laws or generalisations. If they do require the existence of such laws then the semantic account is not compatible with a localised approach.

Paul Thompson, in his (1989) defence of the applicability of the semantic view to evolutionary theory, summarises the basic differences between the syntactic and semantic view. According to Thompson, on the syntactic view theories are direct representations of phenomena, and are linked directly to the world via correspondence rules:

> the statements of the theory are laws that describe the *actual* behaviour of objects in the world. ... The interpreted formal system directly describes the behaviour of entities in the world.[29]

[28] Kitcher 1993, p 18, footnote 22.
[29] Thompson 1989, p. 72.

In contrast, according to the semantic view, theories are merely mathematical representations of an "abstract system". Here it is worth quoting Thompson at length:

> One of the major consequences of the differences in the semantics of the syntactic and semantic conceptions is that the class of models, directly specified in terms of set theory or a state space, is an extralinguistic, highly abstract entity which is most often quite removed from the phenomena to which it is intended to apply. The relationship of a model to phenomena is one of isomorphism, and the establishment of the isomorphism is a complex task not specified by the theory. If the asserted isomorphism is not established, it may be that the theory has no empirical *application*. The theory will nonetheless be empirically *meaningful* (it is a semantic-meaning-structure) in that one knows from the theory what the structure and behaviour of phenomena would be if the phenomena were isomorphic to the theory (see Thompson 1987). Hence, in the semantic conception, the empirical meaning of a theory is separate from the empirical application of the theory. ...
>
> In a semantic conception, a theory (model) is a mathematical entity and is not defined by reference to a formal system. In other words, although a formal system for which the theory could be a model can be constructed, the theory (model) is not constructed as an interpretation of a formal system. It is defined directly by specifying the behaviour of the system. And, most importantly, laws do not describe the behaviour of objects in the world; they specify the nature and behaviour of an abstract system. This abstract system, independently of its specification, is claimed to be isomorphic to a particular empirical system. Establishing this isomorphism requires the employment of a range of other scientific theories and the adoption of theories of methodology (such as theories of experimental design and goodness of fit). None of these theories are part of, or specified by, the scientific theory which is claimed to be isomorphic to the particular empirical system.[30]

With respect to laws of nature there is a significant tension in this account. In the above characterisation of the semantic view laws are one of the central components of the models which form the scientific theory, although the claim is that they "do not describe the behaviour of objects in the world". Yet Thompson also clearly states that "laws ... specify the nature and behaviour of an abstract system" which "is claimed to be isomorphic to a particular empirical system". Thompson's caveat that a theory can be empirically meaningful without the establishment of an isomorphism is of little relevance here. For although it may allow interesting, and perhaps theoretically helpful models in more cases than the syntactic view allowed, it is still very clear that for a theory to be applicable it must be isomorphic to

[30] *Ibid.*, p. 72.

the world. So how can it be that in the case of an applicable theory "laws do not describe the behaviour of objects in the world"?

There is one reasonable explanation for this apparent inconsistency in Thompson's view: in his exposition of the semantic view Thompson largely follows Bas van Fraassen's approach[31], and van Fraassen rejects realism about scientific theory and laws of nature. In fact, van Fraassen explicitly advocates the semantic view as an anti-realist way of conceiving of scientific theory without the need for laws of nature:

> Laws do appear in this view – but only laws of models, basic principles of the theory, fundamental equations. ... Our diagnosis is *not* that the more fundamental parts of a theory are those which reflect a special and different aspect of reality, such as laws of nature! It is only the content of the theory, the information it contains (and not its structure), which is meant to have the proper or relevant *adequatio ad rem.*[32]

Given that van Fraassen's anti-realism entails that the internal structure of scientific models does not necessarily mirror real structures in nature, on his view the laws in the models do not represent actual laws of nature. Instead of reflecting real structures, the models are merely "empirically adequate"; they need only accord with empirical facts:

> Scientific models may, without detriment to their function, contain much structure which corresponds to no element of reality at all. The part of the model which represents reality includes the representation of actual observable phenomena, and *perhaps* something more. But it is explicitly allowed to be only a proper part of the whole model.[33]

But Thompson cannot hold such a view if he adheres to the fact that applicable models are isomorphic to the world. For van Fraassen's view only allows a weaker relationship to exist between model and reality, which may involve a degree of structural similarity or analogy, but explicitly denies the existence of an isomorphism. It is Thompson's insistence on the applicable models being in some way isomorphic to the world that causes the problem here, for such an isomorphism implies that the structures which make up the model *mirror* actual phenomena in the world. That is, given that the relationship between model and the world is one of isomorphism, this implies that the laws (or at least a lawlike isomorphic instance of

[31] This is not all that surprising, given that van Fraassen supervised Thompson's doctoral thesis.
[32] van Fraassen 1989, p. 188.
[33] *Ibid.*, p. 213.

them) must be true in the world if the model is an applicable one. In other words, the existence of a useful and powerful model ultimately *depends* upon the existence of laws in the world. Thus Thompson's approach is incompatible with the local view.

Elisabeth Lloyd's (1988) version of the semantic view has much in common with Thompson's, and thus has similar problems. Firstly, Lloyd's semantic view also assumes that *laws* are the building blocks of the models:

> Laws, used to describe the behaviour of the system in question, must also be defined in a description of a model or set of models.[34]

Like Thompson, when it comes to confirmation of a theory, Lloyd also states that it involves an isomorphism between elements of the model and natural phenomena:

> Under the semantic view of theories, confirming a theory amounts to confirming models – more accurately, confirming the empirical claims made *about* models, i.e., the claims stating that a natural system (or kind of natural system) is isomorphic in certain respects to the model.[35]

Interestingly, Lloyd has added the qualifier "in certain respects", but doesn't explicate precisely what those respects are. Such a qualifier allows for the possibility that some elements of the model are isomorphic to actual phenomena, while other aspects bear a looser relationship to the world. For example, a model could be composed of kind terms which represent actual natural kinds, while having laws which do not correspond to genuine laws of nature. However, when Lloyd goes on to discuss the confirmation of models, it is clear that such confirmation involves evidence of fit between the model-laws and natural laws.[36] The problem with Lloyd's semantic view is made clearer when we look at one of the examples she cites to support the power of her approach for the confirmation of theories. The first example Lloyd uses is that of Island Biogeography, which she claims "offers a straightforward example of confirmation of a model application through its fit with empirical findings."[37] However, as I discuss in detail in Chapter Three, this example is not at all straightforward. The fact that the theory of Island Biogeography involves models based on

[34] Lloyd 1988, p. 20.
[35] *Ibid.*, p. 145.
[36] See *ibid.*, pp. 152-4.
[37] *Ibid.*, p. 152.

lawlike assumptions creates deep problems for the applicability of the theory. Not only do its models provide a very loose fit with actual phenomena, the theory as a whole is particularly fruitless in its applications. I shall leave it until Chapter Three to substantiate these claims, as well as to present an alternative, more powerful, bottom-up approach.

Essentially, being based on laws makes the structure of a model effectively top-down, and therefore incompatible with a localised, bottom-up approach. Thus, as it stands, Lloyd's version of the semantic view is also incompatible with a localised approach, since it appeals to the existence of laws as a basis for scientific theory. Thus, although the semantic view is in many ways a promising and rather common sense improvement to the syntactic view, since it appears to accord more closely to the way scientists actually work, when viewed in the manner Thompson and Lloyd describe it such an approach still embodies the assumptions of the global view.

Yet it wouldn't take much work to reformulate the semantic view to remove any explicit global assumptions from it. Removing the requirement that models require laws would be a good start here. For example, the top-down approach could be replaced with a bottom-up approach that is based on local causal powers instead of laws. Weakening the requirement that models be isomorphic to the world would also help, although this may pose problems for those who wish to maintain a realist interpretation of theories. Certainly that is the way that Giere (1999) intends his version of the semantic view. Giere intentionally keeps his formulation vague enough to ensure that we can have, as his book title states, *Science without Laws*, whilst maintaining a realist view of scientific theories:

> If we are to have scientific hypotheses which are realistic and also have some reasonable chance of being true, we must avoid claims that any real system is exactly captured by some model. …
>
> … I propose we take theoretical hypotheses to have the following general form:
>
> > The designated real system is *similar* to the proposed model in specified *respects* and to specified *degrees*.
>
> … The restriction to specified respects and degrees ensures that our claims of similarity are not vacuous.[38]

[38] Giere 1999, p. 179.

I do not wish to go into the details of such an account here, for I am less concerned with investigating the formal details of scientific theory, and more concerned with arguing more generally for the plausibility and power of a localised approach to science. All I wish to highlight is the dominance of the global view, evidenced in this discussion by the fact that laws still find their way into significant versions of the semantic view, versions which are supposed to apply widely to biological theory. These formal descriptions of biological theory still ultimately appeal to the existence of global laws, even though it is questionable whether such global laws exist in biology. This highlights just how deeply global thinking is embedded in our conception of science.

3. *The Illusory Appeal of Beauty, Unity and Simplicity*

"Beauty is truth, truth beauty," – that is all
Ye know on earth, and all ye need to know.

John Keats, "Ode to a Grecian Urn"

3.1 The Seductiveness of Mathematical Beauty

In the previous section I explored *how* the ideal of global science has infected the philosophy of science, generally at the expense of locally focussed methods of inquiry. In this section I investigate some of the reasons *why* global science has dominated, and continues to dominate, philosophical accounts of science. To do so I take a brief look at some significant assumptions that underlie the dominance of the global view: the appeal of beauty, simplicity and elegance in our approaches to science. The question I wish to investigate here is why there is this assumption that "beauty is truth". Why is there the belief that striving for simplicity and elegance will lead us to a true scientific picture of the world? My first response to this question, as I discuss in this section, looks at the seductiveness of mathematical beauty, both within mathematics and for physical theory. In the next section I look at the appeal of unification in physics, especially regarding the quest to discover the Grand Unified Theory. Finally I present what I take to be the real reasons the ideals of beauty, unity and simplicity have dominated, dispelling the illusion of these ideals, and presenting further reasons to question the claim that our best approaches to science should strive for these ideals.

With few exceptions, both philosophers and scientists have been thoroughly seduced by the idea that the best theories, the most powerful approaches to scientific knowledge, are the beautiful and elegant theories. Since mathematics is the language of science, the ideal of mathematical elegance has been seen as the key to the most powerful approaches to science. Since unity and simplicity contribute greatly to the mathematical elegance of theories, and are themselves aesthetically pleasing, these ideals have been promoted as being those that will produce the best understanding of the way the world works. Scientific theory based on global, universal laws of nature exemplifies these ideals perfectly: in this way the seductiveness of mathematical beauty has led to the dominance of an approach which discards the complexities of localised science.

Mathematics can certainly be a beautiful thing. Much of mathematics is like a complicated game, where by following certain rules we can develop powerful modes of expression which build up beautifully developed pictures of complex concepts. Mathematics abstracts from common sense intuitions to develop mathematical representations of those intuitions, and then builds up a mathematical theory from these abstractions. Such theories can be, and aim to be, beautifully well integrated, so that the pieces fit together in a neat and ordered way. When theories fit together well their power to unify phenomena, consolidate explanation and prediction, and be simply expressible can be extremely seductive, and very powerful.

For example, from our common sense intuitions about numbers pure mathematics abstracts away the structure of our number system and develops the abstract mathematical ideas of group, ring, field and semi-group theory, via which we can explore and discover properties of both our common sense and other number systems. Such theories have not only been pleasing for their elegant mathematical properties, they have also led to powerful developments in practical fields such as artificial intelligence and cryptography.

Another example is that of knot theory, an area of pure mathematics which aims to develop an abstract mathematical representation of knotted structures, which gives both a unique representation and one that embodies important properties of knotted structures: for example whether a knot is symmetric or chiral (left or right handed), and what the level of complexity or 'knottedness' of the knot is. Such a task has proven extremely difficult although substantial progress has been made in developing algebraic representations of knots. Through knot theory we have also discovered connections between different realms of science, with similar representations cropping up in fields as diverse as high energy physics, quantum gravity, statistical physics, the study of the macromolecules in chemistry and the study of the DNA in biology.[39]

When diverse concepts fall into place in a neat and simply expressible theory such a theory has a distinctly pleasing, seductive quality. In developing knot theory we are explicitly aiming for a theory which unifies the complex properties of knots in a unified, simply expressible mathematical representation which has this pleasing quality. Ideally a completed theory of knots will be simply expressible, in that a unified method will give us a one-to-one correspondence between knots and their mathematical representation, and that there will be a clear and unified way of determining properties of knots and relations between knots from their mathematical representations. With knot theory there does seem to be good reasons for searching for a unified theory. For one thing the concept of a knot seems to be a fairly simple one, and experience has taught us that simple physical concepts can generally be given a useful unified mathematical representation. We also seem to be getting closer to a final theory of knots, with present efforts being substantially better than earlier efforts at mathematically representing knots.

A prime example of the appeal of mathematical elegance in physics is Einstein's theory of general relativity, a theory with a mathematical formulation that exhibits astonishing beauty. Indeed, such elegance has been explicitly appealed to as a way of justifying Einstein's theory of general relativity in favour of more complicated competing theories, as the physicist S. Chandrasekhar asserted in 1980:

> The elements of controversy and doubt, that have continued to shroud the general theory of relativity to this day, derives precisely from this fact, namely that in the formulation of his theory Einstein incorporates aesthetic criteria; and every critic feels that he is entitled to his own differing aesthetic and philosophic criteria. Let me simply say that I do not share these doubts; and I shall leave it at that.[40]

Thus the ideals of beauty, simplicity and elegance are seen as deeply important, not only in pure mathematics, but also as ideals to strive for in our mathematical formulations of physical theory. Unification and simplification can be extremely powerful in mathematics, and thus are seen as worthy aims to strive for in science.

[39] For more on knots, and the relationship between knot theory and physical theories, see Kauffman 1994, and the other volumes that make up the *Series on Knots and Everything*, edited by Louis H. Kauffman and published by World Scientific. There is also the *Journal of Knot Theory and its Ramifications*, also edited by Kauffman.

[40] S. Chandrasekhar, quoted in Schutz 1985, p. 197.

Certainly, when such an approach works, these are worthwhile aims. But is such an approach the one we should *always* take? Is the quest for mathematical beauty in our scientific theories always consistent with the quest for realism and predictive and explanatory power? For although elegance, simplicity and order may be qualities we desire in our mathematical theories, it is another question whether such qualities are ones we should strive for more generally in all of our scientific endeavours. Mathematical beauty is clearly something desirable in mathematics, but when it comes to developing theories that model and explain phenomena in the world there are strong reasons for doubting whether it is desirable to try to develop a simple and unified theory. When looking at the assumptions underlying this 'beautiful theory view' it should be clear that such a view should not be held in present times. For this view goes further than merely viewing mathematical beauty as one desirable aspect of our theories, by claiming that such beauty is in some way *constitutive* of a *good* theory. It is this claim that I oppose, for I argue that in developing theories which model and explain our complex world we must often strive for goals other than mathematical elegance in our theories. The world is complex, and our best mathematical representations of the world are quite likely to be complex, not elegant and simple.

In fact, even within mathematics, striving for simplicity and elegance may not always be the most fruitful approach. An example of this is the proof of the Four-Colour Map Theorem. This theorem states that at most four colours are required to colour any map so that no two adjacent regions have the same colour. A fairly simple and elegant proof is possible for the Five-Colour Map Theorem, but the Four-Colour Theorem is apparently *only* provable by an extremely complex and lengthy approach that can only be practically achieved by the use of computers.[41] Here the complexities of what seems to be such a simple topological problem necessitate a messy, inelegant approach to its proof.

When the first proof was originally announced, by Appel and Haken in 1976, serious doubts were raised about its validity. Usually complicated mathematical proofs are checked and verified by other mathematicians before they are considered valid: even the extremely complex proof of Fermat's Last Theorem follows the standard principles of mathematical

[41] Robertson *et al* 1996, p. 17.

deduction, and can be checked by a human. But such a verification is not possible for the Four-Colour Map Theorem, because it is impossible to verify the proof independently without the use of computers. In the case of Appel and Haken 1200 hours of computer time were needed to work through the details of the final proof, a process that could not be manually verified by humans. Despite this fact, doubts about this proof have largely disappeared, not only because it has been independently verified, but also because it has since been simplified to some degree, although its complexity still entails the use of computers. Yet even the authors of the more recent, simpler proof express some doubts about the validity of their approach:

> However, an argument can be made that our 'proof' is not a proof in the traditional sense, because it contains steps that can never be verified by humans. In particular, we have not proved the correctness of the compiler we compiled our programs on, nor have we proved the infallibility of the hardware we ran our programs on. These have to be taken on faith, and are conceivably a source of error. However, from a practical point of view, the chance of a computer error that appears consistently in exactly the same way on all runs of our programs on all the compilers under all the operating systems that our programs run on is infinitesimally small compared to the chance of a human error during the same amount of case-checking. Apart from this hypothetical possibility of a computer consistently giving an incorrect answer, the rest of our proof can be verified in the same way as traditional mathematical proofs. We concede, however, that verifying a computer program is much more difficult than checking a mathematical proof of the same length.[42]

Although the Four-Colour Map Theorem has been proved, there are still those who are not satisfied, wishing for a simple, elegant proof of this theorem. Such is the seductive appeal of mathematical elegance. But perhaps there can never be a simple proof of this theorem – this is a possibility we must take very seriously. Because of this, despite the constant desire for more simplicity in mathematics, computational proof has been largely accepted as a new paradigm for mathematicians.

Thus, even within pure mathematics, there is a realisation that sometimes we must take a messy, complex, inelegant approach in developing our theories. Unification and simplification are not always available to us, and there are other approaches we should use to achieve our theoretical aims. We shouldn't view these approaches as being inferior because

[42] *Ibid.*, p. 24.

they don't incorporate the beauty and elegance of traditional approaches to mathematics. Instead we should embrace these new methods, made available to us by new technology, as new and powerful ways of discovering mathematical truths.

3.2 GUTs, TOEs and the End of Science

Aside from purely mathematical aesthetics, another reason to believe that the ideals of beauty, unity and simplicity can lead to powerful science is to look at the extraordinary success in physics of methods that involve unification, generalisation and simplification. Modern physics has been profoundly successful, both in the power it has given us to control and predict nature, and in the elegant and beautiful way disparate areas of theory have been unified. The reductionist successes, such as the reduction of heat to mean molecular kinetic energy, also lend support to the idea that in the limit all will be explained by physics. Given the theoretical and technological successes of this approach it is not too surprising that philosophers of earlier times raised this view of theories to the highest pedestal. Amongst the sciences physics itself has been raised to the highest pedestal, with the unification of physics being seen as a valuable and achievable goal that will unlock the secrets of the universe. Once we understand the laws of fundamental physics, so the popular story goes, understanding all else is just a matter of deduction from these laws. Such talk of a GUT (Grand Unified Theory) or TOE (Theory of Everything) has supported the idea that we should strive for beauty and unity in our search for the "final theory".

The renowned physicist Stephen Hawking is one who believes the end is nigh. In his widely unread bestseller *A Brief History of Time* (1988) Hawking expresses the belief that "there are grounds for cautious optimism that we may be near the end of the search for the ultimate laws of nature".[43] Hawking reveals what may be part of his motivation for this cautious claim when he makes the extremely bold and revealing claim that:

> We already know the laws that govern the behaviour of matter under all but the most extreme conditions. In particular, we know the basic laws that underlie all of chemistry and biology.[44]

[43] Hawking 1988, p. 172.

[44] *Ibid.*, p.187.

Hawking does not attempt to justify this dubious claim[45], revealing how seriously he has been blinded by the global view. Such a fact, however, is not all that important for his overall discussion. For he raises this point to illustrate that knowledge of all the basic laws does not directly lead to complete knowledge of the world, since there is much we cannot fully explain or perfectly predict in chemistry and biology. (Claiming that we actually *don't* know the basic laws that underlie biology and chemistry would also explain this fact, but Hawking doesn't consider this possibility). Hawking's point is that unified physics will not, on its own, mean the end of science:

> even if we do find a complete set of basic laws, there will still be in the years ahead the intellectually challenging task of developing better approximation methods, so that we can make useful predictions of the probable outcomes in complicated and realistic situations. A complete, consistent, unified theory is only the first step: our goal is a complete *understanding* of the events around us, and of our own existence.[46]

Despite the concession that there will still be a lot of work to do before science is complete, this quote reveals that Hawking sees a unified theory as an essential part of a complete understanding of the universe. Unification, generalisation and simplification are thus not only worthwhile ideals to strive for in our theories, they are *necessary* in order to provide the foundation for all our science. Hawking even casts this belief in theological terms, stating that a unified theory will provide the key to understanding what he calls "the mind of God".

Stephen Weinberg is another physicist who takes on an optimistic tone when discussing the quest for the Final Theory, preaching that we are getting ever closer to the fundamental laws of nature:

> If history is any guide at all, it seems to me that there *is* a final theory. In this century we have seen a convergence of the arrows of explanation, like the convergence of meridians toward the North Pole. Our deepest principles, although not yet final, have become steadily more simple and economical. ... It is very difficult to conceive of a regression of more and more fundamental theories becoming steadily simpler and more unified, without the arrows of explanation having to converge somewhere.[47]

---

[45] Perhaps this claim is justifiable for chemistry, where the Schrödinger equation may provide a basis, although only in principle. In the case of biology, however, the work of Kauffman (1995) shows that we are still some way from discovering laws that underlie biological processes.

[46] Hawking 1988, p.187.

[47] Weinberg 1992, pp. 231-2.

Weinberg has good personal reasons to be optimistic. He received the 1979 Nobel Prize for Physics (along with Abdus Salam) for his work on *electroweak* theory, which he claimed unified two of the four fundamental forces in nature, the *weak* and *electromagnetic* forces. Such unification was claimed to be the most significant unification in physics since Maxwell unified electricity with magnetism. However, Gerard 't Hooft, whose work played an important part in Weinberg's breakthrough[48], presents good reasons for believing that electroweak theory fails to unify the forces:

> I have always maintained that the Weinberg-Salam model does not fully deserve such laudable comments. Did the new theory not start off with two different gauge fields, which we indicate by the mathematical terms 'SU(2)' and 'U(1)'? That is not unification.[49]

Despite these doubts 't Hooft is also optimistic about the possibility of a grand unification of fundamental physics, although he is far more cautious in his claims than either Weinberg or Hawking, stating the belief that a true Theory of Everything will be based on so-far undiscovered principles.[50] Yet 't Hooft also reveals a degree of dogmatism underlying his optimism when he rather flippantly states: "The only true resistance to a 'Theory of Everything' would be of a religious nature."[51] Weinberg and 't Hooft are however far more careful when discussing the implications of such a discovery, stating that it will have little impact on most other areas of science. Weinberg states that "the discovery of a final theory will not end the enterprise of science"[52], while 't Hooft expresses an optimistic hope that a unified physics will lead to success in other sciences, but acknowledges that "the epitheton 'Theory of Everything' is quite deceptive"[53] since there are many questions a TOE cannot possibly answer. Interestingly, when discussing how significant such a result would be for the future of physics 't Hooft presents a rather uninspiring, though probably prophetic vision:

> Probably more realistic was Richard Feynman's response when he was asked what to expect from a 'Theory of Everything'. Feynman said that he did not believe it would ever come. "But", he said, "if it comes I would expect that something will happen not unlike what happens with a mountain top after it has been conquered by courageous and professional

[48] Weinberg discusses the importance of 't Hooft's work in Weinberg 1992, pp. 120-5.

[49] 't Hooft 1997, p. 143.

[50] *Ibid.*, p. 164.

[51] *Ibid.*, p. 179.

[52] Weinberg 1992, pp. 239.

[53] 't Hooft 1997, p. 178.

> mountaineers." These mountaineers may well be the last to enjoy the immense beauty of the unspoilt nature there. They will have been the first to make the top accessible. Easier roads will be made, and after that a cable car, and then the tourists will arrive. In the comfort of the newly built restaurant on the top, these new 'explorers' will air *their* vision of the mountain. The mountaineer will hardly recognize his discovery because of all the garbage on it.[54]

Hawking, Weinberg and 't Hooft all expect we will discover this mountain, and eventually scale its formidable summit, although they differ in opinion on how much progress we have made so far. They do all believe that this will be the highest and most central peak amongst the range of our scientific theories, from which there will be a vast view over the entire universe. But they also accept that, although the view from the top may be vast, it may not be all that clear. Although it may be possible in principle to see everything from the highest summit, in practice this would be extremely difficult to achieve. For most of the world is far from these dizzying heights, and it may be extremely difficult to see clearly from this distance. There may also be misty barriers, clouding our vision of lands deep below. We may require new tools, new theoretical approaches, or even new ways of seeing, in order to see and comprehend all from this lofty peak. In the meantime it is far more fruitful to climb down from the ultimate summit when we want to work within the bulk of the world, and look at things up close, in our well-developed, familiar ways. Thus, even if the Grand Unified Theory exists and we manage to fully comprehend it, there will still be an important role for the rest of science in our complete understanding of the world.

According to these physicists, who are all personally involved in the search for unity, although the Grand Unified Theory would be a most significant achievement in fundamental physics, it would have little impact on most of the other sciences. In particular, it is clear that unified fundamental physics would have no effect whatsoever on biological sciences such as ecology. Even if it were possible in principle to understand ecological systems in terms of quarks and superstrings, there is certainly no way of practically achieving such a reduction since the mathematics involved would be far too complex to be workable. The ideals of unity, simplicity and elegance may lead us to the GUTs of the world, but this might not tell us very much about the diverse and complex parts which make up the world as a whole.

Unfortunately the ideal of unified physics has tended to infect our view of what a successful science should look like. This has led to the view that unity, simplicity and elegance are what

[54] *Ibid.*, pp. 178-9.

we should aspire to in our best science, showing that ultimately a form of 'physics envy' is at the core of this prejudice in favour of these ideals. Instead of expecting that such ideals will always lead to fruitful science we should be alive to the possibility that what works for physics may not work for other sciences. In fact, we should even be open to the possibility that such ideals are of limited value within physics. It may be the case that no Grand Unified Theory can ever be found, no matter how difficult this possibility may be to conceive of. As I discuss in the next section, John Dupré (1996) and Nancy Cartwright (1994, 1999) speculate that the world may be fundamentally disunified and thus not amenable to a unified description. In that case striving for unity is ultimately pointless, and we would have to make do with disparate, disunified science. Yet even if the world is metaphysically unified, we shouldn't let the quest for unification dominate our approach to science. We should be driven to develop powerful methods of investigating the natural world, at all its levels of organization and complexity, rather than dogmatically developing simple and elegant theories. Many aspects of the world are complex and crude, so why expect our best understanding of it to be simple and elegant?

3.3 Unified Science as Theological Dogma

Ultimately neither mathematical aesthetics nor Grand Unified Theory can fully justify the assumption that beauty, unity and simplicity are the primary ideals we should strive for in developing physical theory. So why is it that the standard accounts of science continue to be dominated by global thinking, stressing the importance of simple, unified laws of nature as a basis for explanation, prediction and discovery? Why is the standard view blind to the complexities of the 'special' sciences, seeing it as deeply problematic that there are few, if any, genuine laws in sciences such as ecology? Why are philosophical accounts of science so out of touch with the theories and methods that form much of contemporary science?

Here I present my speculations on what I take to be the real reasons behind the dominance of the global view. In brief, I believe that the appeal of ideals such as simplicity and unity have more to do with an underlying or implicit belief in an all-creating deity than with any genuinely epistemological concerns for powerful, true scientific theory. Thus I see the 'beautiful theory view' as being based on what is essentially theological dogmatism, the belief that the world is essentially simple, uniform and unified because that is the way God created it. Since I take it we have no way of knowing whether this theological vision is the truth I believe we should be agnostic about whether the world conforms to this view. Instead of assuming order we should be open to the possibility that even our best science may be disunified, and need not be based on global laws of nature.

These speculations are not just my own, for they have been well informed and influenced by the discussions of van Fraassen, Giere, Dupré and Cartwright, as outlined in the following discussion.

Looking at the origins of the concept of natural laws, Bas van Fraassen (1993) presents his own speculations, looking at seventeenth-century thinkers such as Descartes, Newton and Leibniz. Here he points out that these thinkers, despite their insistence on separating physics from theology, continually appeal to the idea of God as providing order and harmony in the world.[55] As an example, van Fraassen quotes from Leibniz's (1686) *Discourse on Metaphysics*:

> In whatever manner God might have created the world, it would always have been regular and in a certain order. God, however, has chosen the most perfect, that is to say the one which is at the same time the simplest in hypotheses and the richest in phenomena [56]

Similarly, Ronald Giere (1999) looks at the historical development of the idea of "laws of nature" throughout the seventeenth century, and argues that the theoretical ideal of the world being lawful has an essentially theological basis:

> there can be no doubt that, for Descartes and Newton, the connection between laws of nature and God the creator and lawgiver was explicit. Nor can there be any doubt that it was Newton's conception of science that dominated reflection on the nature of science throughout the eighteenth century, and most of the nineteenth as well.[57]

As far as contemporary philosophy of science goes, Giere points out that although our conception of natural laws has been secularised, so that God's will no longer provides an explicit basis for law, the dominant view on laws still harks back to theological origins:

> My position ... is that the whole notion of "laws of nature" is very likely to be an artifact of circumstances obtaining in the seventeenth century. To understand contemporary science we need not a proper analysis of the concept of a law of nature, but a way of understanding the practice of science that does not simply presuppose that such a concept plays any important role whatsoever.[58]

[55] van Fraassen 1993, pp. 2-5.

[56] From Leibniz 1686, *Discourse on Metaphysics (vi)*, quoted in van Fraassen 1993, p. 5.

[57] Giere 1999, p. 89.

Following what van Fraassen and Giere say above, there is ample evidence from the history of science that the assumption that the world is essentially ordered, and thus understandable via discovery of global laws of nature, has its basis in a deep-seated religious conception of the world. In our modern secular times this is a view we should resist: good science should not be based on religious faith or dogmatism. We should thus question the assumption of order, allowing for the possibility that it may be false.

John Dupré and Nancy Cartwright also consider the assumption that the world is simple and ordered as being unjustifiable. However, rather than look to the history of science, Dupré and Cartwright look to the practice of contemporary science to argue against the dogmatism of the global view. Dupré, in the introduction to *The Disorder of Things* (1993), states this point most clearly when he says:

> I claim that founding metaphysical assumptions of modern Western science, notably those that contribute to the picture of a profoundly orderly universe, have been shown, in large part by the results of that very science, to be untenable.[59]

Dupré introduces the idea of there being some deep disunity to the world as providing an alternative metaphysics, which he sees as being free from the dogmatic assumption that there is "a deterministic, fully law-governed, and potentially fully intelligible structure that pervades the material universe"[60]. This metaphysics of disunity forms a basis for his thesis of the disunity of science, as he claims that science can never be unified because of the presence of fundamental disorder in the world:

> the disunity of science is not merely an unfortunate consequence of our limited computational or other cognitive capacities, but rather reflects accurately the underlying ontological complexities of the world, the disorder of things.[61]

[58] *Ibid.*, pp. 89-90.

[59] Dupré 1993, p. 2.

[60] *Ibid.*

[61] *Ibid.*, p. 7.

In a similar way Cartwright takes very seriously the idea that the world is essentially disunified. When arguing against the reality of fundamental laws in her earlier work *How the Laws of Physics Lie* (1983), Cartwright refers to Pierre Duhem's contrast between the cluttered, messy English mind and the ordered French mind (a distinction I look at in detail in the next chapter for it relates clearly to the distinction I have made between local and global approaches to science). Here she alludes to the theological origins of the concept of natural laws when she states:

> The difference between the realist and me is almost theological. The realist thinks that the creator of the universe worked like a French mathematician. But I think that God has the untidy mind of the English.[62]

More recently Cartwright (1994) has decreed that her enemy is not realism, but what she calls "fundamentalism". Cartwright does not give a precise definition of this term, although she loosely describes it as the "tendency to think that all facts must belong to one grand scheme."[63] Clarke (1998) sums up the term well as "the dogma that order, either discovered in or imposed on the world, is a fundamental condition of the possibility of knowledge about the world."[64] What is interesting about the concept of fundamentalism is that it is an epistemological concept rather than a metaphysical one. Unlike Dupré, instead of making a claim about the underlying structure of the world, Cartwright refers to a dogmatism in our theoretical approach to understanding the world. According to Cartwright we should resist the lure of fundamentalism, and the associated ideals of beauty, unity, simplicity and universality, not because they assume a false metaphysics, but because they entail a poor methodological approach to science:

> The problem is that our beliefs about the structure of the world go hand-in-hand with the methodologies we adopt to study it. The worry is not so much that we will adopt wrong images with which to represent the world, but rather that we will choose wrong tools with which to change it. We yearn for a better, cleaner, more orderly world than the one that, to all appearances, we inhabit. But it will not do to base our methods on our wishes. We had better choose the most probable option and wherever possible hedge our bets.[65]

---

[62] Cartwright 1983, : 19.
[63] Cartwright 1994, p. 281.
[64] Clarke 1998, pp. 2-3
[65] Cartwright 1999, pp. 12-13

Following Cartwright, I suggest we should reject the dogma of fundamentalism, and thereby reject any account of science that is based on a global view. We should not presume the existence of global laws of nature, and we should reject the view that scientific explanation and prediction are essentially based on the existence of global laws. We should allow that bottom-up approaches to science provide us with legitimate and powerful means of understanding and manipulating nature. We should accept the legitimacy of *thinking locally.*

4. *Disunified Epistemology and Scientific Methodology*

So far I have concentrated on what I consider to be incorrect views on the nature of science, looking at how dominant these views are, and questioning the assumptions which underlie them. In doing so I have said little about what I consider the correct view, other than give a brief introduction to what I term the 'local view'. So, to conclude this introductory chapter, I present an overview of some aspects of my positive view on science. Briefly, this is characterised by an acceptance of the possibility of disunity together with a realist attitude to scientific theory.

My view on science begins by looking at the actual structure of science as it is practised. This presents a radically different view to that traditionally described by philosophers of science. The classical picture of the nature of science, and the relationship between different domains of scientific investigation is that of Oppenheim and Putnam (1958) in their discussion of the unification of science. On this view the domain of psychology supervenes on biology which in turn supervenes on physics. This picture sees each domain as fairly well defined and distinct, something which is quite explicit in the discussion on theoretical reduction: the bridge laws are laws which describe how to translate from the language of one domain to the language of the other and there can only be bridge laws if the domains are well defined and distinct.

My view on science is much messier. I see the world of scientific theorising as consisting of many varied and distinct areas of research, each with different aims, each with different standards of success, each with different terminology and different methodology. Following a view probably akin to Laudan's "normative naturalism"[66] I see the relationship between these different domains of science as being far more complex than the standard hierarchical

---

[66] Laudan 1990.

view. "Biology" is not a clearly defined unified discipline, but instead consists of many and varied fields of research that do not form a consistent whole. Biology consists of taxonomy, evolutionary theory, ecology, physiology, natural history, cell biology, molecular chemistry and so on – the fields which make up biology really only bear a loose family resemblance. Similarly ecology consists of many different fields: evolutionary ecology, conservation ecology, ecosystem ecology, biogeography, system dynamics and so on. Each of these fields too consists of a number of research programmes that may not bear clear relationships with one another.

Thus my claim is that the traditional view of science, which is still the dominant view, does not account for the actual relationship between different realms of science, and it gives a distorted account of the scientific world picture. This picture is not one based on a nice neat arrangement of subatomic entities which, following fundamental laws, build together to form larger more complex entities, which then combine and build up again to form the elements of every scientific theory. Instead, the picture we have is more of a mad psychedelic patchwork, which if you look at all at once seems a blurred mess. We can instead focus on small sections of the patchwork, seeing how the edges of the sections are badly frayed and clearly do not fit in with adjoining areas. Yet within the patch, in the local context it focuses on, we have a clear view of everything we require for powerful science.

The above presents a picture of disunity, as held in some form or another by John Dupré and Nancy Cartwright. Cartwright in fact uses the term "patchwork of law"[67] to describe her view that so-called "laws of nature" are generally only locally valid. Clearly this proposal of there being some deep disunity to the world is extremely significant for our view of science. For if the world is fundamentally disunified then any attempt to impose the ideal of mathematical beauty on our physical theories will be largely in vain, since the world will essentially defy an elegant and unified representation. However, according to this patchwork view, this does not rule out certain areas of the world, such as certain classes of physical phenomena, from being unified in the form of an elegant theory, and clearly when this is possible it is something we should try to achieve.

It is fair to say that I do not make such bold metaphysical claims of disunity, as Dupré tends to do. I do however think it important not to eliminate the possibility of there being some

[67] Cartwright 1999, p. 2.

form of deep metaphysical disorder inherent in the universe. The picture I paint above may seem to imply such disorder, but all it really does is account for the *possibility* of such disorder. If things happen to turn out far more ordered and unified than they seem at present then perhaps our patchwork will more resemble a single work.

Thus, like Cartwright, it is methodological rather than metaphysical disunity that primarily motivates me. It is the methodology of a scientific investigation and the corresponding construction of scientific theories and models that concerns me here. For there is a clear distinction between an approach to science that values a consistent and coherent single unified theory as the final aim of science, and an approach that values particular problem solving and local explanation over mathematical unity and elegance. Essentially what I oppose is the approach that values the development of simple and elegant theory over the practical aims of science. This is not to say that the quest for elegant theory is not a worthwhile aim in itself in some contexts, for I fully acknowledge the power of simplification, unification and consilience when they are applicable. But it does seem that the important distinction between the global and local approaches to methodology has generally not been acknowledged, to the detriment of the local approach that suits many of the biological sciences well. By describing and exploring this distinction further I hope to restore a balance in the philosophy of science, which has been far too dominated by global minded, physics-trained philosophers.

Chapter 2

Can the Fox Outwit the Hedgehog?

"Fox knows Eleventythree Tricks and still Gets caught;
Hedgehog knows One but it Always works."

Archilochos[1]

1. *The Fox and the Hedgehog*

In the previous chapter I introduced a distinction between two fundamentally different ways of approaching the world when developing scientific theories: the global and local views. The *global* approach to science is how I describe the standard view, the view that has dominated much successful work in the 'hard' sciences, and the view which has been raised to the pedestal of being exemplary science by the majority of philosophers of science. This view sees nature as being fundamentally ordered and unified, and thus able to be described by unified models. I call this the "beautiful theory view" because it tends to elevate the importance of developing beautiful and elegant theory to being the ultimate goal of science. The alternative *local* view sees nature as complex and potentially un-unifiable under a single conceptual scheme. Rather than attempting to make the whole of nature conform to a single model, those who subscribe to this view attempt to acknowledge the complexity of nature by understanding and modelling particular processes and phenomena as completely as possible, with more an eye towards accurate depiction in those particular cases, rather than deriving widely applicable principles.[2]

This distinction between having a unified, ordered view of the world and having a complex, disunified view is certainly not a new one. In a wonderful essay on Tolstoy's view of history in *War and Peace,* Isaiah Berlin outlines this idea beautifully and elegantly when he argues

---

[1] Archilochos. From Davenport, G. (tr.) 1980, p. 57.

[2] Although there is a more general claim that there are numerous ways of approaching a study of nature I will frame this discussion in terms of there being two opposing views: a globally oriented view versus a locally oriented view. I see these approaches as being opposite ends of a broad spectrum, and thus not necessarily inconsistent with each other (in that an approach which incorporates both global and local aspects is possible). Nevertheless they are different approaches to science, and present quite a sharp distinction, since they involve fundamentally different presuppositions concerning the structure of the world.

for such a distinction between two modes of understanding the world, a distinction which Berlin claims marks "one of the deepest differences which divide writers and thinkers, and, it may be, human beings in general."[3] Berlin begins his essay by quoting a fragment from the ancient Greek poet Archilochos: "The fox knows many things, but the hedgehog knows one big thing." The point Berlin draws from this aphorism is not some claim about *vulpine* and *erinaceidae* epistemology, but is a more general epistemological claim about a fundamental distinction in ontology. He illustrates this point with characteristic flair:

> there exists a great chasm between those, on one side, who relate everything to a single central vision, one system less or more coherent or articulate, in terms of which they understand, think and feel – a single, universal, organizing principle in terms of which alone all that they are and say has significance – and, on the other side, those who pursue many ends, often unrelated and even contradictory, connected, if at all, only in some *de facto* way, for some psychological or physiological cause, related by no moral or aesthetic principle; these last lead lives, perform acts, and entertain ideas that are centrifugal rather than centripetal, their thought is scattered of diffused, moving on many levels, seizing upon the essence of a vast variety of experiences and objects for what they are in themselves, without, consciously or unconsciously, seeking to fit them into, or exclude them from, any one unchanging, all-embracing, sometimes self-contradictory and incomplete, at times fanatical, unitary inner vision.[4]

In his essay Berlin argues that in order to understand Tolstoy's view of history it helps to realise that Tolstoy was a fox trying to be a hedgehog. That is, Tolstoy attempts to provide a grand and unified view of history in *War and Peace*, but his focus in the narrative is on the particular rather than the general. He looks at events in their full complexity, and this natural foxy outlook of his actually pulls him away from the grand task of fitting things together in a unified historical narrative.

I will not go into the details of Berlin's discussion here, since I am not concerned with Tolstoy's analysis of history, but instead with the philosophy of science. In this case, Berlin's distinction between the wily vision of the fox and the solid dependable vision of the hedgehog reflects precisely the distinction I have described between local and global approaches to science. In the previous chapter I argued that in the history of scientific development and the philosophy of science we have been far too enamoured with the global

[3] Berlin 1978, pp. 1-2.
[4] *Ibid.*

view, discarding local approaches to science. Thus just as Tolstoy may have been a fox trying to be a hedgehog, perhaps it is the case that philosophers are telling scientists to be hedgehogs when in fact they should be foxes. For what the hedgehog knows, no matter how big it is, may not answer all the questions we want to answer, while the fox's eleventythree tricks may give us the knowledge we seek in those cases. Contrary to Archilochos' sentiments on the superiority of the hedgehog, my aim is to show that in many cases the fox can indeed outwit the hedgehog. The hedgehog may have a good solid trick, but there is no reason to assume that it will always work.

2. *Duhem's Two Minds*

2.1 Introducing Pierre Duhem

One philosopher who has thoroughly explored the distinction between global and local approaches to science is Pierre Duhem. I choose to look at Duhem's work in detail both because he clearly and explicitly argues for a perspective that is representative of the dominant view, and because he is such an influential thinker for more recent philosophers of science. Duhem's influences flow through the most notable philosophers of science of the last century, including Hempel, Quine, Popper, and van Fraassen. His name is probably best known for the so-called "Duhem-Quine Thesis" on the underdetermination of theory by evidence, which has profoundly influenced views on the nature of scientific progress and the rationality of science throughout the last century. However, my discussion will not revolve around this aspect of Duhem's work, instead concentrating on his remarks concerning the difference between the French and English approaches to scientific theory.

More recently, much of Nancy Cartwright's work on what she terms 'fundamentalism' can be seen as a direct response to Duhem's discussion on French and English minds. In many ways Cartwright's work on this topic can be seen as both a development and a critique of many of the ideas expressed by Duhem around a hundred years ago. Thus my thorough discussion of Duhem here serves a dual purpose, for it not only clarifies my distinction between global and local approaches to science, but later, in chapter five, will also help to clarify some of Cartwright's insights into causation and scientific modelling.

In an essay titled "The English School and Physical Theories: On a Recent Book by W. Thomson" (1893)[5] and in the fourth chapter of his most important work, *The Aim and Structure of Physical Theory* (1906)[6], Duhem introduces the distinction between two kinds of minds: the ample mind of the English and the deep mind of the French. This distinction is aimed at distinguishing between two different approaches to science: one which eschews theory and concerns itself with vast and complex collections of facts; the other which abstracts facts into a small number of powerful generalisations in the form of an abstract theory. Duhem's aim is to emphasise the importance of abstraction and generalisation in the development of scientific knowledge, and to claim that science cannot progress via a mere collection of facts.

Although Duhem's arguments may have been relevant and powerful in the late nineteenth century, especially with regard to the developments of theories in physics and chemistry at that time, with the recent developments of both statistical techniques and computer modelling in complex sciences such as ecology it now seems that his characterisation of the ample minds as being broad but weak severely understates the importance of such methods. Given the importance of bottom-up modelling methods to science (especially so-called "virtual" science: techniques involving computer simulation modelling), and given that Duhem's view is still largely dominant in the philosophy of science, I defend the importance, and in many cases the superiority, of the ample mind approach to science.

2.2 Ample Minds and Deep Minds, French Minds and English Minds

Duhem begins *Aim and Structure* by arguing that a physical theory is a "summary and classification" of a group of laws experimentally established, and an explanation of these laws. The discovery of experimental laws, by which he means laws discovered in experimental situations, is fundamental to the development of a theory. From the wide and diverse array of experimental phenomena discovered we can formulate a "condensed representation" of these laws, which forms the basis of our theory. In his description of physical theory Duhem clearly subscribes to the 'beautiful theory view' of science:

[5] Duhem 1996, pp. 50-74. Hereafter referred to as "The English School".

[6] Hereafter referred to as *Aim and Structure*.

> [Physical theory] classifies these laws and, by classifying, renders them more easily and safely utilizable. At the same time, putting order into the whole, it adds to their beauty.[7]

Duhem explicitly claims that the beautiful theory will reflect the "true order" and will itself be powerful, in that through it we can predict and discover novel phenomena:

> We have proposed that the aim of physical theory is to become a natural classification, to establish among diverse experimental laws a logical coordination serving as a sort of image and reflection of the true order according to which the realities escaping us are organised. Also, we have said that on this condition theory will be fruitful and will suggest discoveries.[8]

However Duhem acknowledges that not all are equally adept at developing such a physical theory. Not all people possess what he terms the "strong and narrow" characteristics of mind needed to abstract "natural classifications" from a vast, diverse collection of complicated facts. For, Duhem claims, not all minds are "deep" like the French. There are in fact other types of minds, endemic in that eternal foe of the French, the English, who cannot conceive of theory, and in fact have no need for such conception: the ample minds.

According to Duhem the ample English mind is brilliant at dealing with mere collections of things, whereas the French mind is useless at conceiving of such collections, instead requiring abstracted summaries. The English revel in the abundance of diversity and difference, whereas the French require order, unity and sameness. The English can only understand a collection of facts as a chaotic mess, whereas the French can abstract, can mould a theory, and can condense the information contained in a vast collection of facts into a small number of fundamental principles. The French mind is "strong enough to be unafraid of abstraction and generalisation but too narrow to imagine anything complex before it is classified in a perfect order"[9]. Duhem takes the difference in modes of thinking even further, proposing:

> The opposition of the French genius and English genius is observed in every work of the mind. It is likewise noticeable in every manifestation of social life.[10]

[7] Duhem 1906, p. 30.

[8] *Ibid.*, p. 31.

[9] *Ibid.*, p. 64.

[10] *Ibid.*, p. 67.

For example, English legislation is "a prodigious mass of laws and customs, disparate and often contradictory, juxtaposed since the Magna Carta, one after the other, without any new laws abrogating those that preceded them."[11] On the other hand, French laws are "grouped by codes in which the articles of law are methodically arranged under headings stating clearly defined abstract ideas."[12] With regards to the literature of English writers such as Charles Dickens or George Eliot: "The English reader sees a charming picture where we French perceive nothing but a chaos importuning us".[13] This English tendency towards complexity and chaos is also particularly evident in the characters of Shakespeare such as Hamlet and Lady Macbeth, who are "a mess of confused, imperfect thoughts, with vague, incoherent outlines, dominating and being dominated at the same time!"[14]

Yet, rather ironically, Duhem also points out that the "broad and weak" ample mind is best exemplified by Napoleon and the "strong but narrow" deep mind is best exemplified by Newton! But of course, as Duhem acknowledges, with any broad French generalising there are exceptions to the general rule, and these are certainly notable exceptions. However, using the despised English method of arguing by accumulating facts rather than by logical principles, Duhem points out that the truly English approach to philosophy of Bacon, Locke, Hume, Bentham, and Mill contrasts sharply with the French approach of Descartes, Pascal and Poisson, and that the physics of Maxwell and William Thomson (Lord Kelvin) seems inconsistent or incoherent to a French thinker.

Here I do not want to question the validity of Duhem's stereotypes, although they are certainly extremely dubious when associated with actual English and French thinkers.[15] The point is not whether Duhem's stereotyping is valid, but whether his valorising of the deep mind approach is justified in the modern context, especially with regards to the emphasis on the importance of laws of nature for scientific explanation. Thus here I will leap in and wholeheartedly adopt Duhem's polemical tool, in order to undermine the idea that the abstracting narrow and deep (French) mind is superior to the prodigiously imaginative (English) mind at developing scientific knowledge in many contexts.

[11] *Ibid.*
[12] *Ibid.*
[13] *Ibid.*
[14] *Ibid.*, pp. 64-5.
[15] For more detail see the discussion in Jaki 1984, pp. 332-4 .

3. *Duhem on the Weakness of Ample Minds*

3.1 The Flawed Brilliance of the Ample Mind

The aim of Duhem's discussions on French and English minds is to denigrate the ample English way of thinking and hold up the French way as an exemplar of strong scientific thinking. Here Duhem is clearly writing more than a mere parochial treatise, for he is arguing that the English way of thinking is genuinely inferior when it comes to developing fruitful explanatory theories. However, at first glance Duhem's writing on the weakness of the ample English mind seems ambiguous. Sometimes he seems to be saying that the English way of thinking is merely different from the French way, and is a way that suits the abilities of the English mind. But at other times what he says is distinctly normative: abstract thinking is better for science. The problem is that Duhem has genuine respect for the power of the ample mind at performing certain tasks, in particular complex tasks that involve a broad and thorough conception of a complex situation. For example, even though he characterises such minds as "weak", Duhem emphasises his respect for the brilliance of the ample mind, making it clear that there are many human endeavours for which the ample mind is extremely well endowed:

> We have ample minds in all those who can unroll in their visual imagination a clear, exact, detailed picture where a multitude of objects are in operation. Ample is the mind of the financial speculator who from a mass of telegrams infers the condition of the wheat or wool market all over the world, and, with one glance, judges whether he is to gamble when the market is high or low. Ample is the mind of the military head of state capable of thinking out a plan of mobilization by which millions of men will arrive at the place of combat on time as necessity demands, without a hitch and without confusion. Ample also is the mind of the chessplayer who, even without looking at the chessboards, can hold matches against five opponents simultaneously.[16]

The idea seems to be that the ample mind is excellent at solving problems which involve a mass of information, which are complex, and which defy systematisation. With this in mind it is interesting to note the occupations which suit one of an ample mind are all fundamentally complex and practical tasks: financial speculator, military commander and chess player. These are tasks that have been fundamentally altered by modern use of communications and computational technology. The first task now predominantly involves

[16] Duhem 1906, p. 62.

the use of computers, especially for data compilation and distribution; the second, at such large scales, now relies heavily on computer technology for communication, planning and execution; and the third is one in which the computer is now superior to any human. (This point is quite significant, for later on I argue that this powerful ability of the ample mind to perform such tasks shows that ample minds are more suited to some sciences, since the tasks required in these sciences are more like the complex tasks mentioned here than the process of abstraction and generalization that Duhem valorises.) Later in the chapter Duhem also comments on the ample mind of the industrialist, for whom:

> His daily occupation keeps him removed from abstract ideas and general principles. Gradually the faculties constituting strength of intellect have atrophied in him, as happens with organs no longer functioning.[17]

Duhem has no problem with the suitability of the ample mind to the tasks facing the industrialist, but he is appalled at the way such methods of thinking have infected how engineers have approached physics. He sees the essentially pragmatic approach of engineers to physics as leading inevitably to a weak English sense of science. They cannot adhere to the French sense because:

> It requires of those who wish to be its pupils a mind broken in by various exercises of logic, and made supple by the gymnastics of mathematical sciences; it will not welcome as a substitute for them any intermediary or complication. How could those concerned with the useful and not with the true be expected to submit themselves to this rigorous discipline?[18]

Here Duhem considers the practical aspect of the engineer's approach to the world to necessitate the weak approach of the ample mind. This is a theme which he develops to encompass all of what he believes is wrong with the spread of what he terms "utilitarianism" in science education. At this point Duhem's discussion of the two modes of thinking degenerates into a vitriolic attack on all he considers wrong with this aspect of the spread of English thinking, for example:

> [The evil of the ample English mind] has penetrated everywhere, propagated by the hatreds and prejudices of the multitude of people who confuse science with industry, who, seeing the dusty, smoky, and smelly automobile, regard it as the triumphal chariot of the human spirit.

[17] *Ibid.*, p. 92.
[18] *Ibid.*

> Higher education is already contaminated by utilitarianism and secondary education is prey to the epidemic. In the name of utilitarianism a clean sweep is made of the methods which served until now to expound the physical sciences. Abstract and deductive theories are rejected in favour of offering students concrete and inductive views. We no longer think of putting into young minds ideas and principles, but substitute numbers and facts.[19]

There is certainly something admirable in Duhem's sentiment here, but he clearly takes things too far. As well as the rather dubious claim that "concrete and inductive views" have nothing to do with "the methods which served until now to expound the physical sciences", there seems to be another problem with his implicit assumptions. Duhem considers mere practical tasks to be of a different sort to the theoretical tasks involved in developing a scientific theory. There is a certain Cartesian quality to this assumption, in that it distinguishes the pursuits of the mind, the theoretical pursuits of the human spirit, from the practical, material pursuits. It does seem reasonable to accept that such a distinction can be made, since there seems to be a difference between merely intellectual pursuits such as language games, logic puzzles, and pure mathematics and practical pursuits such as meteorology, car maintenance and electoral politics. But the question remains whether science is a practical or an intellectual pursuit. While not wishing to dwell on this point, it is clear that many realms of science are practical pursuits: medical science, meteorology, and conservation ecology are all practically oriented. That's not to say that such sciences don't have an important theoretical element, but just to say that the idea that "concrete and inductive views" are an anathema to science is clearly nonsense.

In summary, Duhem considers the ample English mind to be excellently suited to certain tasks, generally complex practical tasks. For such practical tasks Duhem regards the English way of thinking as far superior to the French way. However, when it comes to science, the subject of Duhem's inquiry, the argument is that the English approach is genuinely inferior to the French approach. Thus there is no ambiguity in Duhem's view: the English mind is well suited to certain tasks, but it happens that the task of developing scientific theory is one for which the English mind is ill suited because it is a theoretical task rather than a practical task. Thus for Duhem the English mind is a flawed genius, brilliant at the mechanical drudgery of everyday life, but useless at acquiring profound and deep scientific knowledge of the world.

[19] *Ibid.*, p. 93.

3.2 The Visual Metaphor

Duhem supports the idea that the ample English mind is ill-suited to the task of developing physical theory by arguing that there is no depth of understanding in those of an English mind. This argument relies heavily on Duhem's characterisation of the powers of the ample mind being entirely *visual* abilities. This idea is expressed in Duhem's initial description of the ample English mind as being "broad". The ample mind is broad in scope, in that it can holistically conceive of a vast multitude of facts and sensual data, not just holding everything separately in its memory but by *seeing* the entire view from above.

> [Ample minds] have a wonderful aptitude for holding in their imaginations a complicated collection of disparate objects; they envisage it in a single view without needing to attend myopically first to one object, then to another; and yet this view is not vague and confused, but exact and minute, with each detail clearly perceived in its place and relative importance.[20]

The visual metaphor is explicit here in Duhem's characterisation of the ample mind, for the way such minds hold vast collections in their imagination is to Duhem a process like seeing. The "powerful faculty of imagination" of the ample mind is a visual faculty: ample minds are "*visualizing minds*"[21] that can see everything in their place. The visual metaphor is extended in Duhem's discussion on the differences between French and English theatre. After mentioning what a chaotic mess Shakespearian characters are, Duhem goes on to say:

> The French spectator … tries in vain to *understand* such characters; that is, to deduce clearly from a definite setting that multitude of attitudes and of inexact and contradictory words. The English spectator does not assume this undertaking; he does not seek to understand these characters, to classify and to arrange their gestures in order; he is content to *see* them in their living complexity.[22]

Thus, although the ample English mind can *see* a system (of facts or things) in its entirety, such a mind is weak because there is no *understanding*. The idea is that the ample mind does not comprehend any structure, order or classification beyond the mere enumeration of the elements of a complex system. Because of this inability to see below the surface Duhem classifies these minds as "weak".

[20] *Ibid.*, p. 56.
[21] *Ibid.*
[22] Duhem 1906, pp. 64-5.

Although Duhem acknowledges the power of the ample mind to conceive of a complex scene in its entirety, Duhem points out that most minds do not have this prodigious ability to conceive of an immense and complex scene:

> To bring directly before the visual imagination a very large number of objects so that they may be grasped simultaneously in their complex functioning and not taken one by one, arbitrarily separated from the whole to which they are in reality attached – this is for most men an impossible or, at least, a very painful operation. A host of laws, all put on the same plane, without any classification grouping them, without any system coordinating or subordinating them, appears to such minds as chaotic and frightening to the imagination, as a labyrinth in which their intelligences go astray. On the other hand, they have no difficulty in conceiving of an idea which abstraction has stripped of everything that would stimulate sensuous memory; they grasp clearly and completely the meaning of a judgement connecting such ideas; they are skilful in following, untiringly and unwaveringly, down to its final consequences, the reasoning which adopts such judgements for its principles. Among these men, the faculty of conceiving abstract ideas and reasoning from them is more developed than the faculty of imagining concrete objects.[23]

Here Duhem is of course talking about the French way of thinking. It is because of the power of these "abstract minds" to develop systemisations of laws into a theory that Duhem considers them "deep and strong". Conversely, Duhem classifies ample minds as "broad and weak" because he sees such minds as unable to comprehend the powerful abstractions upon which scientific theories are based:

> A general judgement sounds to them like a hollow void of meaning; a long and rigorous mathematical deduction seems to them to be the monotonous and heavy breathing of a windmill whose parts constantly turn but crush only the wind. Endowed with a powerful faculty of imagination, these minds are ill prepared to abstract and deduce.[24]

Here Duhem seems to slip from claiming that French minds, being unable to conceive as broadly as ample English minds, need to develop abstract theory in order to understand a complex scene, to the claim that the abstract approach *per se* is better for science. Perhaps he just considers broad English minds to be freaks of nature, unrepresentative of human intellect. But in that case the claim he makes about the power of abstract theory can only be a

[23] *Ibid.*, pp. 55-6.
[24] *Ibid.*, p. 56.

relative claim, that *for the French* abstract theory is the way to a fruitful explanatory science. Yet this is not the conclusion that Duhem draws, for he views the English way of seeing the world as one that only sees surfaces, and does not delve below the obvious to discover the true order of the world. True understanding, for Duhem, involves more than just knowledge of all things in their place, for it requires knowledge of the fundamental laws that govern everything and explain why they are what they are. True knowledge requires *theory*, and only the deep mind can develop such scientific theory.

Thus although he shows superficial respect for the powers of the ample mind, Duhem considers these powers ill suited to the task of developing a physical theory. Given Duhem's general argument that the explanatory power of science is derived from the fundamental laws of the theory, it is clear that ample minds, who cannot abstract and generalise, cannot conceive of a scientific theory of the form that Duhem describes. Thus Duhem's claim that they are "weak" minds in an absolute sense.

3.3 Mechanical Models

Duhem's final point, which occupies the remainder of the chapter in *Aim and Structure*, concerns the use of mechanical models by those of an English mind. This point further emphasises the weakness of the ample mind approach to developing scientific theories, by illustrating the concrete weakness of an approach that does not make use of abstractions.

Duhem's arguments on the use of mechanical models can only be fully understood in their historical context. For much of Duhem's writings on this point are his reflections on particular work being done by particular scientists at the time, and thus a full assessment of the validity of his arguments would necessarily involve a detailed understanding of the state of scientific theory at the time, and a knowledge of the contemporary debates. However, this history of science is not an area I wish to delve into, for I am not really concerned with whether Duhem's arguments were indeed valid and powerful at the time. Instead I am concerned with whether modern interpretations of his arguments carry any persuasive force in the present.

Duhem's earlier essay "The English School" is primarily an attack on the work of William Thomson.[25] The significant point which Duhem emphasises over and over again is that for an English thinker such as Thomson "understanding a physical phenomenon is the same thing as constructing a model that imitates the phenomenon".[26] Thus understanding, for an ample mind, does not involve deducing abstract principles which underlie the phenomenon, it merely requires that one construct a model which reliably simulates the processes one is trying to explain.

What Duhem means by a "model" here is more accurately labelled a "mechanical model". Such a "mechanical model" clearly has a distinctly physical element, because the concepts out of which a model are constructed are common sense, everyday physical entities: ropes, pulleys, drums, weights, tubes, cogs, water pumps, etc. However a model is still a theoretical construct, for Duhem is not claiming that English physicists needed to actually construct physical models involving mechanical devices such as ropes and pulleys in order to understand physical phenomena. The point Duhem is making is that for the English mind all that is necessary and sufficient for a thorough understanding of a physical phenomenon is the potential for that phenomenon to be simulated in some sort of mechanical model, and for Duhem that is not enough for a genuine understanding of phenomena. The reasons for this are of course that merely constructing a model does not provide us with underlying laws, necessities or abstractions which form part of the "natural classification" of things.

But on this point the ambiguity of Duhem's view arises again, for although Duhem would like to claim that the ample mind approach is, in absolute terms, far weaker than the French way, he has difficulty providing any resounding arguments for this. He certainly puts the point well that the use of mechanical models is far more difficult for a French thinker than abstract theory:

> It is far from the case that these models help French readers to understand a theory; on the contrary, the French must make a serious effort to understand the functioning of the apparatus that the author has described, which is sometimes very complicated. This effort is often much greater than that required to understand the abstract theory, which the model claims to embody in its pure form.[27]

[25] William Thomson (1824-1907) also became known as Lord Kelvin. He was knighted in 1865 and became Baron Kelvin of Largs in 1892.

[26] "The English School", Duhem 1996, p. 55.

[27] *Ibid.*, p. 54.

However, Duhem has a harder time arguing for the stronger conclusion that such modelling approaches do not lead to genuine explanations of the phenomenon in question. At best he argues, in a later essay from 1913, that such an approach will ultimately be an unworkable approach given our cognitive limitations:

> We have never denied the usefulness of these models, dear to the physicists of the English school. We believe they lend an indispensable aid to minds more broad than deep, more able to imagine the concrete than to conceive the abstract. But the time will undoubtedly come when, through their increasing complications, these representations or models will cease to be aids for the physicist. He will regard them instead as embarrassments and impediments. Putting aside these hypothetical mechanisms, he will carefully release from them the experimental laws they have helped to discover.[28]

But why will such models be regarded as "embarrassments"? In a distinctly Kuhnian way Duhem seems to assume that history will show such complicated approaches to be less fruitful than unified theoretical approaches. On such a point Duhem may have been largely correct concerning the work in physics that he was commenting on, since many major advancements in physics up until and during Duhem's time were significantly based upon the conceptual systematisation, simplification and theory building of the sort championed by Duhem. Yet although in his time Duhem may have been justified in rejecting the ample mind approach, this does not amount to a strong argument that such an approach is weaker in our time. The question remains whether the modern versions of Duhem's mechanical models, the complex computer aided simulations of natural processes and phenomena (such as current modelling approaches in complex sciences like conservation ecology) fall foul of the same criticisms that Duhem raises against the mechanical models of his time. This is a point I return to later, where I defend the modern modelling methods in light of Duhem's criticisms. But before that defence, in the next section I respond more generally to Duhem, exposing the limits of his arguments, and revealing how the approach of the ample, English mind can be a genuinely powerful approach to science.

---

[28] "Logical Examination of Physical Theory", Duhem 1996, p. 238.

4. *Response to Duhem*

4.1 Summary of Duhem's Arguments

In summary, Duhem argues that the ample English mind is weak because it:

(1) is merely practical, not theoretical.
(2) must conceive of the world in a purely visual way – it can just *see* the elements and relations of a complex system but cannot *understand* them.
(3) cannot abstract and generalise, thus cannot conceive of scientific theory.
(4) must understand physical phenomena via mechanical models, as opposed to the abstract, systematic and unified conception of deep minds.

In response I will show how all of these arguments fail to imply that the ample English mind is weak. The view that the ample mind is weak cannot be sustained once we reject the idea of an essential need for a broad and unifying theory. To do this we must unleash the "fundamentalist" dogmatism from Duhem's characterisation of the two kinds of minds.

As I have mentioned, there is often an ambiguity in the way Duhem speaks about the ample approach of the English mind. At times he clearly respects its power and acknowledges that such an approach can be extremely fruitful for one endowed with such a brilliant capacity for conceiving of complexity. Yet at other times he seems to want to claim something quite absolute, that the ample mind approach can never be as powerful as the deep theoretical approach of the French mind. Clearly he wants to claim that the French thinking approach is a more powerful approach to science, but he fully acknowledges the prodigious power of the ample mind to sort and classify complex facts without the need for abstract theory.

For my purposes I will take Duhem's comments on the power of the ample mind quite seriously, and argue that the ample mind approach to science is just as legitimate and powerful as the theoretical deep approach. Duhem tries to argue that the development of theory is the only way to go, and although I certainly agree that the deep mind approach is a powerful approach to understanding physical phenomena, we should also accept that the ample mind approach provides a legitimate and powerful way of investigating the world.

The methods involved in the two approaches are quite different but not necessarily incompatible. A workable approach to science can incorporate aspects of the two approaches, something that even Duhem himself acknowledges. Thus by looking closely at what Duhem says about the distinction between the two kinds of minds I shall be able to show that such a pluralist approach is both an acceptable and powerful approach to the generation of scientific knowledge.

4.2 Responses to Duhem's Arguments

The first of Duhem's arguments as summarised above is that the ample English mind is weaker because it is merely practical and not theoretical.[29] This argument really should be seen as an expression of mere prejudice rather than a substantial argument against the power of the ample mind. For the theoretical/practical distinction essentially breaks down when we consider science to be basically a problem-solving enterprise. Scientific knowledge is generated by problem solving, be that practical or theoretical, and practical knowledge isn't necessarily weaker than theoretical knowledge. In fact, as discussed in the previous chapter, abstract theoretical constraints on theory such as simplicity and unity are generally only progressive when they assist with the essentially practical nature of scientific knowledge.

Yet it does seem that there is some substance to Duhem's claim that practical knowledge is weaker than theoretical knowledge. For example, I may be able to fix my car but have virtually no idea how it works, let alone a comprehensive theory explaining the workings of my car. Thus practical knowledge in this case is a far cry from deep theoretical knowledge. But it seems that if I were able to reliably fix my car when many different things go wrong then I must have a fairly deep understanding of the workings of the car. I would at least have to know the causal relationships between different components of the car and have some sort of general theory about how it actually works. What I would know would largely be determined by what I'd need to know to achieve my practical aims, and such knowledge could quite conceivably have a deep theoretical component. For example, if I wanted to fine tune my car to be as efficient as possible then I'd presumably need to have at least a tacit knowledge of what affects, say, the efficiency of combustion. In these cases my theoretical knowledge may never be explicitly formulated, but it is clear that I do possess some sort of deeper insight into the workings of my car. Clearly I know more about the workings of a car than merely knowing what all the parts look like and how they fit together. Thus having

[29] A more recent proponent of this argument is Jack Smart, who in his (1963) discussion on explanation in the biological sciences argues that the relationship between biology and physics is akin to the relationship between engineering and physics.

practical knowledge in this case amounts to having more than a superficial understanding of the situation.

My point here is that the accusation that the ample mind is weak because it is practical not theoretical seems misplaced. Just because one's aims are practical doesn't mean that one has a weaker understanding of phenomena, it just means that you are asking different questions. Thus on its own this accusation has no force. In order for Duhem to argue that the practical mind is in fact weak he needs to show that no amount of specific complex knowledge of local factors would give us the powerful knowledge that a theory based on laws or generalisations would give us. This is in fact what Duhem attempts to argue so I will now respond to these arguments.

Duhem's second argument is that an ample mind must conceive of the world in a purely visual way – it can just *see* the elements and relations of a complex system but cannot *understand* them. This point appeals to the third argument, that the ample mind cannot develop physical theory because it does not possess the abilities of abstraction and generalisation needed to see below surface appearances.

These arguments fail to show that the ample mind is weak, since it is clear that the ample mind, if it does have the prodigious ability to understand a system in its entirety, sees more than just surface appearances and *has no need* for a formal physical theory. If ample minds really have the ability to "see clearly a very large number of concrete notions, and to grasp simultaneously the whole and the details"[30], and not just the mere details, then why would such minds need apparently superfluous knowledge such as broad generalisations or laws of nature? A truly ample mind could certainly solve any problem that a deep French mind could solve, and without the baggage of a bunch of unnecessary principles. Ample minds do not need to abstract and deduce because they can see all the details of the complete picture.

If ample minds can *really* understand the whole, then this implies that they have an understanding of all the relations between all of the objects they conceive of. For ample minds know "each detail clearly perceived in its place and relative importance"[31]. Thus the holistic view of the ample mind must include knowledge of higher order properties of the system.

---

[30] Duhem 1906, p. 61.

[31] *Ibid.*, p. 56.

In fact, it seems that ample minds could actually do a lot better than deep minds in a world with a dearth of laws, such as a world with a deep metaphysical disunity. This idea lies behind Nancy Cartwright's comment that God has an English mind rather than an ordered French mind[32] – if the world isn't as ordered as is implicitly assumed in the French conception of science, then the abstractions which Duhem considers so powerful just cannot be made, and the only accurate conception of the world that can be made is a messy, disunified, complex conception.

This point on the power of the ample mind is clear when we look at Duhem's claim that a deep mind will play a better game of checkers than of chess, due to the simpler elements and rules of checkers:

> On the other hand, the tactics of chess combine as many distinct elementary moves as there are kinds of pieces, and some of these – the knight's way of moving, for example – are complex enough to disconcert a weak imaginative faculty.[33]

Ampleness here is associated with a powerful "imaginative faculty", by which he seems to mean a mental representation of a visual situation. However, considering Duhem's claim about the chess player who doesn't even see the chessboards, ampleness must be more than the mere ability to visualise a complex situation. For the parallel-blind chess player, playing against multiple opponents without even seeing the board, cannot calculate each move they make, they cannot use brute force rule-governed thinking to play each game. Instead they must use intuition, using experientially gained knowledge from years of chess playing, to instinctively 'see' and thus know the strategic situation in each game to then determine the best move to make. The knowledge of the chess player when they visualise the board is thus deeper than just a picture of each piece in its place, for it involves an understanding of the available moves of each piece, the complex relational network between the pieces, and a deep understanding of the strategic situation in the game.

Of course, in a chess game the whole system is governed by a finite number of basic rules, and it is possible to play chess in an entirely 'French' way, purely on the basis of these rules. It is precisely via such a brute force approach that the most powerful chess playing computers operate at the present time. But such an approach will not allow any human to

[32] Cartwright 1983, p. 19.
[33] Duhem 1906, p. 76.

become a Grandmaster, since we do not have the cognitive abilities to compute all the possibilities via a brute force approach, which is precisely Duhem's point on this example.

Thus it is clear that Duhem's claim that the ample mind is weak because it only has a superficial visual representation of phenomena is a spurious attack on the strength of the ample mind. The ample mind can see more than mere appearances, for it has a deeper understanding of all aspects in its perception, the relationships between the aspects and deeper structures. It does not understand deeper properties in terms of generalisations or laws, since it has no need for explicit statements of deeper properties. However it can fully comprehend those properties when the need arises, since it can perceive all aspects of the situation simultaneously.

4.3 Logic and Disunity: (Systematic) Theories vs (Messy) Models

Here I discuss Duhem's final argument: the claim that ample minds are weaker because they must understand physical phenomena via mechanical models, as opposed to the abstract, systematic and unified conception of deep minds. On this point it is fairly clear that even though much of Duhem's discussion is extremely insightful and valid, in the end he is blinded by the dogmatism of the beautiful theory view.

Duhem's intuitive notion that physical theory essentially involves a unified, coherent rational system rests on the assumption that such an approach better suits the natural conceptual abilities of the deep thinking French hedgehog. Duhem argues for the superiority of *theories* over *models*, arguing that his preferred approach of developing theories based on laws is fundamentally more powerful than the English method of exploring phenomena via the construction of many different and potentially incompatible mechanical models. Yet, interestingly, Duhem acknowledges that there is no problem conceptually or logically with the un-unifiable approach of the ample mind. He freely admits that there are no logical barriers to a disunified view. Thus he acknowledges that such a disunified approach is certainly plausible and potentially highly effective.

There is no substantial further argument in Duhem's work that a unified approach is the way science *must* essentially proceed. In fact, the only argument he has for favouring the unified approach is an appeal to his common sense intuitions. However, whereas Duhem's intuitions may have been quite reasonable in his time it now seems that his strong intuitions concerning the futility of knowledge via modelling can be challenged by looking at recent modelling approaches in complex sciences. By describing the plausibility and power of such approaches, as I do in the next two chapters, I thus present a case for why a disunified approach to understanding the complex world via specific and context dependent modelling techniques is an effective approach to a scientific investigation into complex processes and phenomena.

At this point it is worth quoting Duhem at length in order to clarify what he is saying and to make explicit why his ideas should be rejected. Talking about the "ample but weak mind of the English physicist", Duhem states:

> Theory is for him neither an explanation nor a rational classification of physical laws, but a model of these laws, a model not built for the satisfying of reason but for the pleasure of the imagination. Hence it escapes the domination of logic. It is the English physicist's pleasure to construct one model to represent one group of laws, and quite a different model to represent another group of laws, notwithstanding the fact that certain laws might be common to the two groups. To a mathematician of the school of Laplace or Ampère, it would be absurd to give two distinct theoretical explanations for the same law, and to maintain that these two explanations are equally valid. To a physicist of the school of Thomson or Maxwell, there is no contradiction in the fact that the same law can be represented by two different models. Moreover, the complication thus introduced into science does not shock the Englishman at all; for him it adds the extra charm of variety. His imagination, being more powerful than ours, does not know our need for order or simplicity; it finds its way easily where we would lose ours.
>
> Thus, in English theories we find those disparities, those incoherencies, those contradictions which we are driven to judge severely because we seek a rational system where the author has sought to give us only a work of imagination.[34]

Clearly the major thrust of Duhem's discussion on the use of mechanical models here is to point out and argue against the disunified view of the English mind. For his French mind, the methods of the English seem weak, perhaps even irrational. This is because Duhem views the use of models by those of an English persuasion, with William Thomson being the paradigm case, as a replacement for the theoretical ideals of logic and unity which he holds so dear. The English scientist has no need to construct an abstract theory since all the phenomena that they conceive of can be described (pictured, imagined) by some sort of model; and, crucially for Duhem, there is no necessity that all the models form a consistent

[34] *Ibid.*, p. 81.

system. That is, the way someone of an ample English mind views nature need not be unified or logically consistent, for it could consist of a immensely large number of separate models which represent all known experimental facts, but do not represent phenomena in a consistent way. To illustrate this, Duhem discusses the multitude of models employed by Thomson in his *Lectures on Molecular Dynamics*.[35] He also mentions the example of physicists who simultaneously regard matter as being continuous and as being composed of separate atoms. His ideas may have also been influenced by the then recent discovery of the dual nature of light – the fact that in some circumstances light must be modelled as a wave, in others as a particle, and there is no unified model of light that accounts for its properties in all situations. However, the extent of this influence was probably minor, since according to Jaki (1984), Duhem had little knowledge of the work being done by physicists in the late 1800s.

Yet despite the fact that Duhem attempts to ridicule the complex, disunified model building approach of the English, he does not present any real argument that a disunified view of nature is problematic for science *per se*. In fact, he even acknowledges, in capitals, that there is no logical reason to insist on the claim that physical theory must be coherent and unified:

> IF WE RESTRICT OURSELVES TO INVOKING CONSIDERATIONS OF PURE LOGIC, we cannot prevent a physicist from representing different sets of laws, or even a single group of laws, by several irreconcilable theories. One cannot condemn incoherence in the development of physical theory.[36]

This is re-emphasised in his later work *Aim and Structure*:

> Logic evidently imposes on the physicist only one obligation: not to confuse or mix up the various methods of classification he employs. That is, when he establishes a certain relationship between two laws, he is logically obliged to note in a precise manner which of the proposed methods justifies this relationship. Poincaré expressed this when he wrote ... : "Two contradictory theories can, in fact, both be useful instruments of research, provided that we do not mix them together and provided that we do not seek the bottom of things in them."[37]

[35] *Ibid.*, pp. 81-5.
[36] "The English School", Duhem 1996, p. 66.
[37] Duhem 1906, p.101.

Here Duhem clearly acknowledges that there are no *logical* constraints on there being a scientific worldview consisting of theories that are inconsistent with one another, as long as we do not attempt to unify the theories. Thus he acknowledges that there is a perfectly consistent way that one can have a disunified worldview. However the idea of disunity is so abhorrent to Duhem's French mind that he argues that to defy the imperative of imposing order and unity on our physical theories would be to defy common sense. As Stanley Jaki, a biographer of Duhem, notes:

> After all, it was not in logic as such that Duhem saw the ultimate reason why incoherence among hypotheses making up the theory had to be avoided. Incoherence could not be tolerated because, and he used capitals, 'IT HARMED THE PERFECTION OF PHYSICS'.[38]

Duhem is quite explicit in *Aim and Structure* that from the viewpoint of the deep mind coherence and unity are essential for strong science. But he also seems to accept that such a view need not be held by one of an ample mind, and he never gives a persuasive argument for why unity of theory is essential for powerful science. At best he makes a relative claim:

> This unity of theory and this logical linkage among all the parts of a theory are such natural and necessary consequences of the idea that strength of mind imputes to a physical theory, that to disturb this unity or to break this linkage is to violate the principles of logic or to commit an absurdity, *from its viewpoint*.[39]

At times Duhem does attempt to provide an argument for a unified theory being more powerful than disunified theories, but the best he seems to do is in the following quote, where he just makes the claim that such an approach is "more perfect" without providing any further reason for accepting that view:

> It is better, it is more perfect, to coordinate a set of experimental laws in the midst of a single theory, where all the logically connected parts follow in undeniable order from a certain number of fundamental hypotheses stated once and for all, than to invoke, in classifying these same laws, a great number of irreconcilable theories, some of which rest on certain hypotheses [and] others which rest on hypotheses contradicting them.[40]

[38] Jaki 1984, p. 330.
[39] Duhem 1906, p. 81. (Emphasis added)
[40] "The English School", Duhem 1996, p. 67.

But Duhem just provides the above statement as an assertion without any supporting argument. All he can do to justify his faith in order and unity is rely on his "common sense" intuitions:

> All those who are capable of reflecting and of taking cognizance of their own thoughts feel within them an aspiration, impossible to stifle, towards the logical unity of physical theory. ... To prove by convincing arguments that this feeling is in conformity with truth would be a task beyond the means afforded by physics ... And yet, this feeling surges within us with indomitable strength; whoever would see in this nothing more than a snare and a delusion cannot be reduced to silence by the principle of contradiction; but he would be ex-communicated by common sense.[41]

Thus it is only through a retreat into French intuition that Duhem can claim the superiority of the methodology of French mind, which in itself reveals quite clearly the dogmatism which infects his views – a dogmatism which, if unleashed, will result in the entire argument for the weakness of the ample mind crumbling.

5. *Modelling in the Modern Context*

So why would Duhem consider the use of (mechanical) models to be weak? Given that he accepts there is no logical problem with a disunified view as long as we don't mix our models improperly, and that an ample mind can understand all aspects of a system and their relations simultaneously and thus has no need for general laws, it seems that the best argument he has against mechanical modelling is the one based on our cognitive limitations – "the time will undoubtedly come when, through their increasing complications, these representations or models will cease to be aids for the physicist".

In Duhem's time there may have been good reason to accept this point. But in our times we aren't limited by the cognitive abilities of our grey matter. We now no longer need to develop elegant and simple theories to understand and manipulate the world, for we can represent vastly complex systems in more and more accurate ways through computer modelling. The methods of such so-called "virtual science" that I consider here (and discuss in more detail in the next chapter) are modelling techniques which play a central role in recent approaches to conservation ecology. These modelling techniques, as used in methods

[41] Duhem 1906, pp. 103-4.

such as Population Viability Analysis (PVA)[42], involve constructing models which simulate real-world properties and processes in order to make reliable predictions concerning the future prospects of certain populations. Such approaches have been used to model populations of many kinds of species, such as the Grizzly Bear, Leadbeater's Possum, the Helmeted Honeyeater, Giant Kelp and the Swedish Bush Cricket.

The models are built from the bottom up, building simulations from masses of ecological data. These methods begin with an enormous amount of careful fieldwork to determine the ecological properties significant to the particular species: distribution, diet, habitat requirements and availability, migratory habits and patterns, population structure, breeding habits, life span, birth and death rates, and even possible catastrophic events which could affect the population. From these properties a model is constructed which simulates the complex dynamics of the relevant populations.

Usually such a method is only concerned with solving a particular problem in a particular context, with there being no pretensions that the modelling used in the particular case leads to anything like general laws of nature or even widely applicable generalisations. Each case requires its own approach, and it may be that there is no approach that can apply to all cases. For example, the technique known as Population Viability Analysis is generally concerned with the fate of particular populations of a particular (usually rare or endangered) species in a specific region in the face of various factors which affect the population levels such as habitat reduction or fragmentation, over-predation, climate change, and natural disasters. The details of the particular PVA will depend upon the situation at hand and the aims and objectives of the study. For example, if the PVA is to look at a dwindling population in an area fragmented by timber harvesting, with the aim of preserving viable numbers of the species, the study will concentrate on the factors needed to support such a species (the habitat requirements, reproductive habits, diet, social structure, sex ratio, life span), as well as the factors which may threaten the species (deforestation, fire, disease). But, if a PVA is to look at the prospects of reintroducing a species to a region then it may concentrate on different ecological aspects of the species, such as its tolerance to relocation and its tendency to migrate.

[42] Shaffer 1990; Boyce 1992; Lindenmayer and Possingham 1994.

Without going into any more detail just yet, what is important to note here is that it is likely that there is no globally applicable theory that can tell us everything about conservation ecology. There may be no general theory of conservation ecology of a form structurally analogous to theories in mechanics or electromagnetism. In fact, in the next chapter I argue that an attempt to form such a theory will take one away from what is needed for reliable prediction or adequate explanation, for the complexities and particularities of such biological systems are best understood from the bottom-up rather than from the top-down. I won't go into that issue any more at this point since I deal with it in detail later. All I insist here is that computational models give a similar understanding of phenomena to the mechanical models of Duhem, but these models provide us with more than a merely weak investigation of phenomena. Given the strength of computational power now available the potential of such models to explore phenomena is far greater than in Duhem's day.

Thus, in light of the power of modern modelling techniques, it is worth reassessing Duhem's conclusions on the weakness of the ample mind approach to science. By taking seriously Duhem's positive comments on the brilliance of the ample mind, especially what he says about its prodigious ability to conceive of an intricately complex system in precise detail, it is clear that a bottom-up approach can provide a deeply powerful way of understanding phenomena in our complex world. Bottom-up approaches to science are not only fundamentally different to top-down ones, but also via bottom-up approaches we can potentially discover things that would escape a top-down method, since they may give us insight into properties and processes that would be otherwise unattainable under a unified view. This is because an understanding of phenomena via possibly disunified models allows for far more detailed locally specific knowledge than would be allowed in an understanding involving unified theory. This would be especially true if it is the case that the world is fundamentally disunified, for then any attempt to impose order and unity on the world would necessarily cloud the real detail. But even if the world is somehow unifiable it would still be the case that the messy foxy way of thinking would, in some cases, be more powerful than the ordered understanding of the hedgehog. In circumstances of real-world complexity, with a host of unpredictable factors and complex forces at work, it is quite likely that the fox will nearly always outwit the hedgehog.

Chapter 3

Local thinking in Ecology

1. *Introduction*

In the previous chapter I explored the distinction between two approaches to science: a global, unified view and a local, potentially disunified view. By exploring Pierre Duhem's comments on this distinction I have argued that the two modes are quite distinct approaches to scientific theory and that the bias against the localised view implicit in the dominant accounts of scientific theory is unjustified.

This chapter has two aims. The first is to support the claim that not only is the locally focused disunified view a plausible and valid approach to scientific investigation, but that also, in some cases, it provides a more powerful approach to problem solving than the global view. In doing so I return to my starting point, taking the science of ecology seriously. Thus my second aim is to deal with the criticisms that have been levelled at theoretical ecology over recent years. In my view these aims are clearly related, in that an acknowledgement of the power of a localised approach to scientific methodology in general will lead to a defence of the strength and maturity of the science of ecology in particular.

I begin with a brief overview of the philosophy of ecology, looking at the relationship between environmental ethics and ecology, and then summarising the main criticisms that have been levelled at ecology by a number of commentators in recent years. I then concentrate on some specific cases from conservation ecology in order to support my claim that a localised approach can, in some cases, provide a more appropriate method than a global approach.

2. *Ecology and the Philosophy of Ecology*

Ecology is the science of complex systems which involve living entities. Ecology is about discovering the natural world, exploring the complexity and diversity of the natural world, exploring the interconnections between organisms and their relationship with the environment, being able to trace the causes of certain natural phenomena and being able to manipulate nature to suit our needs. Since the German zoologist Ernst Haeckel coined the word "œcology" in the 1860's to refer to the study of the struggle for existence of organisms and organism-environment relations in the wake of Charles Darwin's theory of evolution, ecology has progressed substantially. It has developed from a generally taxonomic 'stamp collecting' enterprise into a broad number of interconnected fields that use a wide range of mathematical modelling and statistical techniques which have often been adapted from fields such as thermodynamics, cybernetics and information theory.[1]

The common view about ecology is that it informs us about the diversity, complexity and interconnectedness of the level of the natural world which includes living organisms. However such a naïve view of ecology does not fully incorporate the many and varied types of research which come under the banner 'ecology'. Problem solving, prediction, taxonomy, natural history, causal modelling, explanatory theories etc. are all aspects of ecology, with the different research programmes having their own particular aims and standards. Taxonomy is an important part of ecology, for through it we discover the divisions of nature and begin to discover how the elements of nature relate. With modern ecology largely evolving from work such as Darwin's, natural history is an important aspect of many areas of ecology. Prediction is also extremely important, for often ecological research is motivated by practical problems whose solutions require reliable and accurate predictions. Modelling, such as in the Lotka-Volterra approach to population ecology (Lotka 1925; Volterra 1926), in MacArthur and Wilson's (1967) Island Biogeography, and in Population Viability Analysis (Soulé 1987; Shaffer 1990), plays a central role in much of modern ecology, with the increasing computational power of computers giving us the means to solve complex equational systems.

Ecology has sometimes been dubbed "the subversive science"[2] because of the way such knowledge of the complexity and interconnectedness of the natural world challenges many previously held ideas concerning the relationship between humanity and the environment. It is claimed that knowledge of ecology provides us directly with an "environmental ethic", which prescribes us to radically alter our treatment of the environment. This view, presented by Callicott (1982) and Rolston (1988) and derived from the work of Aldo Leopold (1949),

[1] Kingsland 1985, Golley 1993, and Real & Brown (eds.) 1991 provide excellent accounts of the development of ecology over the last century.

[2] Shepard & McKinley (eds.) 1969.

is summarised by Leopold's influential concept of a 'land ethic': "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."[3] According to the land ethic, reliable, detailed and accurate explanation from the ecological sciences is not only essential in order to follow this ethic, it actually entails we should adopt it. Holmes Rolston III expresses this link between environmental ethics and ecology as what he calls "metaecology":

> In environmental ethics one's beliefs about nature, which are based upon but exceed biological and ecological science, have everything to do with beliefs about duty. The way the world *is* informs the way the world *ought to be*.
>
> ...we always shape our values in significant measure in accord with our notion of the kind of universe we live in. What we believe about the nature of nature, how we evaluate nature, drives our sense of duty. Our model of reality implies a model of conduct.
>
> ...This evaluation is not scientific description; hence not ecology per se but metaecology. No amount of research can verify that the right is the optimum biotic community. Yet ecological description generates this valuing of nature, endorsing the systemic rightness. The transition from *is* to *good* and hence to *ought* occurs here; we leave science to enter the domain of evaluation, from which an ethics follows.[4]

Here Rolston is referring to the distinction between *facts* and *values*, and David Hume's related argument that no *ought* can be deduced from an *is*.[5] By explicitly denying Hume's *is/ought* dichotomy Rolston places the very foundations of environmental ethics at odds with traditional approaches to ethics. What is usually referred to as the 'philosophy of ecology' is the attempt to justify this impasse, in order to ground the values of environmental ethics in the science of ecology.

Whether or not Rolston is correct here does not overly concern me, although I do have serious doubts about whether ecological thinking challenges Hume's *is/ought* dichotomy. Here I am sympathetic to the view of Sagoff (1985) that there are different ethical perspectives to different fields of ecological theorising, and thus the type of investigation that we undertake may be deeply infected by moral judgements.[6] Sagoff's point is that there are two general attitudes we may take to nature: one which views nature as a resource to be

[3] Leopold 1949, pp. 224-5.

[4] Rolston 1988, pp. 230-1

[5] Hume 1888, pp. 469-70.

[6] A similar point is made by Shrader-Frechette & McCoy 1993.

controlled and exploited, and the other which views nature as a wonderful, mysterious and complex entity that we value intrinsically. In terms of policy guidance, this distinction implies that ecology can be applied either to *manage* or *protect*, and this choice of how to apply ecology is a question of value. Thus Rolston is only telling half the story about how we can leave science to enter the domain of evaluation, since he assumes that we will adopt an environmental protection attitude to ecology rather than a management approach.[7]

Here such questions of environmental ethics are only of secondary concern to me, although much of my interest in this field is motivated by concern for the future of the environment. What primarily interests me is the sort of knowledge that sciences such as ecology generate: what these sciences discover about the world, how these discoveries are explained and the sorts of models used in these sciences to represent the world. Thus my approach to the 'philosophy of ecology' is to view it as a largely neglected branch of the 'philosophy of biology'. In these terms, there is a clear relationship between the philosophy of ecology and environmental ethics. For in terms of environmental ethics, following any ethical prescriptions concerning the environment requires thorough and reliable knowledge of the environment. In order to ensure an *ought* we need to know the *is*, and we need to know how to get to the *ought* from the *is*. Thus the importance of a reliable, strongly predictive and explanatory approach to ecology.

3. *Crisis in ecology?*

3.1 The Apparent Crisis

During the mid-1980s it became apparent to many commentators that there was a crisis in ecology.[8] The source of this crisis, or at least the reason that this crisis became an important issue was in fact largely economic. In the United States large amounts of money had been invested in ecological research institutes such as the International Biological Program (IBP), and the US military had also poured money into diverse areas of research, generally motivated by the climate of the Cold War and the threat of thermonuclear war. These programs were generally fruitful, however for many ecologists it was clear that there were some significant problems with many areas of research. In particular, there seemed to be many intractable problems associated with large-scale ecological modelling of the sort

[7] For further criticism of Rolston on this point see Taylor 1986, pp. 49-53.

[8] In particular Sagoff 1985, Peters 1991, Shrader-Frechette & McCoy 1993.

favoured by the big money research. As a result the value of ecology as a science, its explanatory power, and its general authority in our society were questioned in both political and scientific arenas.

Although there has certainly been much progress in ecology since the mid-1980s the major criticisms raised then are still significant today. For, in general, ecology as it stands at present is particularly weak at coming up with detailed, realistic and predictively strong models of ecological systems. It does not seem that ecology provides us with much, if any, reliable global knowledge in the same way that physics has furnished us with powerful knowledge of fundamental laws and natural kinds. Although various approaches to ecology have attempted to take a holistic or systems view of the phenomena and systems under investigation, apart from coming up with interesting and perhaps politically and emotively persuasive idealised models such as Lovelock's (1979) Gaia hypothesis, such approaches have been deeply problematic in terms of the ways we normally assess scientific theories. Such theories generally possess very little predictive power, and thus have little practical value for problem solving; they often make quite extreme idealising assumptions and therefore cannot be said to accurately represent the phenomena they claim to describe; they don't provide us with anything approaching a law or generalisation which can be used in assessing systems other than the one under investigation; and thus they provide weak or empty explanations for phenomena, at least under the currently accepted view of scientific explanation.

When one considers just how immature ecology is, it is no surprise that there are deep conceptual and practical problems with the science. But these problems should be seen as just part of the process of scientific development. As should be expected in a developing science, methods have been changing, approaches have been varied, and there has been wide disagreement amongst those within the field of ecology. The lack of consensus on fundamental issues, and the widespread inconsistent use of basic terminology, are both seen as major problems with theoretical ecology by critics such as Peters (1991). This shows that the present status of ecology can be likened to the status of physics prior to the scientific revolution – there is clearly widespread progress but there are deep conceptual problems with the research program.

Biology is a realm of science in which conceptual and empirical issues are intimately and inextricably connected. Recent work in evolutionary theory concerning units of selection has shown that sorting out conceptual problems is an essential part of developing a clear theory. For example, the Hull-Dawkins replicator/vehicle distinction has allowed deeper conceptual insights concerning the units of selection (the gene's eye vs group selection), improving our understanding of the process of natural selection.[9] A similar investigation into ecological terminology, as well as the status of models in ecology, would be extremely useful for advancing both the status and power of ecological theory, and I see my work as at least a first step in that regard.

3.2 Criticisms of Ecology

The main criticisms levelled at ecology concerned work in theoretical ecology in particular, and centred on the terminology and the status of the theories and models used in such work. The main criticisms can be summarised as follows:

- ecological concepts are ill defined, tautologous or empty.
- ecological theories fail to make general or reliable predictions.
- ecological objectivity is infected by value judgements.

The first criticism, which I deal with in detail in the next chapter, concerns terminology in ecology. This includes concepts such as 'ecosystem', 'biome', 'niche', 'food web', as well as kind terms such as 'species', 'predator', 'population', 'community', 'gene'. Criticisms raised about the use of concept terms are that such concepts are ill defined, tautologous or empty, and that as there is no consistent use of these terms by ecologists many of the theories are themselves devoid of content. The substance of this criticism is that since there is no clear consensus on the use and definitions of these terms such terms play an empty explanatory role in theories. This also affects the testability of theories, since terminological ambiguity can result in an ambiguous or confused idea of what counts as a test or falsification of a theory. Peters (1991) makes a great deal of these criticisms, raising important issues concerning the use of tautologies and the importance of operationalism for concepts. However, although he does highlight the need for ecologists to take more care with their use of concepts, I argue that Peters overstates the depth of this problem.

The second criticism, that ecological theories fail to make general or reliable predictions, is far more significant. For it is certainly true that as a predictive science ecology has not had

[9] See Dawkins 1982; Sterelny & Griffiths 1999.

the same success as the 'hard' sciences such as physics and chemistry. Ecology has had an extremely difficult time coming up with general laws that fit the standard explanatory patterns of successful science, with the prime examples of failure being the diversity-stability hypothesis, and the area-species relationship of Island Biogeography (as I discuss in more detail later in this chapter). Thus simple methods of prediction based on well supported empirical generalisations are generally not available in ecology. In general, techniques in ecology do not provide us with general methods that apply globally to real practical situations. For example, computational approaches such as the Lotka-Volterra model of predator-prey dynamics have provided fruitful academic debate but have proved very difficult to apply to real life situations since the particular complexities of a given situation generally mean that the models are significantly unrealistic.[10]

I deal with this criticism in the bulk of this chapter arguing that, in many cases, the fact that ecological theories fail to make general predictions is not really a problem at all. For the methods in ecology that potentially provide us with the most accurate and reliable predictions are not techniques that involve broadly applicable laws, but are focussed instead on modelling a particular context, solving particular problems.

I mention the third criticism, that ecological objectivity is infected by value judgements, more for completeness than as a serious worry. This is not really a significant epistemological problem since such a claim is not usually levelled at ecology *per se*, but at environmental ethicists who unquestioningly use ecological concepts such as 'balance of nature' and 'natural order' to justify a particular political or ethical perspective – such as in Rolston's "metaecology". Commentators such as Mark Sagoff (1988b) and Shrader-Frechette and McCoy (1993) deal with it head on by claiming that this is not a significant problem if we accept that all so-called objective assessments are influenced by value judgements. By being open about the values implicit in our approach to science any epistemological worries with theories can be clearly separated from questions of value.

I will not go into the further details of these criticisms here, for the case has been well presented by Sagoff, Shrader-Frechette, McCoy and Peters. Instead I will look for a way around the criticisms levelled at ecology, which both restores faith that much of the work being done in ecology is sound, and points the way for ecology to mature as a science. I do

[10] These examples are discussed in Kingsland 1985; Pimm 1991; Shrader-Frechette & McCoy 1993.

not think that the fact that ecology does not stand up well against the standards we apply to physics means that ecology is a weak science, doomed to be no more than a trumped up stamp collecting exercise. Instead of damning ecology we need to reassess the rules and shift the goalposts.

The criticisms of ecological theory certainly present challenges to theoretical ecology, although such challenges are generally exaggerated by the critics. Much contemporary work in ecology is clearly sound, and there has certainly been great progress in methods in ecology over the last few decades. In fact it appears that the *methodological* crisis has largely passed, with, as I discuss further on, the acknowledgement that a localised 'case study' approach is much better suited to large areas of ecology than a more globally oriented theoretical approach. In recent years such methods have provided us with more rigorous approaches to problem solving, which give us reasonable predictions and more realistic models of ecological processes. The distinction between two approaches to ecology, a globally oriented top-down theory-based approach, and a locally oriented bottom-up approach, is fundamental to this methodological resolution.

This clearly relates to my earlier, broader claim that standard accounts of scientific theories are typically only accounts of the top-down approach, and that the localised bottom-up approach has been allotted at best a derogatory place within standard accounts of explanation, prediction, and the content of scientific theories. What has been left out of standard accounts of science is an account of methods that work from the bottom up rather than the top down, methods involving what I call a 'localised methodology' or the 'Local View'.

I begin here by briefly looking at how ecologists have dealt with the methodological crisis arising from the failure of their work to mature into a 'hard' predictive science, by describing how the top-down/bottom-up distinction has been used by ecologists to deal with the crisis. In the two following sections I look at particular examples from conservation ecology to illustrate the contrast between global and local oriented approaches to science. Finally I very briefly explore some policy implications of these arguments. I thus present this work as a prelude to the case for a revision of standard ideas concerning natural kinds, scientific explanation and the roles of causation and laws of nature in scientific theories based upon taking a localised approach to science seriously. Such a revision will show that kinds and

concepts in ecological theories are not as problematic as critics have claimed, and will also reassess the relationship between physics and the more complex 'special' sciences.

4. *Two Approaches to Conservation Ecology*

4.1 The 1996 National Research Council Report

In 1986 the U.S. National Academy of Sciences and National Research Council produced a report (Orians *et al*, 1986) which was largely motivated by the apparent crisis in ecology. This report argued that the standard theoretical model building approach to ecology is relatively ineffective for environmental problem solving, and that local-scale case studies are a far more effective strategy. The report cites numerous cases of successful ecological studies that support their recommendations. Sagoff (1988b) used the report to support his rejection of theoretical ecology as being of any use in environmental decision making and problem solving, instead supporting the need for more local ecological knowledge (what he terms 'local empirical ecology'). Shrader-Frechette and McCoy (1993) come to the same conclusion, supporting a natural history based case-study approach.

Here the same distinction between the hedgehog's global (top-down) and the fox's localised (bottom-up) approaches has been applied to ecological theorising. According to the views expressed in the National Research Council report, and Sagoff's commentary, there is a fundamental distinction between two types of work in ecology:

- *theoretical* ecology ('grand or universal ecological theory');
- *local empirical* ecology (as in the *case study* approach).

The first type, *theoretical* ecology, is the quest for laws and universal generalisations which express relations between kinds in nature and properties of those kinds. This approach often involves a holistic *systems* approach to ecology (Bertalanffy 1968), which generally involves expressing a system of differential equations that represent relationships between the important variables involved in a particular or generalised ecological system.[11]

In the second type of approach, instead of looking for general theories or principles, such as laws that hold between significant variables, an investigation is made into the significant

[11] For example, Brown 1995 epitomises this approach.

factors surrounding the problem at hand. The case study or local approach to ecological knowledge is a bottom-up approach – we begin our investigation by concerning ourselves with local facts in the context of locally defined problems. The National Research Council report champions this approach, and this seems to fit well with the conclusions of many theorists about biological theories, such as Rosenberg (1985; 1994), Sagoff (1988), Shrader-Frechette and McCoy (1990; 1993; 1994b), McIntosh (1987), Dupré (1993), and others, who all see the prospects of biology coming up with general laws and explanations, which can cover a wide variety of cases, as being hopeless.

As a way of illustrating the difference between these two approaches to science I will contrast two different approaches to the problem of designing conservation reserves. The first, utilising Island Biogeography, draws from a globally oriented approach while the second, utilising Population Viability Analysis, involves a local approach to ecological knowledge. My use of these two methods is purely to illustrate the difference between the two approaches, and in doing so I may misrepresent how these methods are actually used. My examples here are largely caricatures designed to clarify the distinction between local and global theorising. In reality the distinction between global and local in these approaches is not so clear, for they both involve elements of each type of thinking.[12] I also cannot fully and accurately represent these two methods as they are actually used by ecologists since there are a broad range of methods that can be characterised under either heading. However, the debate about the ideal reserve design to protect conservation values is one that could employ a predominantly global or a local methodology, and my examples are just designed to illustrate the difference between these two approaches.[13]

4.2 Island Biogeography

Robert MacArthur and Edward O. Wilson's[14] *Theory of Island Biogeography* (1967) is one of the most significant and influential works in population ecology. A perusal of

[12] Quammen (1996) provides an excellent account of the development of both approaches to conservation ecology which shows how the PVA (Population Viability Analysis) approach developed directly in the tradition of MacArthur and Wilson's Island Biogeography approach.

[13] Here I focus solely on the predictive power of the two approaches, as a means of guiding us in management decisions, and I ignore the broader issues of the explanatory content of theories within the two approaches. However, in the background I believe there is a connection between predictive power, explanatory content and the realism of theories that can justify these approaches as more than merely useful calculating devices.

[14] E. O. Wilson is better known to philosophers as the founder of sociobiology, and his rather loose and highly speculative claims in the final chapter of *Sociobiology: The New Synthesis* (1975) have been thoroughly criticised by many. However Wilson's far more important and influential work on ecology has been largely ignored by philosophers.

contemporary undergraduate texts in ecology reveals that this work still dominates approaches to conservation reserve design, the implications of climate change, and the study of extinction and repopulation. MacArthur and Wilson's original theory concerned rates of extinction and repopulation of species on islands of varying sizes at different distances from a mainland 'source'. They speculated that over time a new island (such as that formed by the eruption of Krakatau in 1883) will reach an equilibrium in which the rate of extinction of species on the island is equalled by the rate of new species migrating to the island. MacArthur and Wilson argued that their equilibrium theory explained the well-known consistent relation between the area and the number of species of a particular type on islands over the world. Since they derived this species-area relationship from more abstract species-independent considerations, they postulated the species-area relationship as being general in nature.

After the initial publication of the theory Wilson, together with Daniel Simberloff, further corroborated the theory via a series of experiments using many small islands in the Florida Keys.[15] Explicitly taking on the Popperian idea that scientific theories must be testable by experiment, and not just in a *post hoc* way of agreeing with previously gathered data but by actually intervening and manipulating nature, Wilson and Simberloff fumigated six small mangrove islands in Florida Bay with methyl bromide. Having 'removed' the original fauna they monitored population levels of terrestrial arthropods on the islands at frequent intervals over a year. The results of this and related experiments supported the general theory that there is a constant relation between an island's size and relative isolation, and the number of species that it will support in a dynamic equilibrium. When the effects of isolation are taken to be relatively constant amongst a group of islands, as is generally the case for islands in a group, a general relationship between area and number of species is found. This relationship can be represented mathematically by the following equation:

$$S=kA^z$$

or

$$\log S = \log k + z \log A$$

where S is the number of species, A is the area, k and z are *parameters* for a particular region which depend upon the particular kind of organism under investigation and the distance from

[15] Simberloff & Wilson 1969. A brief account of these experiments is in Wilson 1992.

source area of the region.[16] The significant parameter is z, represented by the slope of the line of best fit when log S is plotted against log A. The value of z ranges from about 0.15 to 0.35 and are normally around 0.3, which means that, in general, an area ten times as big supports twice as many species. Note that in this equilibrium the composition of species is constantly changing although the number is stable, thus there may be many local extinctions as long as the lost species are replaced by new ones.

Given that the application of Island Biogeography involves particular local knowledge in determining the values of parameters k and z, I should clarify in what sense I am classifying this approach as global: it has to do with the top-down nature of the theoretical approach involved here and the importance of the general 'law' $S=kA^z$. For the Island Biogeography approach begins with the global assumption, derived via theoretical considerations of extinction and migration, that for any set of islands there exists a species-area curve that describes the equilibrium state for islands in a particular region.[17] Local parameters are inserted into this general equation, but the equation itself is supposed to apply generally. The local parameters are also very high level generalisations that can be derived with little or no specific ecological information on the particular species involved. Although the parameters *depend* upon the ecology of the species involved, little knowledge of this ecology is needed to determine them. We just need to gather some simple data concerning numbers of species within areas.

MacArthur and Wilson's theory can be applied to conservation reserve design if we consider reserves to be 'islands in a sea of development'. For example, if we know how many species are contained in, say, a particular rainforest, and we know enough of the relevant ecological information in order to estimate the parameters z and k, we can determine the minimum area of reserve needed to maintain the level of biodiversity we want. We can also assess the likely loss of biodiversity due to a region contracting in size. The advantage of this method is that we do not need to know the exact composition of species or any of the particular ecological properties of the species involved, only the general number per broad category such as the number of reptile species, number of insect species or number of lichens. Thus Island Biogeography seems ideal as an objective method of minimising biodiversity loss since it doesn't involve us making decisions about which species to save, which invariably involves

[16] See Wilcox 1980 and Huggett 1995, pp. 213-224 for more detail on these parameters.

[17] See Wilcox 1980, p. 101 for a graphical representation of this. The details are discussed in MacArthur & Wilson 1967, pp. 19-67, and Huggett 1995, pp. 213-224.

favouring cute furry mammals and ignoring tiny ugly slimy things. In addition, on a more practical level, such an approach involves far less fieldwork than a species-by-species approach to conservation, and thus seems better suited for dealing with the present explosion of extinctions.

Such a use of Island Biogeography for conservation reserve design was suggested by a number of ecologists including Jared Diamond (1975) and Michael Soulé (1986), although the approach they advocated considered the limitations of the biogeography approach by also taking into account the particular ecological properties of the species and habitat involved, such as the existence of 'keystone' species. Yet there was still an important general role that Island Biogeography played in their approach in showing that larger reserves are significantly better than small reserves because:

> A small reserve not only will eventually contain few species but also will initially lose species at a high rate. For reserves of a few km^2, extinction rates of sedentary bird and mammal species unable to colonise from one reserve to another are so high as to be easily measurable in a few decades. Within a few thousand years even a reserve of 1000 km^2 will have lost most such species confined to the reserve habitat.[18]

Such a use of Island Biogeography seemed extremely enlightening at first, for it gave us an idea of the minimum size that conservation reserves needed to be in order to effectively preserve a region. With a certain amount of general ecological data relevant to the region we can discover the species-area curve and thereby derive the minimum area needed to preserve the ecological balance of the region. Perhaps, more importantly for policy makers, we can also assess the likely damage to a region due to factors that reduce the size of habitat, such as logging, clearing, mining and urban expansion. Although such prediction won't tell us which particular species will be lost, it will give us an indication of the likely overall effect of a particular habitat reduction, and Island Biogeography showed that a loss of habitat area can be extremely devastating.

Calculations based on these principles of Island Biogeography played a prominent part in E. O. Wilson's (1988; 1992) claims about biodiversity loss. By looking at data on the destruction of natural habitats, looking at tropical rain forests in particular, Wilson claimed that the rate of species extinction "would be about 1,000 to 10,000 times that before human

[18] Diamond 1975, p. 139.

intervention."[19] However, in practice, the application of Island Biogeography to the problem of species conservation is not as simple as a straight application of the species-area curve. Budiansky (1995) raises these doubts when he questions Wilson's use of the species-area curve as a basis for the claim that we are presently in the midst of a period of mass extinction, pointing out that

> the "theory" is so loose and sloppy that one can derive almost any answer one wants, according to the assumptions one chooses to start with.[20]

The reason for this is that in most, if not all cases, the particular salient features of the species and region involved create complexities that far outweigh simple biogeographical factors. What became known as the 'SLOSS debate' illustrates this point nicely – SLOSS stands for "single large or several small". The debate was over whether Island Biogeography theory tells us whether a single large reserve is better than several small reserves of the same total area..

It initially seemed that according to the theory of Island Biogeography a single large reserve should support more species than a number of smaller reserves that protect the same total area as the large reserve. Modelling showed that this was clearly the case, for the reasons outlined above: fewer species were preserved in each smaller reserve. However it was shown by Simberloff and Abele (1976) that under plausible assumptions the fragmented reserve design would be more effective for species conservation than the single large reserve. From the species-area curve a reserve of area A/2 supports more than half the species that are supported in a reserve of area A. Thus, *if the species sets in each of the two fragmented reserves are different enough*, the total number of species supported by the two smaller reserves of area A/2 is greater than the number supported by a single reserve of area A. Their argument was that different sets of species would survive in particular niches present in the different reserves, with the total number of species preserved greater than that which would have been preserved in a single reserve. This was because some of the species preserved in the smaller reserves would not be able to survive in a single large reserve due to pressures of competition. Other factors are also significant to this problem. For example, a system of smaller reserves would be far more resistant to catastrophes such as fire and disease than a single large reserve. If the last remaining population of a species exists in a single large

[19] Wilson, E. O. 1988. "The Current State of Biological Diversity." In Wilson (ed.) 1988, p. 13.
[20] Budiansky 1995, p. 166.

reserve then just one disaster could wipe it out, whereas populations spread over a disjointed system of reserves are far more resistant to such catastrophe.

This SLOSS debate became quite heated at times, with each side conceding little ground and the attacks becoming more than merely theoretical.[21] The debate has ended with no clear solution to the problem, with both sides providing examples that support their case. The only conclusion that can be drawn is that the particular optimal choice of reserve design depends strongly upon the particular salient features of the habitat and the ecological properties of the species to be protected.

The significant aspect of this debate was that an abstract use of the theory of Island Biogeography gives us no guidance as to whether a single large reserve is better for conservation than a collection of smaller reserves. If such a fundamental question cannot be answered by appealing to the theory of Island Biogeography alone, it seems that such a general theory is little more than useless in achieving our aims. Since the broad generalisations of Island Biogeography are made effectively useless by local factors such as the local ecology of the particular species involved, there seems to be little or no role that such a theory can play in conservation reserve design without detailed local knowledge.

Thus what seemed at first to be a globally oriented generalising approach to designing reserves in practice involves a good deal of local knowledge. The moral here is that top-down theorising is not enough to give us all the knowledge we need to effectively preserve biodiversity. In fact I go further, to claim that an effective approach to conservation reserve design is not just a matter of local parameters being inserted into a generalised equation such as the species-area relation. To solve these problems we need to become concerned initially with local factors and then build models from the ground up. I consider such an approach in the next section.

4.3 Population Viability Analysis

The localised or bottom-up approach I consider in contrast to Island Biogeography is Population Viability Analysis or PVA (Soulé 1987; Shaffer 1990). In contrast to Island Biogeography this method explicitly begins with an assessment of the valuable species in a particular region and is concerned with protecting those particular species rather than

[21] See Quammen (1996) for an excellent account of the personalities and exchanges involved.

preserving a general level of biodiversity. There are no clear methodological guidelines as to what constitutes a PVA other than it being a general method of projecting future population levels or deriving a figure for a minimum viable population for a particular group of organisms. Part of the reason for this is just due to the nature of PVAs: the particular characteristics of a PVA will largely depend upon the particular case. However part of the reason is also due to the sloppy nature of many completed PVAs, as reported by Boyce:

> Most PVAs are simulation studies that remain unpublished, or when published, they may only include outlines of model structures. Others involve analytical methods or "rules of thumb", always burdened with severe assumptions. PVAs vary according to the ecology of the species, the expertise of the modellers, and the extent of available data.[22]

While unconfessed use of analytical methods or 'rules of thumb' is clearly problematic, especially when "burdened with severe assumptions", the fact that PVAs vary according to the situation is the greatest strength of this method. By concentrating on local factors and specific local aims, ideally such a method need not involve the use of suspect generalisations. Of course, reliable local data is essential to such a method, as is the use of well-developed (causal and stochastic) modelling techniques, since PVAs rely heavily on both computer modelling and a thorough ecological assessment of the situation under study. In recent years advances in both computational and data collection techniques have improved the power of this approach for conservation. Awareness of criticisms such as Boyce's has also improved published accounts of PVAs, with recent PVA assessments attempting to overcome these difficulties.

As mentioned briefly in the last chapter, a PVA is typically concerned with the fate of a particular (usually rare or endangered) species in a specific region in the face of various factors that affect the population levels. This sensitivity to the particular context and particular problem is the method's greatest strength since it allows a high degree of predictive accuracy. Also, since the details of the particular PVA will depend upon the situation at hand and the aims and objectives of the study, a PVA is the ideal tool for policy guidance since it can explicitly look at the various management options and assess the future prospects of a population under the various options.

---

[22] Boyce 1992, p. 481.

The PVA I consider here as a good exemplar of this method is one concerning Leadbeater's Possum by D. B. Lindenmayer and H. P. Possingham (Lindenmayer and Possingham 1994; Possingham and Davies 1995). This is one of the most recently completed and thoroughly documented PVAs, which has also been used to advise the Victorian Government on the best course of action to take in order to fulfil policy commitments to species preservation. In particular this PVA differs from many previous studies in that instead of making predictions of population levels in the future, the authors merely *rank* the various strategies for conservation, rather than come up with a definite assessment as to the outcome of the various strategies. The authors also conducted sensitivity analyses on their models to determine how sensitive their models were to the initial data, thus providing them with some indication of how reliable their predictions were in the face of inaccurate data.

Briefly, Leadbeater's Possum was thought to have been extinct for over 50 years when it was rediscovered by forestry workers in the Central Highlands of Victoria in 1961. Since then populations have been discovered in a number of sites, most of which are within a very small region of the Central Highlands that has largely been zoned for timber and woodpulp production. In order to develop responsible management plans for harvesting the forest while protecting Leadbeater's Possum, a number of studies have been made into the properties of this rare and reclusive creature. The most recent is the PVA conducted throughout the 1990s, which is the most thorough assessment undertaken into the future viability of this species (Lindenmayer 1996).

The Leadbeater's Possum PVA begins with a thorough survey of the relevant ecological properties of the species: its habitat, breeding cycle, sex ratio, life span, nesting habits, migratory trends, diet, competitors, etc. The properties and distribution of the particular habitat patches where populations reside are also studied, and together with the ecological data of the species are input into the simulation model ALEX (Analysis of Likelihood of Extinction). ALEX is a *Monte Carlo* simulation model (Sobol' 1994) which simulates stochastic processes involving populations of possums: births and deaths of individuals, migration and diffusion of individuals between habitat patches, and catastrophes such as wildfires. Each run of the model simulates the fluctuations of population levels in each habitat patch which result from these stochastic processes. Over a large number of simulation runs an estimation of the likelihood of extinction of Leadbeater's Possum can be made for a particular time period and a particular management option.

Simulations are conducted looking explicitly at the outcomes of the various possible management options available. The likelihood of extinction probabilities are compared for each of the possible options, providing a basis upon which to rank them in terms of their relative impact on the survival of the species. On this basis decisions can be made concerning the optimal choice of lands for conservation reserves, and for more specific guidance on how to manage lands which cannot be included in such reserves.

While not going into too much detail, what is most important to acknowledge here about the method of PVA is that it primarily builds its models from an assessment of particular local facts, with particular practical problems in mind. Certain generalisations do play an important part in these models, such as birth and death rates, migration rates and the determinants for habitat quality; however these are merely low-level generalisations which make no claim other than being generalisations of empirical data – they do not profess to represent genuine regularity or laws of nature. It is better to think of them as idealisations that are in some sense justified by the empirical data. The idea is that they are just statistical generalisations of some underlying causal and stochastic process. Thus they are idealisations, which can be investigated in more detail and more accurately by developing more specific bottom-up models of the processes involved. For example, the birth rate input into the ALEX simulation is generated by a more fine grained simulation looking at the ecological processes underlying the birth rates of the species.

Thus I view the Leadbeater's Possum PVA as involving a top-level model using ALEX, along with sub-models which provide the inputs to ALEX, with further sub-models which provide the inputs to the sub-models. Of course there can't be sub-models all the way down, so we have to start somewhere. This could involve some degree of empirical generalisation at the base level, or it could involve a fundamentally stochastic basis for the modelling. In the case of PVA modelling, as well as with many other similar approaches to modelling, the basis involves a combination of these factors. Some general assumptions are made, but, importantly, there is a fundamentally stochastic element in PVA modelling, provided by what are termed *Monte Carlo* methods.

As I elaborate in the next section, these methods directly incorporate randomness in their models, an approach quite different to that which only involves solving general equations. Thus the difference between the two approaches to species conservation is more than a mere matter of degree because of this stochastic nature of the Monte Carlo method. Although both approaches involve the use of generalisations, with there being some difference in the degree of abstraction involved in the use of generalisations, the methodology of the modelling in the two approaches is fundamentally different.

5. Monte Carlo Methods

It is the stochastic nature of the Monte Carlo methods that clearly distinguishes approaches such as PVA modelling from top-down approaches to species conservation such as Island Biogeography. In this section I defend this claim by echoing the insightful comments made by Peter Galison (1996; 1997) regarding the significance of the use of Monte Carlo methods in scientific discovery. Galison provides a thorough overview of the historical development of these methods, as well as presenting a detailed analysis of their methodological and metaphysical implications. He focuses his discussion on the use of Monte Carlo simulations in theoretical and experimental physics, but his insights are clearly relevant to the use of similar stochastic methods in biology.

Although techniques like the Monte Carlo method date back to at least the time of Archimedes, it is because of the rapid development of the computer that such methods have become widespread and powerful in recent years. In fact, it was the program to develop more powerful atomic weapons in the 1950s that provided the incentive, and the funding, to use and improve on such methods:

> only during and shortly after World War II, with the advent of the computer, was a generalised mode of enquiry created to address the wide class of problems too complex for theory and too remote for experiment. By using random numbers (chosen *à la roulette*), nuclear weapons designers could simulate stochastic processes too difficult to calculate in full analytic glory.[23]

The use of stochastic simulation for modelling thermonuclear reactions not only provided a way of solving complex equations, it also allowed experiments to be conducted in simulation that were otherwise too dangerous or too expensive to perform in the real world. In the end it was primarily the use of Monte Carlo simulations that led to the successful test of the "Teller-Ulam" H-Bomb in the South Pacific in 1952.[24] Monte Carlo methods have also had a

---

[23] Galison 1997, pp. 690-1.

[24] For details see Galison 1997, pp. 709-727.

profound impact on biology, with their use becoming more and more widespread over recent years.[25]

In looking at the development and use of Monte Carlo methods involved with developing atomic bombs Galison argues that such methods have more in common with experimentation than with theoretical modelling. This is because both the simulation methods and experimentation use similar techniques of variance-reduction and error tracking, and both require replicability. In other words, both methods use the same techniques to guarantee epistemic validity. Also, and more importantly for my purposes, Galison points out that it is the metaphysical basis of the Monte Carlo method that distinguishes it from a top-down model based on a system of equations. For the method attempts to simulate actual causal and stochastic processes in nature, providing us with what could be called a 'virtual laboratory'.

Galison cites one of the early proponents of this method, the chemist Gilbert King, who made such a claim to show how this method is a huge advance in modelling over the previous techniques involving idealised systems of differential equations (which fit what I have classed as 'global' or 'top-down' methods):

> There is no fundamental reason to pass through the abstraction of the differential equation. Any model of an engineering or physical process involves certain assumptions and idealisations which are more or less openly implied in setting up the mathematical equation. By making other simplifications, sometimes less stringent, the situation to be studied can be put directly to the computing machines, and a more realistic model is obtained than is permissible in the medium of differential or integral equations.[26]

The change in attitude towards mathematics and modelling associated with this method was quite substantial. As Galison comments on the theoretical impact of the Monte Carlo method used to simulate atomic bomb blasts:

> The Monte Carlo in some ways was the culmination of a profound shift in theoretical culture, from the empyrean European mathematicism that treasured the differential equations above all, to a pragmatic empiricised mathematics in which sampling had the final world.[27]

[25] See Manly (1991) for a detailed account of the use of Monte Carlo methods in biology.

[26] King, G. W. 1951. "Monte Carlo Method for Solving Diffusion Problems." *Industrial and Engineering Chemistry*. 43. p. 2475. Quoted in Galison 1996, p. 145.

[27] Galison 1996, p. 154.

Returning to the topic of ecological modelling, the use of Monte Carlo methods in PVA simulations such as ALEX allows us to set up a 'virtual laboratory' within which we can look at the impact of various land management options on the species being studied. In the Leadbeater's Possum study Lindenmayer and Possingham (1994) explicitly make use of this laboratory, by modelling many different scenarios in order to assess the impact of logging regimes and wildfires on the future viability of the species. By incorporating the stochastic nature of natural processes in their modelling the models can more accurately depict the real processes involved. The use of techniques to test the reliability of the model, such as sensitivity analysis, also help to align the structure of the model with the structures and processes it is attempting to model. All these facts add support to Galison's claim that these Monte Carlo simulations should really be seen as a form of experiment.

As such, Monte Carlo methods are particularly well suited to localised investigations, since the models are built up from below, and can be constantly 'tweaked' to make them more consistent with the scenario they are modelling. As 'virtual experiments' Monte Carlo models allow us to greatly expand the scope of our investigations, exploring possibilities that could never occur in nature. It would be impossible in practice to assess the impact of many different types of wildfires on Leadbeater's Possum populations, but this is quite feasible using PVA modelling. We could even explore how populations would survive on possible worlds quite unlike our own. The possibilities are practically endless.

I will not go into further details of these simulations here, although I return for a more detailed look at the Leadbeater's Possum PVA in the final chapter. These methods definitely warrant further investigation, since a thorough analysis of them would illuminate debates concerning the role of abstraction, idealisation, causation, laws, experiment and modelling in science. In particular, care must be taken to look at just what sort of predictions can be made using such methods, and how reliable they are. For example, in the Leadbeater's Possum PVA the authors explicitly shy away from making definite predictions about the viability of the species under each scenario. At best, they claim, they can merely *rank* the various proposed management strategies in terms of how well they can sustain viable populations. As far as policy goes this certainly tells us enough in order to make an informed choice. But if we want to know precisely what will occur under a particular scenario this study cannot really help us, for the models do not give us that kind of reliable detail.

6. *Conclusions: The Power of Local Thinking*

What I have shown via these two examples is that a top-down (theory oriented) approach to modelling for the purposes of conservation ecology provides us with little more than extremely generalised knowledge, which is of little use for particular problems, and that an approach which does give us a detailed way of dealing with particular practical situations must essentially involve a bottom-up approach. Island Biogeography may predict the general effects of certain broad scale habitat fragmentation on biodiversity, but such predictions are likely to be either grossly vague or little more than wild guesses unless particular local knowledge is taken into account for each situation. However, if local factors become the basis of the modelling then the method used is effectively a bottom-up method such as Population Viability Analysis, since the particulars are more than mere parameters of a general model, but are defining features of a localised model. This is particularly true when local stochastic factors are included as part of the model, as occurs in Monte Carlo methods such as PVA.

Significantly, this point not only applies to ecology, but also more generally to sciences that similarly involve local problem solving in complex contexts. For example, Shrader-Frechette (1989b) presents a startling case study from hydrology to argue for a similar conclusion. The example concerns predictions about how far and how fast hazardous radioactive waste, in particular plutonium, will migrate from a particular radioactive waste dump. Using hydrogeological models, engineers and geologists predicted that the plutonium would migrate one-half inch from the site in 24,000 years. These models were based on Darcy's Law, a well-corroborated equation which gives the flow rate of a fluid through a permeable medium. Despite this approach being well-tested, in this case it provided wildly incorrect predictions: only ten years after the facility opened plutonium and other radionuclides were discovered as far as two miles offsite. In this case the difference between prediction and outcome can be explained by particular facts of the case: the presence of clay soil, high levels of soil moisture, and geological formulations which allow flow. Taking these factors into account would involve far more than just applying corrections to Darcy's Law, since the particular context fully invalidates the law. Instead, deriving an accurate prediction would involve modelling the situation from the bottom-up, based on local factors (soil types and geology) rather than from the top-down, based on Darcy's Law.

Thus an important and inescapable part of practical theorising is that it is concerned with a particular limited scope. In the case of ecology, each investigation fundamentally involves

ecological entities, properties and processes within local situations, even if the situation is as broad as the entire planet. The contrast here is with the typical investigation in physics, where we are attempting to discover patterns or processes in the natural world which are universal – we often use a local experimental set-up to isolate phenomena, but the intention is to discover facts about nature that can occur anywhere. However, ecological investigations, even the broadest systems approach, are concerned with understanding or manipulating a particular system, with little or no interest in deriving global laws or generalisations which apply universally, for much of the knowledge we gain from ecology is merely local. We understand particular aspects of local systems, discover particular local facts, and can solve particular local problems without there necessarily being a generalised global theory that supports our predictions and explanations. In investigating such local systems we typically have no intention of developing generalised models which can be applied to a wide range of circumstances. Instead we are concerned with solving particular problems, be they theoretical (e.g. explaining certain phenomena) or practical.

The power of bottom-up approaches to ecological problem solving, such as PVA, is made even clearer by looking at how such methods can apply to the SLOSS problem. As discussed earlier, this problem – whether a single large reserve is better for conservation than several small reserves of the same total area – has no general solution, and cannot be adequately addressed utilising top-down approaches such as Island Biogeography. However, a context sensitive, bottom-up approach, modelling the future viability of endangered populations under each of the possible options, can be used to determine the most effective reserve design. For example, to determine the best system of reserves to protect Leadbeater's Possum, Lindenmayer and Possingham (1994) simulate future populations for a number of possible reserve designs, under a number of scenarios, and determine which design best protects the species. Such modelling is built from local, specific factors: the biology and ecology of the species (nesting requirements, migratory habits, etc.); the local geography (location of suitable habitat); the effects of local catastrophes (fire, drought); and the effects of land use (various logging practices). This form of modelling allows for detailed comparisons between the different management options, providing a clear indication of the most effective strategy for protecting the species. The relevant question for conservation is not about *single large or several small*, but is about finding the best reserve design to suit the particular context.

Given the localised aspect of ecology, and given that we consider ecology to be providing us with genuine scientific knowledge of the world, this provides clear evidence that we need to reassess the standard accounts of explanation and the role of laws of nature in scientific theories. These localised approaches are more than just predictively strong tools, since they provide us with genuine knowledge of the world. In particular, basic causal and stochastic facts are the building blocks of the localised approach, and are not explained by higher-level theory (as particular facts, such as the length of a year, can be explained by Newtonian mechanics). The explanatory basis for these models comes from below rather than from above.

However, the standard conceptions of natural kinds, scientific explanation, causation and laws of nature do not fit well with a localised methodology. There seems to be no place for a localised conception of methodology within a traditional account of scientific theories, which means either the traditional conception is incomplete or that a localised methodology is without merit. If localised methodology is without merit then most, if not all of the work done in ecology (and other similar 'special' sciences) is worthless, a conclusion anyone who takes ecology seriously cannot accept.

Thus we should see the traditional conception of science as incomplete. We should accept local approaches to science as being as valid as traditional global approaches. We should also be open-minded as to which approach will suit a particular problem in a particular context. For, as I have argued, in some cases the localised bottom-up approach is far more powerful than a top-down approach. The moral here is that ecologists should not be seduced by 'physics envy', but follow the methodology that best suits their needs. The moral for conservation policy is that much more emphasis is needed on detailed ecological analysis in order to guide us towards management objectives. Broad policy objectives cannot be met by broad requirements: for example, biodiversity cannot be preserved merely by setting aside quota percentage areas of forest types. There are no reliable short cuts to good conservation.

Such a conclusion is one that has been readily accepted by many notable ecologists. Significantly, Daniel Simberloff, who together with E. O. Wilson played an important role in developing the theory of Island Biogeography, is one who has since rejected the top-down approach in conservation ecology. This is a point he emphasised in a revealing conversation with science journalist David Quammen:

> The proper way to bring science into conservation planning, Simberloff says, is with detailed ecological studies of particular species in particular places, not by applying grandiose theories. The term for what he favours is *autecology*, embracing an imperative to learn the ways of the creature itself, and the immediate relationships that connect it to its place, before drawing conclusions about the overall structure of the community to which the creature belongs. This contrasts with *synecology*, attending more to the community dimension and the sort of organising principles that Diamond saw among the Bismarck Archipelago birds. [...]
>
> Some people are still doing that sort of work [autecology], he says, but they don't get enough credit from their colleagues and they don't exert influence on conservation policy. Too many ecologists are in a hurry to generalise. Too much attention goes to the hotshot theories.
>
> "It's sad. There's an element of physics envy in all of this," Simberloff tells me. "That's what is really at issue here. Conservationists and conservation scientists feel that unless they can point to a theory – and the more quantitative it is, the better – they won't be able to get people to respect their views and ideas." The people whose respect is so crucially at issue are government bureaucrats, conservation administrators, and politicians, as well as scientific colleagues. In order to be heeded in the councils of policy, some ecologists believe, they've got to deliver deductive conclusions in concise mathematical form. "And it's too bad, but ecology isn't that kind of science."[28]

[28] Quammen 1996., pp. 481-2.

Chapter 4

Localised Natural Kinds: A Realist View of Species Pluralism

1. The Problems of Kinds

In the previous chapter I began a response to critics sceptical of the power of biological theory, especially in theoretical ecology, by arguing that a localised, bottom-up approach to science is not only legitimate, but also in some cases a more appropriate methodology for ecological problem solving. In this chapter I look at the criticisms that have been levelled at the central components of much biological theory – the *kind terms*.

One of the main criticisms levelled at theoretical ecology over recent years is that the kind terms that appear in ecological theory are deeply problematic. Sceptics about theoretical ecology such as Peters (1991) and Sagoff (1988b) argue that many of the important theoretical terms used in ecology, such as *species*, *biome*, *community*, *population*, and *niche*, are in fact ill defined, tautologous or empty. Such sceptics conclude that there is no hope of developing a strong predictive and explanatory science of ecology because of the deep conceptual problems associated with these important kind terms.

Looking at the history of ecology, it is clear that there have been theoretical terms that were widely used, but defined in numerous, often inconsistent ways. For example, Shrader-Frechette and McCoy (1993) outline the many uses of the terms *community* and *stability*, and the related stability-diversity hypothesis, showing that much of the confusion over the topic is due to the conceptual difficulties surrounding the terminology. Their review of the use of the term *community* by ecologists reveals that the community concept is still largely a "hodge-podge of ideas":

> When faced with concocting a definition, suitable for conservation applications, it appears to us that ecologists seem able to say what a community should be. When faced with nature in all its complexity, however, they seem unable to say precisely what it is.[1]

Similarly the concept of *stability*, and the related idea of the *balance of nature* are fraught with conceptual difficulties, with it being difficult to define the concept of balance or stability for entities that are essentially dynamic and ever changing. This has meant that different ecologists have used different conceptions of stability in their respective work, creating terminological confusion.[2]

Over recent years some progress has been made towards sorting out these difficulties. Pimm (1991) discusses these problems thoroughly, and looks to have gone some way to solving the conceptual problems associated with such ecological terminology. In the case of *stability* Pimm argues that the term as it is used has multiple meanings, with the definition most appropriate for a particular purpose depending upon the ecological scale of that investigation. A short-term understanding of stability differs from a conception of stability over a long ecological time scale, and a small-scale conception of stability differs from a broad one (such as the difference between stability in a local region and stability on a whole continent). Thus the conception of stability that is most appropriate for an ecological study will depend upon the scale of the study. Understanding, and being explicit about the sense of stability being used can help avoid conceptual confusions.[3]

Peters' (1991) related criticism of ecological terminology is that many of the terms used in theoretical ecology are tautological and thus are of limited practical use. For example, the concept of a *niche* has been notoriously difficult to define in a non-tautologous way: it can be defined in terms of the ecological space occupied by a particular community, but the common definition of community is in terms of the occupied niche. While Peters' criticisms of the current situation amongst theoretical ecologists may be well substantiated, it is another question whether it is possible to refine our definitions of theoretical concepts in ecology in such a way to make them non-tautologous, non-arbitrary, and operational. With the term *niche* we have come a long way towards solving the difficult conceptual problems associated with that term, with there being a useful, non-circular and explanatorily powerful way of

[1] Shrader-Frechette & McCoy 1993, p. 31.
[2] *Ibid.*, pp. 32-47.
[3] Pimm 1991, pp. 4-5.

defining the concept in terms of the multi-dimensional ecological space occupied by the species.[4] Similarly for other problematic terms it may just take time, money, debate, success and failure for their conceptual problems to be successfully ironed out.

Thus although the conceptual difficulties associated with these first two problems are intricate and difficult, they do not present a serious challenge to the foundations of theoretical ecology, since they seem to be problems that can be sorted out by more careful research, more rigorous debate and careful scrutiny of work. They are not deeply conceptual problems that challenge the entire science of ecology, since they are just the result of confusions and inadequacies that you would expect in such an immature science – they reveal the science of ecology merely to be shoddy in some places rather than deeply flawed at a fundamental level.

A final problem, however, seems more serious. This is the criticism that biological kinds are not *natural kinds* at all, but are somewhat arbitrary groupings that do not reflect any underlying division in nature. The term *natural kind* itself is one that appears frequently in much philosophical literature, yet generally is only loosely defined, if defined at all. Usually examples of particular natural kind terms such as *gold*, *water*, *tiger*, and *elm* are used freely as if it is common sense that such terms are natural kinds – in most cases the naming of such natural kind terms generally serves as a definition of a natural kind.[5] Yet despite the common absence of a definition of the term *natural kind,* in the history of philosophy there is a rich tradition of the use of this concept. Discussions by Hacking (1991) and Boyd (1991) trace the history back through the work of Russell, Mill, Peirce, Venn, and Locke to Plato and Aristotle, a genealogy which reveals that the concept has been both extremely important and deeply problematic throughout the history of philosophy and science. However although it would certainly be extremely valuable to delve into the genealogy of the concept of natural kinds, this is something I will largely leave by the wayside, concentrating instead on the contemporary debate over conceptions of natural kinds.

[4] F[illegible]ssions on the *niche* concept see Griesemer 1992; Colwell 1992; Sterelny & Griffiths 1999, pp. 268-2[illegible].
[5] For example, in their influential discussions both Kripke (1972) and Putnam (1975) presume that terms such as these are natural kinds. They then proceed to give an analysis of the semantics of natural kind terms without giving an analysis of what makes a particular term a *natural* kind term. To be fair, however, that issue didn't seem of such importance at that time for Kripke and Putnam, with questions of meaning and reference of proper names and general kind terms being the prime topic of those discussions.

The intuitive idea is that a natural kind is some natural grouping of entities in the world, a class of things which are somehow united by common properties, distinctions, or concepts – what we find when we "carve nature at the joints", a phrase commonly attributed to Plato and always embedded in quotation marks.[6] Intuitively there seems to be a distinction between genuine natural kinds and artificial or arbitrary kinds, which are just arbitrary groupings of things.[7] Thus candidates for natural kinds such as *gold*, *water*, *tiger*, and *elm* can be contrasted with arbitrary kinds such as *yellow metals*, *clear liquids* or *fierce animals*. The natural kinds are supposed to correspond to genuine divisions in the world, reflecting an underlying deep division in nature, whereas the latter categories in the above list are artificially constructed arbitrary groupings that don't reflect significant divisions in nature. The natural kinds are the entities that play a role in explanation, prediction, and induction in our scientific theories; they are the participants in the causal workings of the world. The arbitrary kinds are the classes that don't play any role in the workings of the world, even though they may be classes describable by possession of common properties. For example, it seems that a grouping such as 'fierce animals' shouldn't qualify as a natural kind since the class of fierce animals includes a heterogeneous array of entities that only have the fact that they have the properties of 'being fierce' and 'being animals' in common, and little else of significance can be derived from this fact. With the case of 'clear liquids', although this kind seems somewhat arbitrary it could be argued that such a kind could play an important role in our scientific conception of the world: 'clear liquid' could plausibly play some role in relation to Snell's Law of refraction (although the relevant property for Snell's Law is the refractive index of the liquid, a more particular property than merely being clear, and one that applies to liquids we wouldn't necessarily classify as being clear).

We expect a genuine natural kind to have more cohesion than being united merely by possession of common properties; we expect natural kinds to fulfil some important causal

[6] Surprisingly there are few explicit expressions of this idea in Plato's work. One is in the *Phaedrus* during a discussion on the nature of love, where he mentions the idea of understanding the concept using the principle "of division into species according to the natural formation, where the joint is, not breaking any part as a bad carver might" (*Phaedrus*, 265e). Another use of this phrase, noted by Hacking (1991), is the rather macabre "Let us carve them according to their natural divisions as we would carve a sacrificial victim" (*Statesman*, 287c). Note that although this phrase appears in the J. B. Skemp translation (Plato 1961, p. 1055), in the B. Jowlett translation (Plato 1953, p. 501) the phrase is rather different: "We must carve them like a victim into members or limbs, since we cannot bisect them. For we should certainly divide everything into as few parts as possible."

[7] From here on I will drop the label *artificial* and call such kinds *arbitrary* kinds. The word artificial introduces an ambiguity, since sometimes natural kinds are contrasted with kinds that are human artifacts, such as *chairs* and *bicycles*, as well as with kinds that seem to be somewhat dependent upon our theoretical conceptions, such as *disease* and *child abuse* (as discussed by Hacking 1995). I wish to leave open the possibility that such kinds are genuinely natural kinds, therefore some kinds labelled *artificial* by some may be *natural* according to my account.

role in the workings of the world. However, despite clear intuitions on this distinction, it has been an extraordinarily difficult problem to define just what it is that can be added to the idea of a kind being united by a group of properties (or a *property cluster*), but with added features which distinguish a natural kind from an arbitrary collection of properties. For Aristotle what united a natural kind, and separated one kind from another, was an *essence*, a property that was in some way a defining feature of each kind. All members of the same kind share the same *essential properties*, properties that are necessary and sufficient for membership of that kind. For example, the essence of *gold* is that it is composed of atoms of atomic number 79, atoms containing 79 protons.

However, it has been argued that the kind terms that appear in biological theory differ logically from the kind terms that appear in chemistry and physics, such as proton, quark, photon, heat, and acid. Whereas kind terms in chemistry and physics are said to be genuinely natural kinds, or real divisions in the world, usually defined in terms of *essentialist* foundations, with many biological kinds it is generally agreed that there can be no such essentialist description. From the conclusion that biological kinds such as *species* have no essentialist foundation some commentators have concluded that it is impossible to formulate generalised laws of nature using such terms, and hence theories in biology cannot be truly explanatory (since, on this view, explanation involves an appeal to laws of nature, or at least to empirical generalisations). Following such reasoning Smart (1963) concluded that explanation in biology is of a different type to explanation in the 'hard' sciences such as physics and chemistry, since the kinds in biological theories are clearly not universal in the same way that *mass*, *charge* and *atomic number* are universal. Similarly Alex Rosenberg (1994) concludes that biology is an instrumental science, in the sense that at best the theories in biology can only reflect the particular way we divide up the world to suit our particular interests, since there are no true divisions in nature at the level of our theories in biology.

Although Smart and Rosenberg are generally correct in their conclusion that theories and explanations in biology are of a different sort than in the standard accounts of 'hard' sciences such as physics, this does not entail that biological theory is necessarily merely an instrumental science, or a mere application of a more fundamental science. Smart and Rosenberg adhere to Duhem's intuitive view that strong scientific knowledge must be universal and unified rather than localised and disunified. As I have argued in the previous chapters, this is a view based on the unjustified assumption that we should be aiming primarily for simplicity, beauty and unity in our scientific theory. Instead, as I have argued, a disunified approach can be just as legitimate, and at least as powerful as a unified approach. For these reasons it is clearly far too premature to give up hope of developing strongly explanatory and predictive theories in biology, thus it seems that the criticisms associated with the use of kind terms in ecology do not lead to the sceptical conclusions that some authors claim they do.

In this chapter I concentrate specifically on the term *species*. I do this for a number of reasons. The first is that the term is one of the most important kind terms in many areas of biology: species are generally the objects of study in evolutionary theory; they also tend to be the object of study in many areas of conservation ecology since they are the entities we wish to protect and conserve. The second reason I focus on *species* is that it has become commonplace to claim that species are not natural kinds, a claim that not only seems counterintuitive, but also will clearly be shown to be quite erroneous. The final reason is that I believe that reconceptualizing the notion of natural kinds with regards to *species* will not only show that species are indeed real categories in nature, but will help us to deal with the difficult conceptual problems of how to define and differentiate species.

In concentrating on the one kind term I argue more generally that some of the conceptual problems associated with natural kind terms in biology are rather benign, by relaxing some of the theoretical requirements for such kind terms. Of course, the kind terms in ecology such as *food web* and *community* present different and perhaps far more difficult problems than the concept of *species*, since these ecological terms are potentially far more heavily theory-laden than the very familiar notion of species. As such, a defence of the validity of such terms would involve delving into the complex, diverse mix of theoretical approaches in ecology, a task well beyond the scope of this work. But what this work can contribute to the defence of these kinds is an understanding of how kind terms which differ from the standard essentialist forms found in physics and chemistry can be seen as genuinely natural kinds: real categories in the world which play a real role in the workings of the world.

Both Smart and Rosenberg assume that kind terms must be universal and context independent in order to play a role in explanation and induction, whereas I argue that a *localised* account of natural kinds is not only coherent and sound, but accords well with the methods of scientists, especially in the more complex sciences. I discuss the problem of essentialism with respect to kind terms in biology, and argue for a form of theory-dependent pluralism, claiming that our theories reflect our interests in the sense that they reflect the particular problem we want our theory to solve – the focus on a particular problem is what makes this conception of natural kinds *localised*. However I claim that this in no way leads to the instrumentalist conclusion that our theories reflect arbitrary categories in nature and do not model the reality of nature. Thus I argue that a pluralist, realist and localised conception of natural kind terms is both coherent and accords well with current practices in many areas of science.

2. *Species and Realism*

2.1 Introduction to the Species Problem

There is an enormous volume of literature on the species problem, especially over the last few decades since philosophers and biologists have begun crossing disciplines. In recent years some degree of agreement has been reached over some of the problems associated with the species concept. However there are still some important issues in dispute on the species problem, especially concerning the role of kind terms such as *species* in the theories used by biologists, and how that relates to the explanatory and predictive power of such theories. In particular, the question of whether species are genuinely natural kinds that reflect ontologically real categories in the world is a central aspect of current disputes.

The traditional view of species, which traces back to Aristotle and Plato, is that species are *kinds* that can be defined in terms of the essential properties of those kinds. The system of classification seems natural and obvious when we look at the array of organisms that exist in nature and the clear and natural distinctions between types of organisms. Entities in nature can be classified by grouping them according to physical, ecological and behavioural traits, with the species concept being the most basic and lowest level grouping of organisms. We just have to go out into nature and do some thorough fieldwork in order to discover those sets.

However, in practice things are not so simple, with the problem of defining species being one of the most thoroughly debated and fruitful areas of theoretical work in the philosophy of biology. Yet despite this flourishing of intellectual activity we seem further than ever from a unitary definition which encompasses the concept of what species are. It was with both a degree of cynicism and a kernel of truth that Philip Kitcher wrote:

> The most accurate definition of "species" is the cynic's. Species are those groups of organisms which are recognised as species by a competent taxonomist. Competent taxonomists, of course, are those who can recognise true species.[8]

Kitcher's point here is actually to reject such cynicism, for he sees it as a conclusion that has been hastily accepted far too often. Yet there is the problem that, looking at how biologists actually work, the cynic's definition seems fairly correct. Clearly unless there is a way of cashing out precisely what a "competent taxonomist" actually does this definition seems like just the sort of tautology that critics of biology thrive on. If there are no clear methodological guidelines that a "competent taxonomist" should follow, then perhaps we should accept that the species concept does not divide nature at its joints, but instead the choice of species definition is largely a choice of whim made by the individual scientist.

Following Kitcher's lead my response to this challenge involves exploring the conceptual terrain of the *species* concept in order to look for an alternative conception which allows for a realist interpretation. But before delving into the complex zoo of approaches to defining species I shall discuss just how significant this problem is for biology, and why it is so important to formulate a realist conception of species.

2.2 Rosenberg's 'Instrumental' Biology

The criticism I investigate here is the claim that *species* cannot be defined in any clear and consistent way, and so the use of the term in theory is largely arbitrary, merely reflecting the interests of the scientists rather than any genuine division in nature. This 'instrumentalism' is advocated by Alex Rosenberg (1985, 1994), and also lies behind John Dupré's (1993) claims about the disunity of science, although Dupré would surely resist the strong 'instrumentalist' conclusions of Rosenberg.

Rosenberg's idea of biology being 'instrumental' actually involves two claims. The first claim concerns the status of theoretical terms in biology, which he claims do not "divide nature at its joints" and thus can only reflect the interests of the biologist in the context they are used. It is this claim I will respond to in this chapter. Rosenberg's other claim is that theories in biology cannot ever provide realistic models of reality because in biology there are no laws, and not even any exceptionless generalisations, and so according to standard

[8] Kitcher 1984, p. 308.

accounts of scientific explanation, which require laws of nature in order to explain, theories in biology can never provide adequate explanations. In the previous chapters I effectively responded to this claim of Rosenberg's, since I have argued that laws of nature are not essential for powerful science, and that explanation can be derived from below as well as from above.

I have put Rosenberg's 'instrumentalism' in 'scare quotes' here because there is a difference between the 'instrumentalism' defended by Rosenberg, which relates to biological theory (and other complex sciences), and the standard idea of scientific instrumentalism which is normally directed at theoretical physics. Standard instrumentalism is usually directed at unobservable theoretical entities, and claims that the posits of our theories are not real entities, but are just theoretically useful terms which work well in order to "save the phenomena". For an instrumentalist our scientific theories do not necessarily inform us about the nature of reality, with theories only useful given their degree of empirical adequacy. For example, an instrumentalist would claim that the fact that our current physical theory uses terms such as 'quark' and 'gluon' doesn't mean that there are actually such entities, just that such theories employing such terms provide us with reliable predictions and are consistent with all observable phenomena. For an instrumentalist, theories do not carve nature at the joints, since theories don't provide any direct knowledge of the structure of reality. Thus the terms used in theories are defined by us just to play certain theoretical roles in order to explain observable phenomena and provide reliable predictions, and in that sense can be said to reflect the interests of the scientist rather than any genuine division in nature.

However, Rosenberg's use of the term 'instrumentalism' differs from this standard sense. Rosenberg is not a standard instrumentalist, for he does not hold that theories in 'hard' sciences such as physics merely save the phenomena. Rosenberg is a realist about physics and he wants to distinguish such 'hard' and real sciences from complex sciences such as biology and economics. Rosenberg's claim is that, unlike concepts in physics, the concepts used in biology represent arbitrary gerrymandered categories, which serve useful purposes as far as meeting the needs of biologists, but do not represent real divisions in nature. Rosenberg does not deny that biological entities such as tigers exist. However what he does deny is that there could be a clear-cut way of defining the tiger species, based on a method that could also be applied to defining all the other species.

Thus Rosenberg's claim is not merely an epistemological claim, as a standard instrumentalist's claim is. Instead it is a metaphysical claim, based in the complexity of nature. The difference lies in the direction of fit between theory and nature. For the standard instrumentalist the concepts employed in the theories cannot represent actual entities in the world because, according to the instrumentalist, at best all our theories can do is "save the phenomena". The reason that the theoretical terms do not represent real entities is due to the representative limits of our development of theory – theories cannot truly represent the real world since we have no epistemic access to such an entity. Rosenberg's 'instrumentalism' about biology has things the other way round, for Rosenberg uses his scepticism about theoretical terms in biology as a basis for his instrumentalism concerning biological theory. Rosenberg's scepticism is not driven by epistemic concerns at all, but is driven by metaphysical concerns over the complexity of nature. Given the problems of complexity and the historical nature of species Rosenberg concludes that there can be no clear way of dividing up nature into distinct classes called 'species', and goes on to argue that because of this there can be no laws in biology, and so theories in biology cannot represent the structure of reality. Thus it is nature itself which is a barrier to realism for Rosenberg, and not the limitations of sensory experience that underlie the empiricist's instrumentalism.

Since Rosenberg's form of instrumentalism differs from the standard sense, the persuasive realist responses to instrumentalism, such as Hacking's entity realism[9], have no force. In order to reply to Rosenberg's scepticism regarding biological kinds, another approach is needed, one which takes into account the intractable difficulties of defining species while maintaining a realist conception of species as natural kinds.

2.3 The Importance of Realism in Biology

Rosenberg's claim that biology is an instrumental science is a damning one, because in making such a claim Rosenberg delineates between models in the 'hard' realistic sciences such as physics and chemistry, which represent actual structures and describe laws of nature, and models in biology, which because of the immense complexity of biological phenomena can only be rough and imperfect representations which do not mirror any underlying structure. Thus, even though he explicitly denies this, implicit in Rosenberg's claim is a denigration of theories and models in biology, since they cannot provide the same sorts of explanations as 'hard' physical theories.

[9] Hacking 1983, pp. 262-275.

Rosenberg does not see his criticisms as so damning, for he puts forward the proposal that "each of the disciplines of biology, besides the most general molecular biology and evolutionary theory, is to be viewed as a *case-study*-oriented research program"[10], with different aims and different explanatory ideals to the *universal theory* approach of the physical sciences. This approach is certainly appealing, and is largely in agreement with the conclusions of the previous chapter. Much of biology should be viewed as a science of case studies and as such has different explanatory ideals that are no less 'hard' than those of physics or chemistry.[11] Nevertheless, I will argue against Rosenberg's antirealism, arguing for a realist account of kind terms used in ecological models.

This realist conception, especially in the case of *species*, is important because in sciences such as ecology these natural kind concepts are fundamental. Species feature in many areas of ecology, such as in population ecology, in the study of predator-prey interactions, and obviously in conservation ecology, where the objects of study are generally endangered species. It is only with a realist view on species that discussions on conservation ecology such as Pimm's (1991) can be seen to relate to genuine features in the world. In fact, without a realist view of species it is difficult to make sense of many of the statements on conservation ecology made in the proceeding chapter. Without a realist view of species what do we make of the story of the rediscovery of Leadbeater's Possum in Victoria's Central Highlands? What are biogeographical studies and PVA models concerned with if not objectively characterised populations and species? It is thus important for a workable and realistic science of ecology that ecological concepts such as species are well defined and reflect genuine aspects of nature.

Another reason to take a realist view on species has to do with the problem of defining *biodiversity*, since much of conservation ecology is motivated by a desire to preserve biodiversity. The usual approach is to view biodiversity as diversity in as many levels of biological organization as possible. This can mean diversity at the level of genotypes,

[10] Rosenberg 1985, p. 219.

[11] Rosenberg (1994) also uses the *semantic* conception of theories to support his account, and to defend his antirealism about biological models, as well as to defend his claim that biology is no less 'hard' a science than physics. As discussed in the first chapter, this approach may have clear virtues over the standard *syntactic* approach, especially in its applicability to theories in biology. But to claim that the semantic approach entails antirealism about theory is clearly incorrect since a realist interpretation of the semantic view is quite possible. I will not go into the details of the semantic approach any further here, since it doesn't directly relate to the problem of kind terms – although it is worth noting that the semantic view is perhaps more amenable to the view on natural kinds I defend, since it involves a class of models rather than a single framework.

species, or ecosystems. However, usually prescriptions concerning biodiversity concentrate on species: we maximise biodiversity by maintaining as diverse as possible an array of stable species populations. In order for this particular ethical prescription to preserve biodiversity to be grounded in reality, and not just the whim of conservationists or policy makers, conservation ecologists need to consider the species to be real entities in nature, not just arbitrary gerrymandered categories which suit the needs of biologists. This realist attitude is also especially important for many who hold a so-called 'deep ecology' view on environmental ethics, as this view attributes some sort of intrinsic value to natural entities such as animals, plants, species and ecosystems. For example, a realist attitude to species is central to the discussion of Rolston (1985) on our duties to endangered species. Similarly Callicott, Rolston and other deep ecologists explicitly appeal to the knowledge provided by ecology as a way of underpinning their ethical approach to the environment. This attitude is also not restricted to deep ecologists: Brennan (1986) explicitly looks to ecological theory as a way of giving substance to the claim that we can have duties to the natural world. Thus, for these environmental ethicists, a realist understanding of biological kinds is central to their ethical claims.

It is not only the concept *species* that is important in ecology; there are other terms that are often associated with *species* for which we also desire a realist interpretation. For example conservation ecology research aims at understanding the complex network of relationships that exist between various species and the environmental factors in the regions where they live. Usually when an ecologist is concerned with the preservation of a particular species she investigates the factors in the ecosystem which concern the particular species: the *niche* the species occupies, its *habitat*, its *food web*, the various *energy transfers* involved in the species' environment. Such ecological models certainly aim to be predictively useful, especially in terms of coming up with policy guidelines for management of natural reserves or forestry land. But they also aim for *realism* – the models are attempting to model real processes between real structures in nature.

I take general scientific realism as given, following the detailed work of Hacking (1983), Bigelow and Pargetter (1990), Boyd (1989) and Kitcher (1993). The relevance here is that anyone who accepts these general arguments for realism in the 'hard' sciences should concede this point: that a scientific model that predicts reliably and explains well reflects a genuine pattern in nature. This is certainly a defeasible view of theories, yet it is no more problematic for biology than it is for physics. Thus, contra Rosenberg, we should view successful theory in biology as being representative of real structures and processes.

On this issue of realism in ecology Sagoff (1988) raises some interesting points about the relationship between realism, precision and generality. According to Sagoff ecological modelling involves a trade off between these three factors, and often we must sacrifice realism in order to attain better precision or generality. But Sagoff's point is *not* that we shouldn't aim for realism in our models, for Sagoff clearly believes that good scientific theory should model the real world. His point is merely that being obsessed with detailed realistic modelling, trying to model every causal process as accurately as possible, can make a model so complex that it becomes unworkable. We must always make some sort of trade off, making simplifying assumptions in our modelling in order to create useful models. Yet this doesn't detract from the overall realism of such modelling, because it is still fundamentally based on real world structures and properties – just as we can maintain realism while making idealising assumptions in physics (such as frictionless planes or point masses), we can maintain a realist attitude to our biological modelling.

Thus the importance of a realist view of biological kinds relates closely to the discussion on bottom-up modelling in the previous chapter. For in order to defend a realist view of bottom-up, or localised modelling, we need to have a realist view of the kinds that are the constituents of these models. Thus in order to defend the realism of models in biology, with examples from ecology particularly in mind, we must defend the realism of biological kinds, and in particular, a realist view of *species*.

3. *The Problems of Species*

3.1 Two Problems Concerning Species

There are two related philosophical problems concerning the definition of species. The first is the problem of what the *ontological* status of *species* is: are species *sets* of which the members are the organisms which are of that species, or are species *individuals*, albeit individuals whose parts (the organisms) are spatially distributed discontinuously? Are species *types* with many existing *tokens*, or are they *tokens* whose parts are scattered?

The second question concerns how to define the boundaries of a particular species. It seems that the answer to this question is quite independent of the first question about the ontological status of species, although how the question is framed will depend upon whether you take species to be sets or individuals. If you think that species are sets the question becomes one of defining the membership of the set; if you think species are individuals the question becomes one of telling whether some organism is a part of a particular species.

I will argue that the answer to the first question is relatively insignificant as far as sciences such as ecology are concerned. There is almost a general consensus on this question, with the view that since species are really historically continuous entities they are better characterised as individuals rather than sets (although the label 'individual' is probably not the best, since it may conjure up images of a physically contiguous entity, which species certainly are not). The second question, however, is far more significant, and will be the focus of my discussion. Here I defend a pluralist account of how we should define *species*: the definition we use in a particular context will depend upon the type of investigation we require the concept for. I argue that we may never be able to come up with a unified monistic account, and yet this does not pose a problem for the reality of species.

3.2 The Ontological Status of Species

Since Darwin the Aristotelian conception of species as eternal forms in nature has been replaced by the idea of species as evolving entities. In recent years the idea of species being historically evolving entities presented a problem for the traditional conception of species, which led to a profound challenge to the standard (pre-Darwinian) concept of species by Ghiselin (1974) and Hull (1974; 1978), which has since been defended by many others including Mayr, Sober and Rosenberg. The challenge to the standard conception is that since species are historically evolving entities they are better characterised as *individuals* rather than *sets*.

Intuitions on this point pull both ways. Certainly our pre-reflective conception of what a species is, is that it is a set like any other. In fact, species are often given as examples to illustrate what sets are: "the set of tigers". This seems to work well enough, and we include in the set of tigers just those animals that qualify as tigers: quadruped feline carnivores, having a tawny yellow coat with black stripes. Why should we think any differently? Yet species evolve, and thus are constantly changing: how can dynamic entities be characterised as sets, which are essentially static entities?

The *individualist* is one who takes the evolving nature of species seriously, claiming that this implies that species are better characterised as individuals rather than sets. Their position relies on an analogy: just as we think of an individual organism, such as a tiger, as developing and changing its appearance and structure over time while maintaining a coherent identity, we can think of a species as having a continuous identity over time whilst there is a continuous changing of its parts. Just as the particular cells which make up a time-slice of the tiger are themselves not a defining feature of the tiger, but are just contingent parts of the tiger, on the species-as-individuals view the individual organisms are just contingent parts of the species.

The strongest motivation for the species-as-individuals view is that it appears to accord well with evolutionary theory. If we view species as historical individuals then we can make sense of the idea that species are coherent entities that change over time, can branch into new species, or can become extinct. Over the past few years this Ghiselin-Hull view has generally become the accepted view on the ontological status of species. The main opponent of this view is Philip Kitcher (1984, 1989), who argues that species can clearly be considered as sets of organisms, although the members of those sets do not share a common (non-trivial) property – thus Kitcher also claims that an essentialist view of species is false. Kitcher argues that a construal of species-as-sets is still consistent with the tenets of evolutionary theory, and that anyone who thinks otherwise has a rather narrow view of the mechanics of set theory. For Kitcher claims that the mechanics of set theory are flexible enough to accommodate all that is important about species. I basically endorse Kitcher's main idea, that the two construals of species can be made effectively equivalent – anything that can be explained by the view of species-as-individuals can be translated into the language of species-as-sets, and vice versa.

A standard response to Kitcher's line (as in Mayr 1988) is to point out that once you have used the elaborate machinery of set theory to form the sets that are species, these sets bear little resemblance to any simple intuitive idea of species as sets that we may have begun with. Thus, even though a set theoretic formulation of species may be *possible*, the view of species as individuals is far clearer and accords better with our conception of species as historically evolving entities.

One difference between Kitcher's view and the individualist's is that Kitcher claims that the assumption that species are historically connected entities is far too strong, and he cites an example to support this. The case is where two species of the lizard species *Cnemidophorus* hybridise to form a new species. The new species then becomes extinct, but a later identical hybridisation of members the original two species produces a new species whose members "fall within the same range of genetic (morphological, behavioural, ecological) variation"[12] as the original hybridised species. Kitcher's intuition is to consider this second hybrid the same species as the first, even though there is no direct historical continuity between them, and on this point he seems correct. For if the original hybridised species had not become extinct, and later an identical hybridisation of the two original species occurred, we would clearly consider the new hybrid the same species as the original, for there seems to be no reason for considering them to be a new species.

However, despite this example, it seems that this difference does not favour either side of the argument, for the individualist need not be committed to the idea that species are historically connected entities in such a strong sense. Just as branching is possible for a species, so is a type of 'fission' in which two seemingly different identities are in fact just parts of the same entity – i.e. a single species may involve discontinuous elements of a genealogical tree. In fact, following Kitcher's argument, it is likely that any formulation of the species-as-individuals thesis can be translated into the language of species-as-sets. Thus there is no correct answer to the question of whether species are sets or individuals: they can be represented in either form. It could be argued that a parsimony argument will favour the individualist – the elaborate set theoretical tools required to classify species as sets is unnecessarily complex compared to the species-as-individuals view. However the idea of parsimony providing good grounds for accepting one view over the other is something I am generally dubious about, since I reject the idea that simplicity and beauty necessarily lead to truth.

Perhaps another reason for favouring the individualist view is that if we view species just as sets then we could easily misconceive the true nature of species – but this does not in itself rule out the species-as-sets thesis. Interestingly, David Hull, one of the strongest proponents of the species-as-individuals view, has since largely conceded Kitcher's point, although he still favours talking about species as individuals: Hull points out that the types of sets needed to encapsulate the species-as-individuals hypothesis are quite different to standardly

[12] Kitcher 1984, p. 315.

conceived sets, and that talking of individuals as sets can confuse the distinction between a group of things and a single entity composed of parts.[13]

Thus, for my purposes, this debate is actually quite peripheral. The more important problem I am concerned with is the problem of defining species, as well as higher taxa, and of explicating a realist view of these important kinds. On this point both realists and anti-realists have appealed to the individualist conception to support their view. For example Mayr (1976; 1987; 1988), Hull (1999), Griffiths (1999) and Sterelny & Griffiths (1999) clearly favour a realist approach, while Rosenberg (1994), Stanford (1995) and Ereshefsky (1998) advocate instrumentalism. The problem of the ontological conception of species does not really affect the more practical problem of distinguishing species. All it affects is how we phrase the questions: If species are individuals, how do we individuate them? If they are sets, how do we define them?

There is, however, a sense in which the ontological question is important in terms of defining species, although in this case it is not a question of whether species are better represented as sets or as individuals. The substantial question is in fact a question of whether species are essentially historical lineages, or whether a non-historical conception of species is possible. Kitcher and, as I discuss further on, Dupré seem to be the main voices of dissent to the dominant view that species are essentially historically continuous lineages, since they argue that there are robust classes of organisms that should be classified as species but do not count as lineages – groups of organisms united by ecological properties being the main examples. This dispute actually has more to do with the problem of defining species, rather than the standard debate over the ontological conception of species, since it concerns the question of how species should be defined. Thus I will now turn to this more substantial problem.

3.3 Definitions of Species

Since the recognition of the evolving nature of species, the idea of species being united by a common set of internal properties has been rejected. Now although there are multiple and conflicting definitions of species used throughout biology, it is clear that there can be no formulation of necessary and sufficient conditions for an organism to be a member of a particular species that applies across the range of biological theories.[14] The rejection of

---

[13] Hull 1999, pp. 31-34.

[14] See Rosenberg 1985, Chapter 7; Dupré 1993, Chapter 2; Kitcher 1989.

essentialism is the common element of all the generally accepted rival conceptions of species. Thus species are not defined in terms of their members bearing common properties, or some common *eidos*. Rosenberg (1985) describes the argument against essentialism thus:

> The crucial feature of the theory of natural selection is that the unit of evolution is the *species*: it is they that evolve. Their evolution consists in the changes in their relative proportions with which their members in successive generations manifest the varying hereditary characteristics, or phenotypes, determined by changes in the units of heredity and the forces of selection. This explains both why biologists can provide no necessary and sufficient conditions for various particular species and why there seem to be no exceptionless generalisations about particular species. Because species evolve, there is no trait that jointly meets the requirement of being hereditary and the requirement of being either necessary or sufficient for species membership through the course of its evolution: There are no essential properties of species.[15]

The above reasoning shows why any simple appeal to essential properties to define species will be inadequate as a way of accounting for a species' continued existence as it evolves. The general point is that since the particular properties of a species are constantly changing any definition purely in terms of properties will not be sufficient. Also, since there is much genetic diversity *within* species as well as genetic diversity between species, and given the complexity of the genetic material, a purely genome based definition of species is problematic, if not impossible. In simple terms, we cannot say that an organism is X if it possesses genome *g*, since random mutations within the species may result in offspring that do not possess genome *g*, but should clearly be considered part of the same species because of morphological and behavioural similarity.

So where do we go from the failure of simple essentialism? The approach over recent years has been to delve into the many processes that underlie our intuitions about what unites species, resulting in a literal plethora of rival species concepts. As David Hull says, "We are drowning in a sea of species concepts."[16] For those brought up on a diet of twentieth-century empiricist philosophy, those who sharpened their wits with Occam's razor and acquired Quine's "taste for desert landscapes"[17], the recent population explosion of approaches to defining *species* must make it seem like biology is becoming the overcrowded ontological

[15] Rosenberg 1985, p. 205.
[16] Hull 1999, p. 44.
[17] Quine 1953, p. 4.

slum that Quine so abhorred. Perhaps for others, with more of a taste for the richness and diversity of the rainforest, or an understanding of the true richness of desert ecosystems, this profusion is to be welcomed. Whatever our attitude, it is clear that the zoo of *species* concepts seems to be breeding and mutating at an alarming rate. For example, the papers in one recent collection (Ereshefsky [ed.] 1992) discuss many different conceptions of species: biological, ecological, morphological, phylogenetic, cohesion, and recognition accounts of species, just to name the most important ones. Sterelny and Griffiths (1999) divide these into five distinct types, each with numerous variations; these authors also mention that there may be more than twenty modern species concepts out there. Mayden (1997) provides a thorough taxonomy of the various species concepts in use at the time, listing 22 distinct species concepts. So which is the correct view, is there a single correct view, or is there no correct view at all?

Here I shall discuss only a few important conceptions of species, aiming to point out that the fundamental barrier to a unified species definition is the conflict between the Aristotelian *synchronic* conception and the Darwinian *diachronic* conception of species. The idea is that we want our definition to reflect our pre-theoretical ideas about species being stable sets of entities united by common properties, a conception which is *synchronic* since it is concerned with these properties at a given time. But we also wish to incorporate the *diachronic* evolving nature of species in our definition. I shall claim that this shows that there may be no unified definition of species, but that this does not preclude the possibility of taking a realist attitude to the existence of species.

Phenetics

The first approach I discuss is called the *phenetic* species concept (Sokal and Sneath, 1963). I discuss this approach because it was originally touted as an objective, clear-cut way of dividing the natural world into distinct species in a way that accords well with Aristotelian intuitions. But as it was a method entirely divorced from the evolutionary nature of species, as well as being at root antagonistic to a realist view on species, it has since fallen into disrepute.[18]

[18] The rise and fall of phenetics is a fascinating tale which David Hull (1988) tells: this often sordid history of the 'debate' between the pheneticists and their opponents provides an excellent story as well as an interesting example of a power struggle between groups of scientists following different research programmes.

Phenetics is a system of classification based upon morphological similarity. Pheneticists divide up the range of organisms by defining a set of morphological characteristics and assigning weightings to the characteristics. Each organism is then given a 'score', and organisms with similar scores are said to belong to the same species. This numerical approach to taxonomy is based on a positivist view of science – the view that we can only classify things by reference to observable properties. As such this method attempts to be devoid of any theoretical commitments.

In practice this method initially seems to work fairly well, in that it divides up the natural world into distinct groups in a relatively unambiguous way. Once we have determined the list of relevant features and weighted their importance the task of classification becomes just a glorified stamp collecting exercise. However, despite claims by eminent thinkers such as Smart (1963), classification in biology is not so simple and there are significant problems with the phenetic approach to classifying species. Firstly the range of relevant characteristics, and the weightings to assign to them are determined in a largely ad-hoc way. What is chosen as a relevant characteristic, and the weight given to that characteristic, is a choice we make from an infinity of possibilities – thus a natural classification based purely on morphology is fraught with problems. What makes one feature more natural and more significant than others? Is there a distinction between a genuinely natural and an arbitrary feature? For example, why consider the number of appendages of an organism to be significant and not eye colour?

In order to build their objective system of classification the pheneticist must answer these questions. There may be ways around this problem, but the solutions must inevitably appeal to more than just morphological features, thereby undermining the theory-neutral basis of the phenetic approach. Effectively these questions raise the same problems that the phenetic approach was trying to avoid, moved down to the level of features rather than at the level of organisms. The theory dependence of observation spells doom for this purely theory-independent approach: the fact that observation itself is not theory independent means that it is impossible to formulate a theory-independent categorisation of species based purely on observable properties.

There are other problems for this approach. Since this method has no connection with the evolution of species and the process of speciation, its usefulness in explanatory contexts is problematic. It may provide a way of determining the extensions of particular species at a

particular time, but given that it has no connection with evolutionary and ecological properties even if this method gets the right answers this in no way implies that it provides the correct view of species. There are also other problems for this approach, such as the existence of sibling species – species that are morphologically identical but have important behavioural or ecological differences.[19] A purely morphological approach cannot distinguish these species, whereas there are often good reasons for believing that they are quite distinct. For example, recognition of the different sibling species of *Anopheles* can provide us with an effective understanding of the spread of malaria (Lane 1997).

The moral here is that a theory-free approach to classification leads one away from a definition of species that is of any use in biology – the *species* concept is essentially part of our body of biological theory and as such involves theoretical commitments. So the question becomes one of what we should commit ourselves to when it comes to defining *species*.

Biological species definition

Up until recently the most promising single approach to defining species was some form of Ernst Mayr's *biological conception* of species (Mayr 1942, 1963, 1976, 1988). Mayr's original motivation for this definition came from looking at the process of speciation itself as a way of defining species. This sees new species developing through a process of geographic isolation followed by an evolutionary development that reproductively isolates the geographically isolated population, so that that population cannot interbreed with the population it was originally separated from. The *biological conception* of species says that organisms that can reproduce successfully and are reproductively isolated constitute a species:

> Species are groups of actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups.[20]

Mayr presents the later versions of the biological species definition as being a way of explicating the view of species as historical entities. The biological definition gives us a way of understanding species in their evolutionary context, as entities that are the result of a process rather than a static, unchanging entity. As such the *biological conception* accords well with the Hull-Ghiselin approach of viewing species as complex evolving entities,

[19] Mayr 1976, pp. 509-514.
[20] Mayr 1942, p. 120.

especially if one thinks of a species as being a *gene pool*: in that way species can be conceived as being continuous entities that evolve over time, and also as classes of organisms at any particular time.

A good initial test of any species definition is to see how well it works for biologists (although there is a tendency amongst biologists to overemphasise the importance of such operationalism). Mayr claims that the biological species definition is one that accounts for more known cases than any other approach, and has been extremely useful in resolving disputes. He also claims that taking species to be reproductively isolated populations means that species are generally quite easy to discover and delineate. However in reality this is not such an easy task, with there being some important examples of problem cases for this definition.

The most obvious problem for the biological conception of species is the existence of asexual species. Since each asexual organism is reproductively isolated from every other on the *biological conception* either each single asexual organism is a species in its own right, or else it is clearly not part of any species. There are those who just bite the bullet at this point, claiming that asexual organisms never form species (eg. Ghiselin 1987, Mayr 1987). However such an approach is not only counter to our intuitions on species, it also faces serious problems in the face of examples of hybridisation which give rise to a unisexual species from bisexual ones – the lizard hybrid *Cnemidophorus tesselatus* in the example of Kitcher's discussed earlier exhibits this property (Kitcher 1985). Templeton (1989) provides further argument that we shouldn't ignore nonsexual taxa, arguing that to do so not only involves excluding many easily recognisable subdivisions of nature, but also necessitates excluding organisms that make up populations of closed mating systems, for example self-mating populations such as barley. Thus the problem of asexual taxa is a serious one, and one which the *biological conception* cannot adequately deal with.

An alternative response to the problem of asexual species is to take a dualistic approach to species definition, applying the biological definition to sexual species and morphological similarity or genotypic similarity as a way of defining asexual species.[21] However, this solution appears problematic given that the division between sexual and asexual organisms isn't a sharp one, especially in the case of plants. There is also a more general problem – that

[21] Rosenberg 1985, pp. 191-197.

reproductive isolation is often weak, especially in plants, and thus provides a very unclear way of defining species.

Other problems for this account include the point that sexual species originated from asexual species (such as the lizard hybrid *Cnemidophorus tesselatus)*, which reveals the weakness of this account in broader explanatory frameworks. The example of ring species, such as the populations of gulls that circle the Arctic[22], also presents a problem for the biological definition. With ring species, each neighbouring population can interbreed with each other but cannot interbreed with more distant populations. On the biological account it is impossible to say that all the populations are part of a single species yet there is no clear way to say which populations form the distinct species.

Added to these problems is the fact that adopting the biological definition commits one to the thesis that reproductive isolation is the sole cause of speciation, a thesis that is most certainly under dispute. In particular the biological definition ignores the possibility of anagenesis, where a single species evolves into a single new species via a continuous line of descent. However the biggest problem with the biological definition is that it doesn't actually provide a clear and objective way of distinguishing species. One reason for this is the fact that reproductive isolation is often weak, and thus we inevitably have to make arbitrary choices about how fine-grained we look at the evolutionary tree. Another reason concerns the vagueness of the term 'potentially' that features in the definition: in what sense should we allow populations to [potentially] interbreed? Should we consider brushtail possums in Tasmania to be a distinct species from those on mainland Australia, just because they are geographically isolated? What about possums in New Zealand? Are thoroughbred racehorses a distinct species from other horses just because they happen to be reproductively isolated by rigid breeding conventions? If pigs fly into space, never to return, should we consider them a new species? If so, at what point do the space pigs diverge?

Due to these serious problems taxonomists often have to refer to factors such as morphological similarity, or similarity in ecological traits, in order to delineate species, or else just make arbitrary decisions. Because of such facts Rosenberg sums up his scepticism about the biological species definition:

[22] Dawkins 1993, p. 82.

> A stark way of putting it is that the biological species definition does not carve nature at the joints. It does not provide a uniform, general, easily applicable way of identifying the natural kinds into which living organisms fall. Employing it will require arbitrary decisions that cannot be justified by biological theories already in hand, decisions in taxonomic classification that may differ from naturalist to naturalist and will not cite the explanatory basis of speciation.[23]

Thus we have come back to Kitcher's cynical definition and seem little closer to a realist view of species. The *biological conception* is too vague in its formulation to give us an unambiguous way of delineating species. The concept of *reproductive isolation*, upon which the definition centrally depends, is itself too fuzzy around the edges to be clearly applicable in many important cases. In the end this leads to arbitrary decisions being a part of the taxonomic process. And all the way we have excluded entities that seem to be quite clear cases of species: those of asexual organisms.

Phylogenetic species definition

An alternative to the *biological conception* is the *evolutionary* or *phylogenetic* conception of species (Cracraft 1983; de Queiroz and Gauthier 1992). This approach largely follows the *cladistic* school of biological taxonomy defining *species* in terms of the evolutionary lineages of organisms, with the basic idea that members of a species all share a common descent: on this view taxonomy should reflect genealogy. Simpson gave an early and quite general form of the definition as follows:

> An evolutionary species is a lineage (an ancestral-descendant sequence of populations) evolving separately with its own unitary evolutionary role and tendencies.[24]

This genealogical approach seems promising as a way of separating sets of genotypes into discrete sets, since looking at a genealogical tree will reveal fairly distinct 'clumps' of organisms.

There are certainly similarities between this approach and the *biological conception* since both definitions define species in terms of speciation events, although the *phylogenetic conception* allows other factors to be relevant rather than just reproductive isolation. The

[23] Rosenberg 1985, p. 194.

[24] Quoted in Rosenberg 1985, p. 197, from Simpson, G. G. 1961, *The Principles of Animal Taxonomy*. New York: Columbia University Press.

relevant factors here can be any factors that cause a branching in line of descent in the 'tree of life'. However the *phylogenetic conception* is clearly different from the *biological conception* since there are examples of biological species that do not qualify as phylogenetic species and vice versa. Ereshefsky (1998) gives the example of asexual organisms for phylogenetic species that are not biological species (although he not does solve the problem, mentioned below, of giving an account of how such organisms form species on the phylogenetic conception). The example of biological species that are not phylogenetic species are ancestral species: reproductively isolated populations descendant from a common ancestor, which form a single phylogenetic species but multiple biological species.[25] Following these examples Ereshefsky notes "These two approaches to species ... carve the tree of life in different ways".[26] Thus the two approaches are genuine rivals, so how do we choose which is the better, or do we have to make the choice at all?

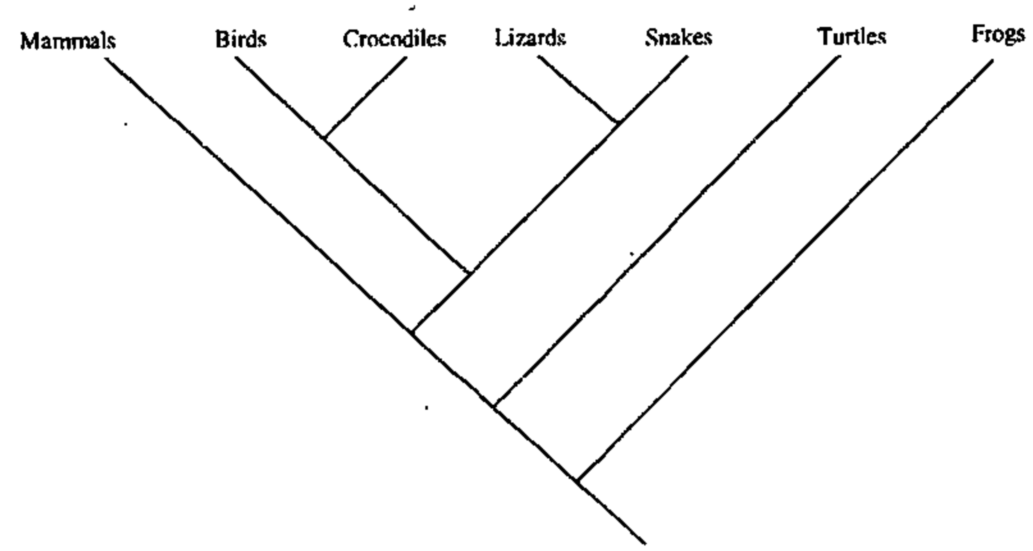
There are significant problems with the *phylogenetic conception*. One problem is that without an independent account of what unites members of an asexual species on this view, asexual organisms can never be members of species: there will be no 'clumping' (or branching) in their genealogical trees since the individual organisms are each part of a distinct lineage. As argued above this problem is a serious one since there are numerous reasons for wanting asexual organisms to form species. Yet this is something both the *biological conception* and a simple version of the *phylogenetic conception* cannot account for. Thus we need some further account of what unites species in order for the *phylogenetic conception* to be of widespread use.

Another problem with the *phylogenetic conception* is that it is not clear how far into the grain of the evolutionary tree we must go to get to the level of species. Obviously we don't want to go all the way down to the level of the individual organism, for *species* certainly exist above the level of 'family units', but how far up do we go? Making this decision requires us to define what makes a particular segment of a branch of the evolutionary tree stand out, thus we must somehow define what makes a group of organisms cohere as a species as well as giving an account of when speciation occurs. But in doing this we must look at more than just historical relationships: we must add some concept like the *biological conception* in order to separate the 'clumps'. On its own the *phylogenetic conception* is far too vague about the process of speciation to be of any practical use.

[25] Ereshefsky 1998, pp. 105-6.

[26] *Ibid.*, p. 105.

There is also the question of how strictly a *phylogenetic conception* should stick to the cladistic idea that species are strictly *monophyletic* – that species consist of all and only the descendants of a particular ancestral species. Monophyletic groups are contrasted with *paraphyletic groups*, groups which are descendant from a single ancestor species but don't include all of the ancestors, and *polyphyletic groups*, which are groups of organisms with no single common ancestor.



For example, in the above cladogram[27] the group birds-crocodiles-lizards-snakes is monophyletic, whereas the group lizards-snakes-crocodiles is paraphyletic and the group mammals-birds is polyphyletic. This is why strict cladistic principles entail that we should reclassify birds as a subgroup of the class Reptilia. Strict cladists state that all biological taxa, including species as well as higher taxa, must be monophyletic. However a more flexible approach to the *phylogenetic conception* is possible, whereby definitions of taxa must merely be consistent with the pattern of descent represented in a cladogram. Allowing a more flexible approach here means that we can, for example, allow the class of birds to be distinct from Reptilia, which seems more a more appropriate classification for ecological studies.

Kevin de Queiroz (1999) clearly illustrates these differences in phyly for definitions of species, arguing that monophyletic, paraphyletic and polyphyletic species can all be accommodated under the general *lineage* concept of species, as in Simpson's original

[27] Taken from both Griffiths 1997, p. 207, and Sterelny & Griffiths 1999, p. 198.

definition quoted above. The point de Queiroz makes is that all rival species definitions are versions of the general lineage concept, since it seems reasonable to hold that any definition of species must be compatible with evolutionary theory. Thus de Queiroz claims to have solved the species problem, since he has provided a definition of the species concept. But de Queiroz's approach still leaves open the question of which types of lineages can form species, what processes should count as speciation, and how we should divide up the tree of life. Thus the real problem of defining species has not been solved by this "solution", since it does not answer these substantive questions.

Another objection to the phylogenetic approach as a method of classification of species is that although this approach may provide us with a way of separating organisms into classificatory units, it tells us very little about the higher order biological properties of those organisms, and thus tells us very little about many of the complex relations that exist between the different classificatory units (Dupré 1993). This point is a critical part of a defence of a pluralist view on species, which I discuss in the next section.

Thus the *phylogenetic* approach will not necessarily provide a basis for explaining the ecological and behavioural coherence of members of a particular species, because it only involves strict genealogy. It may not even divide organisms into groups with similar ecological properties, since genealogy alone does not necessarily reflect higher order properties. Thus as far as explanatory usefulness in broader non-evolutionary contexts goes this purely *diachronic* definition is lacking.

Ecological species definition

The main problem with the *biological conception* was that it presumed that isolation was the only cause of speciation. The *phylogenetic conception* allowed there to be other mechanisms whereby speciation occurs, but required a more precise account of this process in order to provide us with a workable conception. Just looking at species as lineages is not enough, since this leaves open the possibility of many interpretations of how to segregate the lineages. Thus a purely *diachronic* conception of species will not suffice: the only approach is to include *synchronic* factors in our definition, factors which tell us what unites a particular species at a particular time. One such approach, the *phenetic* approach, has been discussed and rejected due to its failure to reflect an adequate and realist view on species. The *biological conception* is also essentially a synchronic approach, since it is based on the relational properties of organisms at a particular time (although the problems of assessing the *potentially* interbreeding populations complicate this somewhat, since this involves extending the concept to cut across time). However the problems associated with this approach show that its effectiveness as a workable species concept is limited.

One more promising approach along synchronic lines is to consider the particular relationships between organisms, groups of organisms and their environment, looking at these relational properties as defining distinctions between species. This is an *ecological conception* of species, a view first explicitly formulated by Van Valen (1976) as an improvement on both the biological and phylogenetic conceptions. This conception views a species as a group of organisms that occupy a particular ecological *niche* in an ecosystem. In one sense this definition can be seen as a refinement of the biological definition, using the concept of an ecological niche as a way of defining isolation. One major advantage of this approach is that it solves the problem of defining asexual species, since an asexual species can be viewed as a group of organisms that occupy the same ecological niche. It also does away with the problems of defining *potentially* interbreeding populations since on this view it is niche occupation that unites members of a species, not interbreeding.

Yet this approach also has its share of significant problems. The major difficulties faced by the ecological definition of species have to do with its reliance upon the problematic concept of a *niche*. If species are to be defined by niche occupation, then the concept of a niche cannot legitimately be defined in terms of species. Coming up with a non-circular definition has not been an easy task but, as stated earlier, this is not an insurmountable one. However even if a niche is defined independently of species, for example in terms of "the occupation of a hypervolume of a phase space whose dimensions represent all environmental factors acting on organisms"[28], it seems that to define a species in terms of occupying a niche is too restrictive a definition. It is plausible that two different species can occupy the same ecological niche, as with some sibling species (Mayr 1948), or that a single species can occupy distinct niches, being able to switch roles. Thus defining species in terms of ecological niches has things the wrong way around: we cannot make sense of the idea of a species occupying a niche without first defining what a species is.

Another problem with the above approach is the fact that such a definition is largely divorced from the conception of species as evolving entities. This is because it is based on

[28] Griesemer 1992, p. 238.

the concept of a niche that is essentially a synchronic concept rather than a diachronic one – it appeals to factors at a particular time rather than factors across time. Unless we can incorporate a diachronic element into our definition of a niche, such an ecological approach cannot adequately account for the evolving nature of species.

The problems of defining species in terms of niches are further emphasised by Lewontin's (1991) discussion on the relationship between organisms and the environment.[29] Lewontin pointed out that the ecological environment of an organism is partly shaped by the organism itself, thus the niche is in some way a product of the occupant's behaviour and biology. As Sterelny and Griffiths point out, "If organisms both make and are made by their environments, then we need a transformation of the idea of a niche."[30] Following Lewontin's approach, and marrying it with the traditional approach to defining niche, Sterelny and Griffiths propose a version of the niche concept that incorporates diachronic historical and developmental considerations.[31] Yet in doing so they are left with a concept of niche that, on its own, is of little help for delineating the species, since it presumes we already have a way of distinguishing the higher clades. Since (as I discuss later) their solution to the species problem involves taking a fundamentally phylogenetic approach, and supplementing it with the biological and ecological accounts where required, this is not a problem for these authors. But it does show that as an all-encompassing approach to defining species the ecological approach fails to deliver.

More generally, the tension between the Aristotelian synchronic and Darwinian diachronic factors involved in the definition of species underlies a more fundamental tension between ecology and evolutionary theory. The problem is that ecology is concerned with ecological systems, the forces that bind them and act on them, and how such systems change – such studies are generally conducted over relatively short time periods. In contrast, evolutionary periods are generally far greater time scales. To put it crudely, ecological studies tend to consider species as being static entities, whereas evolutionary studies concern the dynamic nature of evolutionary change. Thus although ecological studies are not, strictly speaking, synchronic, since studies such as PVA modelling on Leadbeater's Possum look at the changing state of populations over time, there is still a strongly synchronic element to these approaches. This is evident in the fact that factors such as genetic drift are usually excluded

[29] Also see Levins & Lewontin 1985; Sterelny & Griffiths 1999.

[30] Sterelny & Griffiths 1999, p. 268.

[31] *Ibid.*, pp. 272-276.

from ecological studies – it has been exceedingly difficult to include evolutionary factors in ecological models. For example, in their PVA modelling of Leadbeater's Possum, Lindenmayer and Possingham note that it would have been impossible to complete the large number of scenarios explored in their study if they had not assumed that genetic factors were negligible.[32] Thus, in effect, a synchronic account of species is used in ecological studies, rather than an evolving diachronic account.

The problem is that once we start focusing upon ecological traits of organisms as a way of defining species, we begin to lose some of the genealogical facts that are relevant to the process of speciation. The synchronic and diachronic factors that underlie our intuitions about species seem to pull in opposite directions. What the ecological approach does add, however, is a clear way of delineating species so that they can be explanatorily useful for ecologists in their particular studies, even though the concept may have little use in broader, longer-term evolutionary contexts.

3.4 The Definitional Conflict – Embracing Pluralism

Each of the conceptions discussed above have significant problems dealing with the full range of organisms that seem to be obvious candidates for species. Many have deemed these problems and inconsistencies intractable, and have concluded that rather than try to come up with a unitary conception of species we should accept a pluralist view, on which there is no single overall classificatory scheme for species. The motivation for pluralism comes from the fact that the different approaches all capture something significant about the species concept yet they seem at root to be irresolvable into a single unitary concept. A pluralist view accepts the strengths and difficulties of the above definitions, and provides a way of keeping the heuristic power of the various definitions without being committed to a single mode of classification.

At this point another approach may seem more appealing, and more in line with the methods of science that have served us so well. This is just to keep plodding on, looking to solve the problem of defining species in a clear-cut monistic way. Although there is no unifying definition of species at present, this does not show that we can never hope to find a unified, useful and explanatory definition of species. The history of science is a history of conceptual development and progress, with confusions and terminological inconsistencies being ironed

[32] Lindenmayer & Possingham 1994, p. 17.

out by theoretical breakthroughs and the development of consensus amongst scientists. The history of chemical classification is such a story, with initial widespread confusion eventually leading to a simple and unified picture in the form of the periodic table. So it seems reasonable to assume that the problems of classifying species will similarly disappear over time.

This idea, however, is misguided, for reasons that strike deeply at the assumptions of those who favour Duhem's "French mind" view of reality. For as I have pointed out, it is a bold and unjustified assumption that the world can be encompassed by simple, unitary theory. More importantly, it is an assumption we can clearly do without. Already our best scientific theories involve kinds that are defined in a pluralist way: pluralist kinds are an important part of their explanatory and predictive content. This is especially true for the biological world, although later in this chapter I provide an example from chemistry to further support this view. But for now I shall remain focused on conceptions of species, arguing that it is plausible that a unitary definition is impossible.

In striving for a unitary approach some have taken the somewhat 'heroic' approach, latching onto one of the above approaches and accepting the counterintuitive problems with their preferred approach. I have already mentioned that Ghiselin (1987) and Mayr (1987), two of the staunchest defenders of the *biological conception,* accept the problematic conclusion that asexual organisms do not form species. With similar dogmatism the staunch pheneticist claims that their method is the only plausible method for a workable taxonomy, thereby accepting the conventionalism associated with such an approach. For example, Sokal and Crovello (1970) replace the species concept with that of "operational taxonomic units", and admit that in some cases "the criterion of phenetic similarity to be employed is necessarily arbitrary"[33], even though this clearly undermines the original operational motivation for their approach. In these cases the strongly counterintuitive consequences of taking on these monist approaches seem reason enough to reject them.

More recent attempts to define species under a single concept, such as the lineage conception of species of de Queiroz (1999) and Sterelny and Griffiths (1999) – "species are lineages

[33] Sokal & Crovello 1970, in Ereshefsky (ed.) 1992, p. 41. Sokal & Crovello ultimately defend a very broad version of phenetics that incorporates many factors, adding biochemical and behavioural similarity to the standard idea of physical similarity, as well as adding ecological properties to the mix.

between speciation events"[34] – initially seem like monistic accounts since they involve a simple categorisation of the species concept. However identifying species as lineages does not necessarily lead to a monistic account, since we may accept that there are multiple ways of dividing up the tree of life into separate lineages which all have enough of what it takes to be identified as a genuine species. A monistic account based on this idea may still be possible: Sterelny and Griffiths in particular endorse a fundamentally phylogenetic account, with speciation defined by a combination of the biological and ecological accounts. Thus while Sterelny and Griffiths maintain a pluralist position on speciation – holding that there is no single account of speciation, and that we need multiple accounts in order to identify all the species – they seem to hold a monist position on species: it is a monist account because it is compatible with the tree of life having a unique decomposition into species. Just how this can and should be done in practice they only hint at, their account being a rough sketch that clearly needs further development.[35]

Thus, although I will not rule out the possibility that, some time in the future, a monistic account of species will be given, it is fair to say that at present no adequate monistic account can be given. Furthermore, the tensions between process and structure, between the synchronic and diachronic aspects of species, and between evolutionary theory and ecology, seem to leave as quite real the possibility that our best account of species will be a multiplicitous one. As such we should seriously embrace the possibility of pluralism.

[34] Sterelny & Griffiths 1999, p. 192.

[35] There does seem to be a tension between Sterelny & Griffiths' approach to defining species and their view on the concept of a niche that I outlined previously. In discussing species they endorse an essentially cladistic approach, and then acknowledge that this approach may leave doubt as to the validity of higher order taxonomic categories:

> Within [evolutionary or phenetic] taxonomic pictures, the idea of genus, family, order, and so on makes quite good sense. If cladism is the only defensible picture of systematics, the situation is more troubling. From that perspective, these taxonomic ranks make little sense. (Sterelny & Griffiths 1999, p. 201)

Yet when they later discuss niches, they explicitly rely on the idea of a higher order clade:

> We think the solution may be to construct an account of niches intermediate in generality between Lewontin's niches, whose dimensions are generated by the life history of the particular species, and the classic idea of a niche as a role in a community of a particular type. We can extract niche dimensions not from the species in question, but from the larger clade to which it belongs. (Sterelny & Griffiths 1999, p. 273.)

If the definition of larger clades is somewhat arbitrary, this presents problems for their approach to niches. Thus in order for Sterelny & Griffiths to solve the problems of both species and niches, they must qualify their endorsement of cladism, in order to allow the existence of higher order clades.

4. Pluralism

4.1 A Return to Babel?

Many find the idea of pluralism a counterintuitive and frightening prospect that will lead to a world of confusion, relativism and ultimately, meaninglessness. Kitcher himself says, "the most obvious worry about the pluralism that I recommend is that it will engender a return to Babel, a situation in which biological discourse is plunged into confusion."[36] David Hull (1999) takes this worry most seriously, arguing against the plurality of species on what seem like strongly prejudiced anti-pluralist grounds. Hull equates pluralism with a position on which there is ultimately no correct view in science, just a plurality of views that are equally valid. Thus Hull sees the person who asserts a pluralist view of species as being somewhat inconsistent in their views because they in fact hold a *monist* view – that *the* correct view of species is a pluralist one. Fortunately for the pluralist, the only one confused by pluralism here is Hull. Just because someone holds a pluralist view in one framework doesn't mean they must hold a pluralist view universally. For example, holding a pluralist view on species doesn't entail holding a pluralist view on all natural kinds, for there may be particular reasons for holding a pluralist view in one context and not in another.

To help explain this, consider the following example, the question: "What is the best way of getting from A to B?" – let's call this question Q. I am a pluralist about the answer to Q because I hold there may be no best way. There are multiple ways to get from my home to Monash University, and not one of them is the best way. Some are clearly better than others – driving is better than walking – but I can't give a final answer without knowing more detail about the question. I'll just say, "It depends ..." When I have more information about the context of the question, I may be able to answer it more clearly, cutting down the number of relevant answers and possibly coming up with the best answer – if you like cycling, have a bicycle and the weather is good, then the best way is to ride a bike; if you are a quadriplegic with a wheelchair, then the best way is by taxi. However in other contexts I may still be left with a range of equally valid answers.

While still maintaining my pluralism about the answer to the general question Q, for other versions of the question I can consistently claim Q could always be answered in a simple way. There is clearly a best way to get from my house to the airport – by car on the direct

[36] Kitcher 1984, p. 326.

and fast road. This answer applies no matter the context, since all the alternative means of transport are clearly less effective (too far to cycle or walk, terrible public transport, etc.).[37] Thus I can consistently hold a pluralist view on question Q in one context, and a monist view in another.

This example shows that a pluralist outlook doesn't entail relativism or confusion, and it doesn't even entail that pluralism is actually true in every case – we just start out open minded about the possibility of pluralism. We can still distinguish between some better and worse answers to our questions, with increasing accuracy as we come to know more about the context of the question.

In a similar way, a pluralist view on species doesn't entail that there are no better or worse definitions of species. Thus a pluralist view of species need not be a relativist one. All that it involves is accepting the possibility that there is no way of coming up with a universally applicable, monistic account of species. We should not be frightened by the possibility of natural kinds pluralism, and species pluralism in particular. We shouldn't reject pluralism outright just because it is pluralism, as Hull does. Of course, we do need to have a pluralist account of species (and natural kinds more generally) that doesn't collapse into the sort of relativism that Hull fears. Thus I outline and defend such an account later in this chapter.

If pluralism is a return to Babel then, if that is the way things must be, we should accept the situation and learn to adapt. In fact there is good reason to believe this is already happening in the biological sciences: following Kitcher's previous quote about Babel he goes on to say that "biology has already been forced to cope with a different case of the same general problem, and … has done so successfully."[38] Thus we should openly embrace the possibility of pluralism. For after all, the world we actually live in is in fact the world of Babel which Hull fears we may return to, a world with multiple languages and multiple frameworks. Yet this is not only a world in which communication is possible, it is a world far richer in terms of what we can say, what we can communicate, and what we can *know*, than a world with a

[37] Strictly this isn't quite true, since it is possible to dream up an example in which one wouldn't take a car to the airport. For example, if the person had a deep ideological objection to cars as a mode of urban transport, and insisted on using public transport, or cycling everywhere. Or, perhaps, in the more far-fetched context in which someone could teleport at will, and could instantaneously transport themselves to the airport. This does not undermine my point at all – in fact these examples further emphasise the need to maintain a broadly pluralist outlook for questions such as these.

[38] Kitcher 1984, p. 326. Here Kitcher is referring to the problems of defining the term *gene*, which he argues is another biological concept that is impossible to define monistically yet still plays a useful role in many scientific contexts.

single language would be. Rather than fear a return to Babel, we should marvel at the power and knowledge that such as world can give us, and make the most of its richness and complexity.

4.2 Degrees of Pluralism

The term *pluralism* is an ambiguous one: there is a distinction between stronger and weaker forms of pluralism, and positions that are sometimes labelled as pluralist are not clearly so. Take for example the 'pluralist' position on *species* on which there are multiple approaches to the definition, which depend upon the properties of the organism in question, yet there is still only one overall classificatory scheme. This position is actually better labelled as a *disjunctive monism* – the account is disjunctive because it is based on multiple accounts of speciation, but is monist because there is still a unique classificatory system. For example, those who hold a biological view about sexual species together with an ecological view on asexual species have a *disjunctive monist* view on species. It is this form of monism that Sterelny and Griffiths (1999) are committed to since they appeal to multiple accounts of speciation in their approach, but still adhere to the idea that there is only one way of dividing up the tree of life. The so-called 'pluralism' of Mishler and Brandon (1987) is also of this weak variety since they hold that each organism is part of precisely one species; there are just multiple methods we use in delineating the species. Thus *disjunctive monism* is not really a form of pluralism at all, although it is similar to pluralism in the way that it requires a multitude of species definitions.

In contrast, a genuinely pluralist view is one on which the multiple accounts of species are genuine rivals that conflict, carving the tree of life in different ways; these accounts cannot be put together to provide a single, non-overlapping decomposition of the tree of life into distinct species. On this view no single classificatory scheme is ideal since there is no single way to capture all that we require of the species concept in a single all-encompassing definition. Even a disjunctive approach will not do since the factors we include in the rival definitions are in fact inconsistent with one another. Each organism is not necessarily a part of a single species concept since it may be part of rival species concepts that are not coextensive.

Such a pluralist approach is endorsed by Ereshefsky (1998, 1999). Ereshefsky argues that since many biological species are not phylogenetic species, and many phylogenetic species are not biological species, both approaches are needed to fully classify the tree of life.

Examples of these are commonplace, and this conflict is widespread and serious, as Ereshefsky points out:

> The disunity of the species category is due to major discrepancies in the biological world, not a few exceptions.[39]

Thus Ereshefsky holds a genuinely pluralist view. Yet he shies away from an even more radical form of pluralism, maintaining the lineage view of species that "all species taxa are genealogical entities".[40] Thus he still disallows groupings that cut across different lineages. What Ereshefsky does allow is that *paraphyletic groups* can form species, a view that cladists reject. Thus his view is somewhat broader than that of Sterelny and Griffiths who largely endorse the cladist principle that species must be monophyletic. (Although Sterelny and Griffiths do concede that it is likely that there may be good evolutionary hypotheses about paraphyletic groups, and therefore allow that such groups may attain a species-like status.[41] Thus their view may effectively be broader than a disjunctive monist position, and not all that different to Ereshefsky's, especially since both views agree that such paraphyletic groups do not represent real species, though for different reasons: Sterelny and Griffiths reject the idea that such groups are real species because of their cladism, whereas Ereshefsky rejects realism as part of his broader anti-realism about the species concept, as I discuss in the next section.)

A more radical form of pluralism is proposed by Kitcher (1984) and Dupré (1993). Initially their approach sounds similar to Ereshefsky's: Kitcher argues that the set of species taxa required for explanation over the full range of theories in biology is a heterogeneous set that cannot be defined by a single species concept. Kitcher claims that biology requires both what he terms *structural* and *historical* explanations, and thus requires both structural and historical conceptions of species. Dupré endorses Kitcher's approach, stating that "the complexity and variety of the biological world is such that only a pluralist approach is likely to prove adequate for its investigation".[42] Where their approach diverges from Ereshefsky's is in the range of species concepts they accept as valid, since they allow a much broader

[39] Ereshefsky 1998, p. 113.
[40] *Ibid.*, p. 107.
[41] Sterelny & Griffiths 1999, p. 198.
[42] Dupré 1993, p. 53.

class of groupings to be genuine species concepts. This is particularly clear in Dupré's version of species pluralism, as I outline below.

Dupré's approach is probably the most radical form of kind pluralism, for he argues that the classes to be considered genuine natural kinds can cut across a wide and heterogeneous array of conceptual categories. To illustrate this Dupré invites us to consider the lily. What is commonly called a *lily* actually cuts across a large array of taxonomic classes, occurring in many genera of the family *Liliaceae*, but not including the entire family. To classify the whole of the family *Liliaceae* as lilies would involve classing onions and garlic as lilies, something that would seem absurd to any decent florist, and could plausibly be a useless classification for an ecologist. Dupré concludes from this that kinds such as *lily* should be classified as genuine natural kinds, and that revising such kinds to fit in with a narrow cladistic view on taxonomy not only defies common sense but also denies the reality of kinds that have proved to be of great use in realms of investigation other than cladistic taxonomic classification. This example aptly describes Dupré's position of *promiscuous realism*, which he applies to the more specific problem of defining the genuine species.

For Dupré his "pluralistic" approach to species, which rejects the assumption of biology becoming a "unified project", forms the beginning of his broader claim of the disunity of science.[43] Thus for Dupré the fact that there is no unique and unified method of taxonomy in biology does not reveal biology to be a 'weaker' science in any sense, though perhaps it shows us that much of what we hope to achieve as far as unification in biology is impossible.

In outlining his pluralism, Dupré endorses the claim that evolutionary descent may not be the sole criterion for defining species. Thus he even rejects the lineage account of species, arguing that there may be genuine species, united by ecological or other synchronic properties, whose members do not share descent. Initially Dupré was seen to endorse a very radical version of his promiscuous realism, allowing a very wide class of kinds to qualify as genuine species. Interestingly, Dupré has since partially dissociated himself from this most radical view, stating that we should "return to a definition of the species as the basal unit in the taxonomic hierarchy, where the taxonomic hierarchy is considered as no more than the currently best (and minimally revised) general purpose reference system for the cataloguing of biological diversity."[44] This view seems to bring Dupré closer to endorsing the lineage

[43] *Ibid.*, p. 52.
[44] Dupré 1999, p. 18.

account of species, although this may be more of a clarification than a serious revision of his views, since it still allows a broad interpretation of how "general purpose" the taxonomic system should be, thus allowing a broad range of heterogeneous species concepts.

The discussions of Kitcher and Dupré are insightful: they show that there is a basic tension between the rival approaches to defining *species*, due to the fundamentally different aspects of the nature of species which the different definitions capture. The phylogenetic definition takes into account the *diachronic* nature of species, the fact that they evolve over time, whereas the biological and ecological definitions takes into account more of the *synchronic* aspects of species, the fact that there are unifying (relational) properties between members of a particular species at a particular time. We view species as both evolving entities and as coherent groups of organisms, and both of these properties of species – *process* and *structure* – feature in our use of the species concept in biology. Thus there is the possibility that either approach taken, on its own, will be inadequate since it will not encompass all of the theoretical requirements that the term *species* needs to fulfil in all areas of biology. This, together with the fact that the synchronic and diachronic factors may be fundamentally irreconcilable, entails that at this stage we should endorse a pluralist approach to species.

The idea is that any strictly phylogenetic definition of species may not divide organisms into generally distinct sets which reflect certain higher-order properties – such as where two closely genetically related species differ markedly in their ecological properties. Thus there may always be the need for both the phylogenetic definition and the ecological definition, the former being useful explanations in terms of the evolutionary history of the organism, the latter being essential for ecological theory. We cannot dispense with either the synchronic or diachronic aspects of species, yet we may not be able to unify them.

However, due to the ambiguity of the term *pluralism*, there is a further question here: what form of pluralism are we left with on this disunified view? Should we endorse a radical form of pluralism such as Dupré does, or is a weaker form, such as Ereshefsky's, powerful enough to reconcile the opposing pulls of structure and process? Perhaps a disjunctive monist view is powerful enough. I shall leave this an open question for the time being. Since I wish to defend a realist view of species, the important question here, which I address in the next section, is how the various pluralist conceptions relate to the ontological status of the species concept: how do they relate to the reality of species?

4.3 Defending Realist Pluralism

Does it make sense to endorse both a pluralist and a realist position on species? Stanford (1995) says no, arguing that species pluralism entails rejecting the idea that species are genuine, objective divisions in the natural world. Ereshefsky (1998, 1999) accepts the reality of individual species concepts, but rejects the idea that *species* represents a real category in nature. In contrast Kitcher (1984) and Dupré (1993), who endorse a strong form of species pluralism, according to which the multiple ways of "carving nature" are all equally necessary and all reflect genuine divisions in the natural world, support a realist interpretation of the species category. In this section I assess these arguments, establishing whether it is possible to endorse a realist, pluralist position on species (and natural kinds more generally).

Stanford's argument for species anti-realism follows on from his endorsement of Kitcher's form of species pluralism. Stanford endorses the idea that a genuine species concept is one that is "biologically interesting" while being "neither redundant, boring nor wrongheaded".[45] Following on from this definition Stanford says that "judgements of what is biologically interesting can only be made relative to a particular time and theoretical context."[46] Thus he claims that species cannot represent real, objective categories in nature, since species concepts have changed and continue to change in science, because what is biologically interesting changes through time. Since this change is caused by the practical interests of biologists, rather than physical changes in the world, Stanford claims that the species concept is not mind-independent, and thus cannot be real.

The problem with Stanford's argument for anti-realism is that it actually does not hinge upon the fact of species pluralism, but instead is based on a general scepticism about the reality of kind terms that alter and develop as scientific theories evolve. Thus it applies equally to other well-accepted kind terms in science, such as *electron* or *acid*. Therefore, if we accept Stanford's anti-realism in the case of *species*, we should also accept anti-realism about many other kind terms in science, a conclusion I am not willing to accept given my general endorsement of scientific realism. In particular, the realist perspective of Kitcher (1993), which Stanford originally appealed to in outlining his form of pluralism, provides good reason to reject Stanford's anti-realism. I shall return to this argument shortly.

[45] Stanford 1995, p. 80.
[46] *Ibid.*, pp. 80-81.

Ereshefsky's argument for anti-realism differs from Stanford's in that it stems directly from his endorsement of species pluralism:

> In sum, a realistic interpretation of species pluralism – one that accepts the existence of different types of species taxa – implies that there is no unified ontological category called 'species'. It implies that the species category does not exist. This I take to be the strong argument from species pluralism to the non-existence of the species category.[47]

Notice that Ereshefsky does accept the reality of the individual taxa that are defined by each of the species concepts; he accepts each of the species concepts he considers valid (those that accord with the general lineage view) as picking out a real, objective category in nature. It is the heterogeneous nature of all these definitions, which prevents a unified approach to defining species, that leads to Ereshefsky's anti-realism about the species category. Thus he claims we should drop the use of the general term "species" in biology:

> 'Species' has outlived its usefulness in biology, and when it is used it is ambiguous. Why not replace it with terms that explicitly distinguish the lineages we now call 'species'?[48]

This idea of being more specific about which species concept you apply in each context certainly sounds reasonable. Ereshefsky's view allows us to be realist about each particular approach to defining species, and thus allows a realist view of species for a bottom-up, localised methodology of science. The lack of a global approach to species does not present a problem for biology if all we require are real, relevant kinds in each specific context. Such a *localised* approach to natural kinds – one that doesn't require a global definition, but can be defined in each particular case – gives us a realist view of each type of species, and is enough to support a strong methodology in biology. Thus Ereshefsky's approach seems to be an excellent way of attaining the realism we require in biology, in that each of the particular species concepts is real and valid in its particular context.

Importantly, Ereshefsky's approach could also be applied to a more radical form of species pluralism. Although Ereshefsky restricts his pluralism to lineage species concepts, the more broadly conceived approaches to defining species of Kitcher and Dupré can be accommodated within Ereshefsky's approach, so long as there is an acceptance that each of

[47] Ereshefsky 1998, p. 113.

[48] *Ibid.*, p. 117.

the individual species concepts within the more radical pluralism is a real category of nature. This is something Dupré argues for in outlining his position of "promiscuous realism". In defending his more radical form of species pluralism Dupré argues that we can view a kind as a genuinely *natural* kind without being essentialist about such a kind. In his view we can consider a class to be a *natural* kind in a perfectly ordinary language sense, if we consider the class to be a genuinely important class of natural things. There may be nothing that defines such a class, in terms of essential properties, but for Dupré that does not count against its 'naturalness' or realness.[49]

However, what makes a class *genuinely important* is a rather vague notion that needs more explanation, especially if we wish to adopt a realist view under Dupré's approach to natural kinds. If the notion of 'importance' is very broad then it may seem that we should take on board the instrumentalist conclusions of Rosenberg: that the theories of biology can only be ones which reflect our interests, that relate to the categories that we see as important in the world, but which do not reveal any real structure in nature. If 'important' can mean important for evolutionary theorists, geneticists, palaeontologists, ecologists and developmental biologists then perhaps it is reasonable to conclude that such structures are genuine natural kinds. But if we also include gardeners, herbalists, witches, artists and poets as worthy of bestowing a class with genuine importance then the claim that such categories are genuinely natural seems rather dubious. Thus we need a way of spelling out in what sense a kind must be significant in order for it to be genuinely natural rather than merely arbitrary.

Stanford actually provided such a way with his idea that a genuine species concept is one that is "biologically interesting" while being "neither redundant, boring nor wrongheaded".[50] The provisos here provide a way of restricting species concepts to those that are genuinely relevant to biology, since they are linked to a conception of theoretical progress in biology. Stanford's conception of progress in science is derived from Kitcher's (1993) discussion, which links scientific progress to the idea of *erotetic* progress: making progress in answering the questions and solving the problems that drive our scientific inquiries of the world:

[49] Dupré does contrast such general *natural kinds* with the more narrowly conceived *strong natural kinds*, for which there are essential properties, and argues that essentialism is false for biological kinds and thus they are not strong natural kinds. Yet for Dupré this distinction between strong and weak natural kinds is not significant in terms of their reality or usefulness.

[50] Stanford 1995, p. 80.

> Erotetic progress is obtained by asking significant questions which extend, apply, or defend the presumptions of our increasingly successful schemata, and species concepts are accepted or rejected, I suggest, insofar as they render such significant questions more tractable.[51]

Kitcher illustrates this idea of truth being connected with the progress of our theories with the following argument, which questions the relevance of the creationist idea of species for the evolutionary hypothesis linking human and chimpanzee descent:

> We have scientific views about the relations between ourselves and the rest of nature. In the light of these scientific views we can evaluate the likelihood that we are right about various kinds of things. So we examine the procedures that are involved in accepting the hypothesis about human-chimp separation and show, by appealing to background ideas about reliable detection of aspects of nature, that such procedures would regularly deliver truth (understood in a realist way). Other procedures, such as those underlying the creationist claims that are rejected, would not.[52]

On the view advocated by Stanford, the acceptable species concepts are those that extend the problem-solving power of our scientific theories. But if we follow Kitcher's view as he intends, not only does such an approach provide a way of demarcating the useful, truly valid species concepts from those that are patently useless or irrelevant, it also provides us with a realist interpretation of the species concepts. This is because this approach to theoretical progress is part of Kitcher's overall realist attitude to science.[53] Thus it is odd that Stanford draws anti-realist conclusions here – instead he should have noted that his endorsement of species pluralism involves accepting a realist view of the species concepts. This idea, together with Ereshefsky's idea of endorsing the reality of the individual species concepts, allows a realist interpretation of Dupré's species pluralism.

A recent approach to natural kinds, involving the concept of *causal homeostasis,* also seems extremely promising as another way of explicating Dupré's notion of 'importance' while restricting the promiscuity of kind pluralism to kinds that are genuinely important for

---

[51] *Ibid.*, p. 79.

[52] Kitcher 1993, pp. 134-5.

[53] Although it is worth noting that Kitcher distances himself from what he terms 'naïve' or 'strong' realism, instead endorsing an alternative form of realism he labels as "Kantian in spirit" (Kitcher 1993, p. 171.) This approach links the structure of our scientific theories, the notions of *natural kinds* and *causal relations*, to *our* conceptual organization of nature, rather than solely to facts about nature itself. Thus in some sense Kitcher's Kantian realism denies the relevance of a mind-independent reality. Kitcher nevertheless asserts this is a realist view, and endorses a correspondence theory of truth.

scientific theory. The *causal homeostatic* theory of kinds captures the essentialist intuition that there is something special underlying a classing which is a genuine natural kind, whilst maintaining the intuition that kinds are united by possession of a common property cluster. This approach is outlined by Boyd (1989, 1991, 1999), and it is endorsed and applied by Paul Griffiths in his discussion of biological species (1999) and his theory on emotions (1997). For both of these writers, this approach provides a powerful way of defending a realist view on species: Boyd boldly states that biological species are homeostatic cluster kinds, while Griffith applies the theory to defend an essentialist view on species that is immune to the standard criticisms of species essentialism.

The basic idea behind the *causal homeostatic* account is that natural kinds are united by "homeostatic property clusters": sets of properties that tend to cluster together due to some sort of underlying factor. The concept of *causal homeostasis* is a way of describing how members of a category can be united as a kind, being bound by a property cluster, with some form of underlying causal factors or mechanisms that support the cohesion of those particular properties. Entities that possess one of the properties in the cluster tend to possess many of the other properties, due to underlying factors that cause the properties to be co-instantiated. To be a member of a particular kind an entity must possess a significant number of the properties in the property cluster, although the *causal homeostatic* account is deliberately vague on this matter, thus allowing natural kinds to be somewhat vague or fuzzy.

The causal homeostatic theory of natural kinds is a form of essentialism, though with some important differences to the standard essentialist view. The essence of a kind is tied to the concept of a homeostatic property cluster, since the essence could be said to be the underlying mechanism that unites the properties in the cluster. Where this view differs from standard essentialism is that it allows for the possibility that we could never have explicit knowledge of what causes the homeostatic clustering of properties that define a kind. Although we should surely endeavour to discover these mechanisms, it is quite possible that such knowledge is beyond out grasp. Also, unlike the standard essentialist view, the homeostatic definition of kinds is extensionally vague. Thirdly, and most importantly in terms of defining *species*, homeostatic property clusters may change over time while remaining the same kind. Boyd states this explicitly:

> the property cluster is individuated like a (type or token) historical object or process: certain changes over time (or in space) in the property cluster or in the underlying homeostatic mechanisms preserve the identity of the defining cluster. In consequence, the properties which determine the conditions for falling under [kind term] *t* may vary over time (or space), *while t continues to have the same definition.*[54]

Boyd's theory is perfectly suited to defining species as natural kinds, since it gives us a way of incorporating both the synchronic and diachronic aspects of species. It fits in excellently with Dupré's approach to species, since it does not involve what Dupré considers problematic about essentialism – there are no particular properties that provide the essence of the kind. It also satisfies the essentialist intuition that there is some matter of fact that unites the members of a kind. Finally, since these matters of fact are real properties and processes, it is a realist conception. Thus the *causal homeostatic* account of natural kinds provides a further way of endorsing the reality of a heterogeneous multitude of species concepts.

There is, however, a further question that needs to be answered here: should we accept Ereshefsky's argument that we reject a realist account of the species category? The heterogeneity of the different species concepts – the fact that the fundamental conflicts between them preclude a unified approach to defining species – is the only argument he has for denying the reality of *species*. So is the disunity of *species* reason to believe there is no such category?

John Dupré would say surely not. Dupré takes the realism part of his "promiscuous realism" very seriously, claiming his promiscuous pluralist view does not undermine any use of kind terms such as *species*. Not only does Dupré argue that we should accept the reality of each individual species concept, he also argues for the reality of the species category. For Dupré disunity itself is no barrier to realism.

Interestingly, Ereshefsky concludes his discussion on species anti-realism by admitting that the idea of eliminating the term *species* from biology is "overly idealistic, indeed far-fetched."[55]

[54] Boyd 1989, p. 17.
[55] Ereshefsky 1998, p. 118.

> The term 'species' is well entrenched in biology and has been used extensively for hundreds of years. ... It is hard to see how 'species' could be eliminated from both ordinary and biological discourse.[56]

This point alone provides enough to assert the reality of the species concept – since the category is genuinely important for biological discourse, and thus is "biologically interesting" in the sort of way that would appease Stanford (and Kitcher), it should be considered as real. Thus Ereshefsky should consider the species category as being real, and just accept that it is a disunified category.

However, does it really matter whether Ereshefsky accepts this argument or not? For in a bottom-up, localised approach to science, all that is required for a workable conception of natural kinds is that they can be defined successfully in each particular context. This is clearly possible within Ereshefsky's approach, since each particular approach to species gives us an important, genuine category of nature, a real type of species. Whether or not we believe in the broader *species* category is largely irrelevant for a localised methodology. For in each case we apply the term *species* we will actually be required to pick out a more refined, particular species concept to use. (Note, however, that this may severely complicate some of the original ethical concerns surrounding the importance of the term species, such as the preservation of biodiversity.)

Thus the concern of realism gives us no way of determining whether we should adopt a weak or strong form of pluralism. Ultimately, it seems the form of pluralism we should adopt will be determined by what our theoretical requirements are – what we need for powerful, informative, and strongly predictive theories. However, before I say any more on this, I will investigate another case of kind pluralism in science. This will help to support the idea that a pluralist approach to natural kinds is legitimate and well accepted in scientific theory.

4.5 An Example of Kind Pluralism: *Acids*

To see how a pluralist definition of kinds may be perfectly acceptable in biology consider the following use of terminology in a science usually considered as more rigorous and less problematic than biology. In chemistry there are concepts that are problematic in an analogous way to the concept of species in biology, in that they require a pluralist definition.

[56] *Ibid.*, p. 118.

An example is the concept of an *acid*, which Ian Hacking calls a *bifurcating kind* [57], since it was initially defined as a monistic natural kind but has now become a kind that requires at least two definitions.

The name "acid" comes from the Latin *acidus*, which means "sour", and refers to the sharp odour and sour taste of many acids.[58] In 1661 Robert Boyle summarised the properties of acids, stating that acids have a sour taste, are corrosive, change the colour of certain vegetable dyes, such as litmus, from blue to red, and lose their acidity when they are combined with what he called "alkalies". Alkalies are what we now call bases because they serve as the "base" for making certain salts.

In 1884 Svante Arrhenius came up with a definition that appealed to more than purely qualitative properties. He suggested that salts such as NaCl dissociate when they dissolve in water to give particles he called *ions*. Three years later Arrhenius extended this theory by suggesting that acids are neutral compounds that *ionise* when they dissolve in water to give H^+ ions and a corresponding negative ion. According to his theory, hydrogen chloride is an acid because it ionises when it dissolves in water to give hydrogen (H^+) and chloride (Cl^-) ions. However the Arrhenius theory doesn't encompass the full range of chemicals that Boyle would have classed as an acid, since it can be applied only to reactions that occur in water because it defines acids and bases in terms of what happens when compounds dissolve in water. It also doesn't explain why some compounds in which hydrogen has an oxidation number of +1 (such as HCl) dissolve in water to give acidic solutions, whereas others (such as CH_4) do not.

The Arrhenius theory was improved in 1923, when J. N. Brönsted in Denmark and T. M. Lowry in England independently, and almost simultaneously, proposed the modern 'protonic' or Brönsted-Lowry theory of acid-base behaviour. According to the Brönsted-Lowry (B-L) concept, an acid is any compound or ion that can give up a proton, while a base is any compound or ion that can accept a proton. In 1923 the rival Lewis theory of acid-base behaviour was also devised, by the American physical chemist G. N. Lewis. A Lewis acid is a substance that can take up an electron pair to form a covalent bond, a Lewis base being a

[57] Hacking 1983, pp. 85-6.

[58] For this historical summary I largely follow Bell 1969, pp. 1-12; Moeller 1982, p. 585; Huheey *et al* 1993, pp. 318-330.

substance that can donate an electron pair. The Lewis theory is more general and encompasses the Brönsted-Lowry acids, since any substance that can give up a proton can also take up an electron pair to form a covalent bond (both theories agree on the bases). However not every Lewis acid is a B-L acid. For example, aluminium chloride, $AlCl_3$ is a Lewis acid but not a B-L acid.

The modern view on acids is a pluralist view, accepting both the B-L and Lewis definitions[59], according to the context in which the term "acid" is used. In most circumstances the B-L theory is sufficient to explain the role of the acid in a chemical reaction, however for some reactions the Lewis definition is required. For example, the Lewis acid-base theory explains why BF_3 reacts with ammonia, an explanation that would be unavailable under the B-L view since BF_3 is not a B-L acid. Lewis acids are especially significant in organic chemistry. The organic process of Friedel-Crafts alkylation[60], the most important method for adding alkyl side chains to aromatic rings, is explained by the action of the Lewis acid $AlCl_3$ and the (Lewis) acid-base equilibrium. Without the Lewis definition it would be impossible to classify such a reaction as involving an acid-base equilibrium, yet this is the best way of describing the reaction because of its explanatory power.

Surprisingly, although the Lewis theory encompasses all the B-L acids and bases, it has not eliminated the need for the B-L theory. In many contexts it is not only sufficient to use the B-L definition of an acid, it is far more convenient and powerful for explanatory purposes. These include especially those reactions that involve aqueous solutions, since the B-L acid concept applies particularly well in these contexts. The importance of the B-L view is also apparent when considering how we assess the relative strengths of acids: the common measure of acidity, the pH index, is a measure of the concentration of H+ ions in an aqueous solution, and so essentially involves the B-L definition of acids rather than the Lewis definition. Thus, for example, the B-L theory provides a powerful way of explaining the relative strength of the Lewis acid $AlCl_3$ which has a pH of 3.0. We can't attribute the acidity of aqueous solutions of $AlCl_3$ to the Cl^- ions because these ions are weak bases. The acidity of these solutions must result from the behaviour of the Al^{3+}. However the Al^{3+}ions can't be B-L acids by themselves; they can only act as proton donors by influencing the ability of the

[59] There is also an even more general definition of acids, first proposed in 1939: the Usanovich Definition (Moeller 1982, pp. 601-2; Huheey *et al* 1993, pp. 325-6). However this approach has not been very widely used, and has been criticised for being far too broad, including far too many substances as acids.

[60] Morrison & Boyd 1983, p. 632.

neighbouring water molecules to give up H^+ ions. They do this by first forming covalent bonds to six water molecules to form a complex ion. Water molecules covalently bound to one of these metal ions are more acidic than normal. Thus, reactions such as the following occur:

$$Al(H_2O)_6^{3+}(aq) + H_2O(l) \rightarrow Al(H_2O)_5(OH)^{2+}(aq) + H_3O^+(aq)$$

These reactions give rise to a net increase in the H_3O^+ ion concentration in these solutions, thereby making the solutions acidic. Thus the B-L approach to acids provides us with important explanatory tools for assessing the relative strengths of acids.

More recently other approaches have been developed for defining acid strength more generally for Lewis acids without utilising the B-L definition. These approaches involve looking at the thermodynamics of solutionless, gas-phase acid-base reactions, rather than the reactions in aqueous solutions that the pH definition of acid strength relies upon.[61] Without going into the technical details of these techniques, it is worth noting that these approaches are still problematic if we wish for a single, universal scale of acid-base strength, as pointed out by Moeller (1982):

> a more serious disadvantage [of the Lewis definition] is the absence of a uniform scale of acid or base strengths. Instead, these are made variable by dependence upon the reaction or method used for their evaluation.[62]

The reason for this variation of scale is that the measure of acid or base strength is dependent upon the particular method used to determine strength, with the ranking of relative strength varying according to the method. What this means is that there is no single scale on which we can rank the relative strengths of acids and bases. However, in any particular context there is a clear and rigorous way of determining relative acid-base strengths, without ambiguity for that particular context. Thus what is seen as a disadvantage in the above quote may not be all that serious a problem, since we can deal successfully with each relevant case. This may just be another example of where we have to accept the disunity of a natural concept, accepting that there is no unified, global approach to defining acid strength. Clearly this method also accords well with a bottom-up approach to scientific methodology, since it

[61] See Huheey *et al* 1993, pp. 330-344.

[62] Moeller 1982, p. 600.

can be applied successfully on a case-by-case basis, when problems are dealt with in their context and assessed according to their particularities.[63]

Returning to acid-base definitions, because of the importance of aqueous solution chemistry, and the power of the B-L approach to acids in that context (especially in terms of assessing relative strength) the B-L definition is an essential part of a powerful approach to chemistry. The Lewis approach is also essential, since it adds explanatory power in other contexts, especially in organic chemistry. There is thus an important sense in which both definitions are necessary for an adequate explanation of many chemical reactions. We don't conclude from this that the term *acid* only reflects the interests of the scientist because it does not cut nature at its joints. We just conclude that there are two approaches we can take to acids, and that the approach we take is dependent upon the context we want to explain in. This is precisely what Moeller (1982) says:

> each set of definitions is correct within its own framework, and contrary to unfortunate presentations in the literature, there is no conflict among these definitions. Proper breadth dictates that one should be conversant with each point of view and should apply whatever approach seems best suited to the problem he is considering. No single set of definitions is necessarily the most useful for all circumstances. Each has its own realm of specialized applicability. Knowledge and utilization of the fundamentals of all the definitions are essential.[64]

This attitude is similarly echoed in a textbook on organic chemistry, revealing that the acceptance of such kind pluralism in chemistry is widespread:

> The terms *acid* and *base* have been defined in a number of ways, each definition corresponding to a particular way of looking at the properties of acidity and basicity. We shall find it useful to look at acids and bases from these two viewpoints; the one we select will depend upon the problem at hand.[65]

[63] There is a further approach to acids and bases, which involves classifying them in terms of *hardness* and *softness* (see Huheey *et al* 1993, pp. 344-355), rather than as *strong* and *weak*. This approach provides alternative conceptual apparatus for assessing acid-base interactions which, in some cases, is more powerful than an approach utilising acid-base strength. This further emphasises the pluralism of the acid-base concept.

[64] Moeller 1982, p. 603.

[65] Morrison & Boyd 1983, p. 36.

However, despite this uniform acceptance of pluralism for defining acids, there actually does exist a well-accepted generalised approach to defining acids and bases. This involves recognising a fundamental similarity with all of the proposed definitions:

> All define the *acid* in terms of *donating a positive species* (a hydrogen ion or solvent cation) or *accepting a negative species* (an oxide ion, a pair of electrons, etc.) A *base* is defined as *donating a negative species* (a pair of electrons, an oxide ion, a solvent anion) or *accepting a positive species* (hydrogen ion). We can generalize all these definitions by defining *acidity as a positive character of a chemical species which is decreased by reaction with a base*; similarly *basicity is a negative character of a chemical species which is decreased by reaction with an acid.* [66]

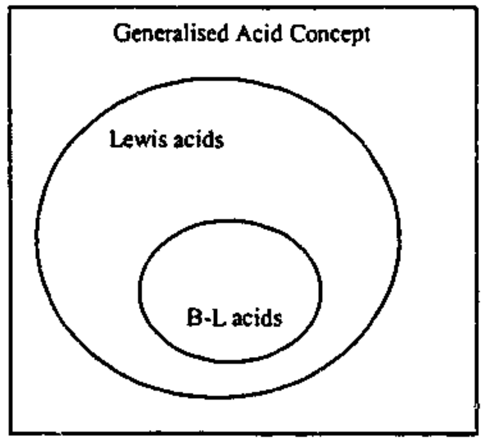
It may seem strange that despite the existence of this generalised approach to acids and bases there is still an acceptance of pluralism for the acid-base concept. Indeed both Moeller (1982) and Huheey *et al* (1993) describe this generalised approach immediately after explicitly endorsing pluralism. However, these authors also recognize that the existence of a generalised approach to acids does not challenge the fundamental pluralism of the concept. The reason for this is that the generalised approach is far too general to apply in a useful, informative way in each particular context. In each context there is the need for a more specific definition of acids, and therefore the two definitions both have an important role in chemistry. Thus one must accept that no single approach to defining acids will suffice, and that a pluralist conception is one we have to live with.

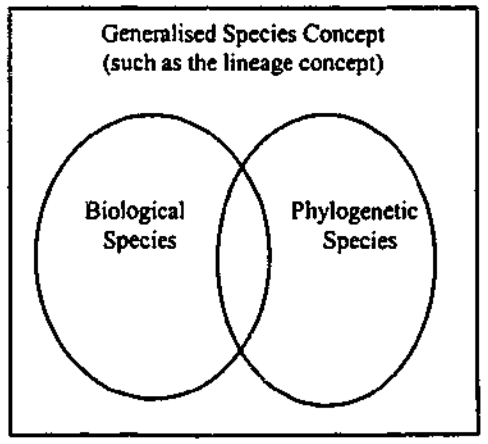
Note how similar this example is to the case of defining species: especially to the approach of Ereshefsky, and Sterelny and Griffiths, who all endorse a general lineage concept of species. With both these examples there is a general approach to defining the natural kind, which provides a broad outline of the kind – enough to determine the "homeostatic property clusters" that unite the kind. However, in each particular context a more specific approach to defining the kind is needed; and, importantly, the set of concepts that are required to fully accommodate the kind cannot be united into a single ubiquitous concept.

There is, however, a difference between the pluralisms advocated for species and the pluralism just described for acids. In the case of acids the multiple definitions give increasingly specific, 'nested' accounts of a general acid concept – they carve the notion

[66] Huheey *et al* 1993, p. 326.

more finely. Whereas, in the case of species, the definitions cut the world in distinctly different ways. This difference is illustrated in the following diagram:





The question here is whether this amounts to a significant difference between the two examples – if this is so, then one may conclude that acid pluralism is acceptable whereas species pluralism is not. In response to this question, it seems that this difference is not all that important when we are adopting a bottom-up approach to science – the most important fact is that we can apply a particular definition in each particular context, and that each particular definition has an important explanatory role in our theory. Given this fact, the relationship between the individual definitions is not relevant for a realist view of kind pluralism. By adopting the complex, disunified, model-building approach of the English mind, we have all we need for a powerful approach to science.

Given the acceptance of such kind pluralism in chemistry, it is thus reasonable to claim that theoretical kind terms in biology such as *species* – and presumably other kind terms such as *predator* and *population* – can be treated likewise in a pluralist way. Doing so does not imply that these terms are ill defined or theoretically weak or useless, but rather that the use of such kind terms is more complex and more particular than traditional views of scientific theories have previously allowed. Treating natural kind terms in this more flexible way will not only allow disunified, localised methodologies to be valid approaches [to s]cience, it will also help to bring philosophical accounts of scientific theories closer in line with the actual practice of scientists.

5. *Conclusion: Localised Natural Kinds*

In this chapter I have defended the kinds of kinds that appear in ecology, and biology more generally, by arguing for the validity of what I term a *localised* approach to natural kinds. This approach embraces the pluralism that appears inevitable in defining many natural kinds, especially in biology, but also, as I have just shown, in traditionally 'stronger' sciences like chemistry. On this pluralist view, questions of classification should be addressed locally, in terms of how that classification is to be used.

Returning to the problem of defining *species*, this outlook allows for a flexible and powerful approach to defining this important kind. As I claimed earlier, the form of pluralism we should adopt for a particular kind will be determined by what our theoretical requirements are – what we need for powerful, informative, and strongly predictive theories. In the case of *species*, this means that we should accept the validity of many of the rival definitions. For example, some problems in developmental biology may require the use of the phylogenetic definition, whereas ecological problems may use the ecological definition of species. I shall even leave open the possibility that there may be a need for the morphological approach to defining species in some areas of biology – with palaeontology it may be that in some cases all we have to go on is morphology as a way of determining phylogeny.

This pluralist approach to species could be seen as quite radical, since it allows for the possibility that valid species concepts may clash with the conception of species as monophyletic evolutionary units. Many hold the lineage view, that an essential aspect of any reasonable approach to defining species must be within the constraints of evolutionary theory, and so argue that any definition of species must at least be some refinement of the *phylogenetic conception*. Paul Griffiths (1997) states this most strongly in his preferred approach to species, where he defends a *causal homeostatic* account of species, whereby species are essentially defined by their historical properties. However, because of the significant role that non-historical, synchronic conceptions of species can play in ecological contexts, we should accept these conceptions as representing real and important divisions of nature. Perhaps these shouldn't be considered as 'genuine' species, but merely as something like 'significant ecological units'. Perhaps we should ditch the entire *species* concept, and just keep the individual concepts, as Ereshefsky suggests.

Ultimately, I believe, these questions are not all that important, since they are largely disputes over semantics rather than deep differences of ontology. All that is important, from the perspective of a bottom-up approach to science, is that a relevant species concept can be defined in each particular case. This approach gives us a realist view of each type of species, and is enough to support a strong methodology in biology.

Despite this strong endorsement of pluralism, ultimately I remain agnostic about what a final workable conception of species may look like. This is because it is at least plausible that a monistic account could eventually be formulated that will solve all the difficult conceptual problems discussed – although this would presumably involve some sort of paradigm shift as to how we view the biological world. What my agnosticism does mean is that I accept as very real the *possibility* that the best conception we could have of species is a pluralist one. Thus I defend the idea that a pluralist conception of species can leave us with just as workable a concept for a realist approach to biology as a monist account would.

Chapter 5

Piecing the World Together

1. *Cartwright's Epistemology and Localised Models*

Over the last few chapters I have presented the ingredients of an argument for a localised view of science. Now I put the pieces together, defending the overall picture of science we must accept in order to think locally. Here I look in some detail at Nancy Cartwright's work on scientific modelling, laws of nature, and causation. Cartwright's conception of epistemology is well-suited to the localised, realist picture of scientific inquiry that I have been painting. Her views on the role of idealization and abstraction, and the importance of singular causation rather than general laws, can be applied to defend the importance of the bottom-up modelling methods I have discussed.

Cartwright's views have informed and influenced much of what I have defended in this thesis. In the first chapter I introduced the distinction between a top-down, global approach to science and a bottom-up, local approach. I pointed out that it is dogmatic to believe that scientific theory must be simple and unified, and that a disunified, complex view of science based on localised modelling can provide us with all we want from science. Here I briefly discussed how these ideas relate to Cartwright's criticism of what she calls "fundamentalism", along with her comment that she believes God has a messy, disordered, English mind rather than an ordered French mind. In the second chapter I elaborated on this idea, looking in detail at its origins in Pierre Duhem's philosophy of science. In doing so, I claimed that this would help to clarify what Cartwright has been saying in her recent work. This is because Cartwright's work is best seen as a response to much of what she finds abhorrent in Duhem's philosophy, especially his overt fundamentalism, and his belief that the approach of the French mind is more powerful than that of the English mind.

In the third chapter I argued that a localised, bottom-up methodology can, in some cases, provide us with a more powerful, more accurate method of problem-solving than a global, top-down methodology. In sciences such as conservation ecology, trying to base theory on abstract universally applicable laws actually takes us away from a realistic, accurate, and useful approach, which is best achieved via locally applicable, complex models.

In the previous chapter I gave more substance to the idea of a localised approach to science, arguing for the acceptability of a localised conception of natural kinds. Such localised kinds form the basis of the bottom-up modelling methods I praised in the earlier chapters. Much of what I argued here fits in extremely well with Cartwright's view that the business of science is not about discovering general laws, but discovering causal powers or propensities in local, isolated circumstances.

In this chapter I not only show how Cartwright's views bring together many of the ideas I have argued for, but also how they can paint a full and coherent picture of scientific inquiry, that can fit methodologies across the spectrum of the sciences. For, rather ironically, Cartwright's views on metaphysical and epistemological disunity lead to a rather unified view on scientific methodology, whereby the business of science is the uncovering of causal capacities in nature. I also take a closer look at the role of modelling in ecological theorising and investigate the relationship between the constructed models and reality. Here I appeal to Cartwright's insights into the role of modelling in scientific theorising and the roles that causation and laws of nature play in the development of scientific theories. In doing so I show how a localised, disunified methodology of science not only paints a more accurate picture of the enterprise of science as it now stands, but also represents a genuinely powerful and fruitful approach for many realms of science.

2. *Cartwright's Project*

Cartwright's project stems from her interest in looking at the ways scientists actually work, how they formulate their theories, develop their ideas and come to their conclusions. Rather than begin with a purely theoretical question, such as what constitutes good explanation in science, or (as Popper asked) how we can distinguish good science from pseudo or non-science, Cartwright is more concerned with the actual everyday work of scientists. Instead of looking abstractly at scientific theory, Cartwright looks closely at how that theory is generated: how models are constructed, how experiments are set up, how the relevant mathematics is derived and justified, how quantities are measured, and how knowledge claims are arrived at.

By looking at how scientists actually work Cartwright turns a number of strongly held views in the philosophy of science on their heads. Most importantly, she argues against the importance of laws of nature in scientific theory. In *How the Laws of Physics Lie* (1983)[1] she looks closely at the work of physicists, presenting an argument for anti-realism about the fundamental laws of physics. Here Cartwright's central and controversial claim is that so-called 'laws of nature' do not express true facts about the world. Her argument appeals to the fact that in the real, complex, messy world the laws of physics are strictly not true, nor even approximately true, even in the most isolated of circumstances. It is only in very specially controlled conditions that things in nature behave in such a way that they appear to conform to the fundamental laws of physics. Yet even in the most controlled conditions it is impossible to fully isolate the system from external factors that disrupt the laws, thus the laws are never strictly true in real-world circumstances. Therefore the fundamental laws never describe what actually happens in the world. What does happen can be explained only by broader, vaguer, higher order generalisations – what she terms "phenomenological laws". The fundamental laws, however, are true in *idealised abstracted models*, which are theoretical representations of isolated experimental conditions. The fundamental laws "lie", in the sense that they are only true in these most idealised, abstract circumstances; in the real world they are never true, because there are a myriad of other factors present, which can and do intervene in any situation.

Thus, for example, Coulomb's Law, which describes the electrostatic force between two charged particles ($F = kqq'/r^2$) never describes a real force in the world. What it does describe is the force in a purely idealised situation, where the particles are zero dimensional and of zero mass, and there are no other forces involved. Since this situation is clearly impossible in reality, it can never be said that Coulomb's Law states a true fact about the world. The same can also be said for Newton's Law of Gravitation ($F = Gmm'/r^2$). Cartwright claims equations such as these do not tell us about true necessary laws since they are never actually true. (Instead, they tell us something about the *causal powers* at work in the world.)

One response to Cartwright is to argue that the laws and idealisations inform us about the various *component* forces that combine to produce the overall force. Cartwright's response to this idea, in HLPL, is to explicitly deny that there are such things as the electrostatic or

[1] Hereafter referred to as HLPL.

gravitational component of the total force on a body. She claims that vector addition of component forces is merely a metaphor:

> For the force of size Gmm'/r^2 and the force of size qq'/r^2 are not real, occurrent forces. In interaction a single force occurs – the force we call the 'resultant' – and this force is neither the force due to gravity nor the electric force. On the vector addition story, the gravitational and electric forces are both produced, yet neither exists.[2]

To defend this view Cartwright appeals to the following argument: "When a body has moved along a path due north-east, it has travelled neither due north nor due east."[3] The intuition is that although the motion can be separated into a due north and due east component, neither of these components are representative of real motion. Similarly, Cartwright denies the reality of components of a resultant force. However, in reply, components forces are clearly not the same as components of motion. Imagine pulling a weight across a frictionless surface by two strings simultaneously, one pulling due north, the other due east, with the same amount of force on each string. The object will move due north-east, the *resultant* force on the weight is due north-east, but the *actual* forces on the object are the two component forces: one due north, the other due east. To deny the existence of these component forces seems clearly counterintuitive. Cartwright responds to this merely by stating that our intuitions here are strongly divided (since she has different intuitions), and she blankly asserts: "It is implausible to take the force due to gravity and the force due to electricity literally as parts of the actually occurring force."[4]

More recently Cartwright has attempted to clarify her position in *The Dappled World* (1999). Firstly she emphasises that she is looking specifically at the usefulness of Coulomb's Law and Newton's Law for determining the overall motion of a particle when it is subject to the multitude of causal factors present in the real world:

> let us consider what Coulomb's Law tells us about the motions of the particle pair. It tells us absolutely nothing. Before any motion at all is fixed, the particles must be placed in a special kind of environment; ... Without a specific environment, no motion at all is determined.[5]

[2] Cartwright 1983, p. 60.
[3] *Ibid.*, p. 60.
[4] *Ibid.*, p. 61.
[5] Cartwright 1999, p. 59.

Here Cartwright seems to be shying away from the earlier claim that the total force on a particle is not determined by adding component vectors. Instead she points out that these so-called laws actually tell us nothing about the resultant effect in the real, complex world. However, what she is saying here seems to be fully consistent with realism about laws. Realists would agree that we need to know both the initial conditions and the effects of Coulomb's Law (and any other incident forces) in order to calculate motion. Denying that Coulomb's Law alone can tell us all we need to know about motion doesn't challenge the idea that such laws are real. If we know the initial conditions of a particle, we could calculate the component vectors of all the forces acting on the particle by applying Coulomb's Law, Newton's Law and whatever other laws we need (if we know the laws), and thereby calculate the resultant motion of the particle. On this view, the laws inform us of the real effects of the phenomena they are describing. Thus the fact that laws alone do not inform us of overall effect does not challenge the truth of the laws.

Yet Cartwright does deny that these laws inform us of real phenomena, and she provides further reasons for denying the reality of these component forces. Her argument is that the concepts we apply in order to determine the resultant force in examples such as these are not the fundamental laws. Instead, we apply knowledge of the *causal natures* of the particles involved. This does not mean that statements such as Coulomb's Law are irrelevant, since Cartwright sees such equations as ways of describing the casual natures of the particles:

> Coulomb's law tells not what force charged particles experience but rather what it is in their nature, *qua* charged, to experience.[6]

Similarly, what we discover in a controlled experiment that isolates away interfering factors are not fundamental laws, but knowledge of the causal natures of the objects in the experiment. This, for Cartwright, is the only way for an empiricist to make sense of what is going on in these experiments – rather than appealing to physically impossible, idealist notions, such as objects having zero mass, she sees these experiments as revealing the causal powers of the objects due to their having mass, charge etc.:

> By controlling for or calculating away the gravitational effects, we try to find out how two charged bodies 'would interact if their masses were zero'. But this is just a stage; in itself this information is uninteresting. The ultimate aim is to find out how the charged bodies interact

[6] *Ibid.*, p. 82.

> not when their masses are zero, nor under any other *specific* set of circumstances, but rather how they interact *qua* charged.[7]

The importance of causal capacities is emphasised in Cartwright's second major work, *Nature's Capacities and Their Measurements* (1989a).[8] Here Cartwright follows on from what she says in HLPL, maintaining that the laws of physics lie, while developing the idea that there are underlying capacities or propensities in nature that are isolated and discovered in controlled experiments. Regularities certainly occur, although they will never be exceptionless because of (at least) the external effects on any system. But what underlies these regularities are the *causal capacities* at work in the world rather than the brute necessity of law.

What Cartwright means by *causal capacity* is a general notion about the ability of things in the world to act, interact, transform, or generally manifest in some way. It has to do with the objects of nature themselves being active, rather than having action imposed on them by the natural laws of the universe. The concept is related to but broader than that of a causal power, propensity or disposition, something that Cartwright emphasises (and which will be relevant in the next section):

> What is important about capacities is their open-endedness: what we know about them suggests strategies rather than underwriting conclusions, as a vending-machine view of science would require. To see the open-endedness it is useful to understand how capacities differ from dispositions.
>
> Disposition terms, as they are usually understood, are tied to one-to-one law-like regularities. But capacities, as I use the term, are not restricted to any single kind of manifestation. Objects with a given capacity can behave very differently in different circumstances.[9]

Thus Cartwright presents a strong and profoundly anti-Humean metaphysical thesis: that capacities are real and that science is the job of discovering those capacities. This view is in contrast to both the standard Humean view that singular causation is explained as an instance of a regularity, and the standard statistical relevance view on causation as championed by

[7] *Ibid.*, pp. 83-4.
[8] Hereafter referred to as NCM.
[9] Cartwright 1999, p. 59.

Salmon and others.[10] In analysing causation David Hume argued against causal powers, claiming that causation is nothing more than a "constant conjunction" of events: "A causes B" means that A is invariably followed by B. Thus all "smoking causes cancer" means is that the event 'X smoking' is invariably followed by the event 'X having cancer'. The statistical relevance view is just a refinement of this view, quantifying the notion of constant conjunction more precisely for probabilistic cases, whilst still keeping causation within the realm of direct experience. On both these empiricist views causation is nothing more than the right sort of co-occurrence of events. The concept of a causal power, the idea that objects act with some sort of causal efficacy which underlies why A is invariably followed by B, has no place in such an empiricist view.

Cartwright's insistence that causal powers are real goes against this, seemingly adding something beyond our experience to the concept of causation – the intangible notion of a *causal capacity*, something that has standardly been banished from modern empiricist views of science. Yet Cartwright identifies herself as an empiricist, as someone who stays in the realm of direct experience and shies away from speculative metaphysics, so why is she messing with these deep and mysterious fundamental notions?

The answer to this question lies in Cartwright's approach of looking closely at the actual methodology scientists employ. This is something she has stated explicitly:

> I think the reason I want to pursue these metaphysical views – that capacities are basic, not laws – is not as a piece of metaphysics, that I want somehow to get the ontology of the world right, but rather that I think that certain ways of picturing the world lead to certain scientific methodologies, and that some are better than others.[11]

Furthermore, as discussed previously in this section, Cartwright directly argues that accepting the existence of causal capacities provides a more tenable view for an empiricist, since in doing so we no longer have to appeal to abstract, non-existent entities such as massless particles, frictionless planes, and the like, in our formulation of scientific theory.

It is therefore important to remember that Cartwright's philosophy of science is not abstract, but is rooted in the everyday practice of scientists. The metaphysics is just what emerges

[10] Kitcher & Salmon 1989.
[11] Pyle (ed.) 1999, p. 208.

from her account of how science works and how scientific knowledge claims are constructed and justified. Similarly, it is important to note that when Cartwright talks about causation she is referring to more than just microphysical causation, and more than the relatively simple Newtonian causation such as that evident in a game of billiards. For she also talks about causation in a general everyday sense, in the sense that smoking *causes* cancer, aspirin *relieves* headaches, poison *causes* things to get sick and die. Rather than microphysical causation, it is causation in this everyday sense of the word that many, if not most of the sciences are interested in.

Thus Cartwright is not just interested in physics and the fundamental laws of physics; she is interested in a more general account of scientific practice. In NCM she considers econometrics, and argues that inquiry in econometrics also concerns the uncovering of causal capacities. Through an investigation of linear modelling in econometrics Cartwright defends the view that probabilities can reveal capacities, and that *singular* causation is of fundamental importance. This is the same claim she makes of fundamental physics – that experiments reveal instances of singular causation, and from these instances we uncover knowledge of the causal capacities involved.

What can be seen from this is that although Cartwright preaches, alongside John Dupré, for the disunity of science, what she is trying to achieve is actually a unified view of science. That is, she is trying to formulate an all-encompassing view of science that can apply across the range of sciences, from the 'hardest' realms of fundamental physics, to the 'softer' special sciences such as the social, economic, and biological sciences.

However, one may accept the bulk of what Cartwright says without accepting this unified view. That is, one may accept that causal capacities are significant in scientific theory, and that theory can do all we require of it without there being true fundamental laws, whilst still accepting that in some sciences fundamental laws could be true and of central importance. We can believe that the laws of physics are globally applicable and true, yet still hold that in biology, chemistry or econometrics no such laws exist, and that this fact presents no barrier to strong, realistic theory based on an ontology of causal capacities.

Thus there is a strong and a weak claim that can be made here. The stronger claim, which Cartwright favours, is that all scientific investigation is primarily concerned with the discovery of causal structures. What appears to be the search for universal laws is in fact just

the search for (local) causal capacities. The weaker claim is that there are two modes of scientific investigation, one concerned with discovering global regularities and fundamental laws of nature, and the other concerned with local-scale causal stories. What both these claims have in common is a rejection of the 'beautiful theory view' – a rejection of the idea that scientific theory must essentially be unified, simple, elegant, and globally applicable. Thus, in terms of this thesis, I do not need to assert the strong claim. It is enough to insist that the weaker claim is true, and defend it by pointing out how a bottom-up, locally focussed approach to science can be a genuinely powerful approach for attaining scientific knowledge. In doing so I also leave open the possibility that the stronger claim is true, for I show that knowledge of global regularities and fundamental laws of nature is not a necessary element of science. Thus my position is fully consistent with Cartwright's stronger claim, but does not entail it.

There is one sense in which the stronger claim is clearly false. Given the clearly empirical nature of some areas of mathematics, such as knot theory (Kauffman 1994), it is reasonable to consider mathematics itself as a mode of scientific investigation. This then falsifies the stronger claim, since in mathematics we are not concerned with causal structures and localised capacities. However, as I stated in the first chapter, recent work in these areas of mathematics clearly defies the 'beautiful theory view', since it involves complex, messy methods that are far from elegant. The stronger claim may also be falsified by some areas of physics, where the discovery of generalised laws of nature is the primary aim – such as in the quest for a 'Grand Unified Theory'. Yet, as also discussed in the first chapter, even if the ideals of unity and beauty have a role in some areas of science, they may be of little value in the more complex sciences.

Even if some areas of science do involve global laws of nature, in the bulk of scientific investigations we are generally not in the business of discovering global laws. Instead we are interested in investigating the underlying causal basis: how things *react*, what the causes of particular phenomena are, what makes things work the way they do. This involves an approach from the bottom-up, rather than the top-down. In these sciences, which include most of the biological, medical, chemical and environmental sciences, as well as many physical and behavioural sciences, solving particular, local problems is the fundamental aim. In solving these local problems, in local contexts, global laws are of little relevance. Locally valid generalisations are certainly relevant when they exist, and these may even be instances of more general laws. But they are not necessary for a bottom-up approach, and may even hinder such an approach, as the discussion in Chapter Three demonstrated. Thus the weaker claim, that there are two modes of scientific investigation, still involves a radical revision of many dominant ideas in the philosophy of science, since this claim entails that laws are not necessarily the foundation of scientific theory. Instead of theories (or models) always being built from global laws, we must allow that theories can be locally specific, based on locally defined natural kinds and involving local causal and stochastic properties.

This is where Cartwright's patchwork metaphor, and her idea of the "dappled world", can prove extremely useful. In the introduction to her most recent collection, *The Dappled World*, Cartwright elaborates eloquently on this broad view:

> This book supposes that, as appearances suggest, we live in a dappled world, a world rich in different things, with different natures, behaving in different ways. The laws that describe this world are a patchwork, not a pyramid. They do not take after the simple, elegant and abstract structure of a system of axioms and theorems. Rather they look like – and steadfastly stick to looking like – science as we know it; apportioned into disciplines, apparently arbitrarily grown up; governing different sets of properties at different levels of abstraction; pockets of great precision; large parcels of qualitative maxims resisting precise formulation; erratic overlaps; here and there, once in a while, corners that line up, but mostly ragged edges; and always the cover of law just loosely attached to the jumbled world of material things. For all we know, most of what occurs in nature occurs by hap, subject to no law at all. What happens is more like an outcome of negotiation between domains than the logical consequence of a system of order. The dappled world is what, for the most part, comes naturally: regimented behaviour results from good engineering.[12]

The metaphor of the dappled world describes a clear alternative to the 'fundamentalist' view that the world is best understood as being ordered, unifiable, and governed by global laws. It provides a powerful means of distinguishing a complex, disunified view of science from the ordered, top-down view favoured by those such as Duhem and Hempel. The direct response to Duhem's 'French mind' approach to theory is especially clear in the above quotation – the dappled world can only be understood correctly from the perspective of an English mind. In contrast to the dominant "received view" on scientific theory and explanation, a scientific approach that takes the dappled world into account cannot be based on the idea that the world is governed by pattern, order and unity. The whole of science would involve many such approaches, each covering a domain that is locally specific to that discipline. As a

[12] Cartwright 1999, p. 1.

whole, science may cover the entire physical world, but there may be no way of unifying the disciplines or linking them coherently – such is the disunity of the dappled world.

3. *The Chalmers-Clarke Debate*

Alan Chalmers and Steve Clarke crossed swords in a lively and illuminating exchange in the *Australasian Journal of Philosophy*, concerning the relationship between capacities and fundamental laws in Cartwright's work.[13] This debate is significant for this discussion because it highlights the importance of abstract models and idealisations for Cartwright's overall argument, that science does not concern itself with the discovery of laws of nature. Significantly, both Clarke and Chalmers endorse Cartwright's ontology of causal capacities. However, while Clarke maintains Cartwright's line that, strictly speaking, the laws of nature lie, Chalmers claims that Cartwright's empiricism actually entails that the laws of nature are true. The moot point of their debate is whether the relationship between capacities and laws is that of truthmakers to truthbearers. If this is so, as Chalmers claims, then accepting an ontology of capacities (or powers, propensities, dispositions), as Cartwright does, commits one to accepting that fundamental laws are in fact true – the capacities are what make the fundamental laws true. However if one denies the claim, as Clarke does, one must show how such an understanding of real causal capacities can be reconciled with antirealism about fundamental laws.

Chalmers, in his original discussion (Chalmers 1993), claims that with the publication of NCM Cartwright, in arguing for an ontology of capacities, must reject her antirealist position on fundamental laws as argued in HLPL. His point is that once we make an appeal to causes and theoretical entities with causal powers in our explanations, we have all we need to formulate a realistic interpretation of fundamental laws – the laws describe the capacities.

> whilst it is correct to say that our theories are applied to models rather than to descriptions of real situations, it is nevertheless inappropriate to conclude that 'the fundamental laws do not govern reality' [HLPL, p. 162]. The fundamental laws govern the capacities which are picked by our models. They describe capacities actually at work in the world.[14]

[13] Chalmers 1993, 1996; Clarke 1995.

[14] Chalmers 1993, pp. 202-3.

Clarke, in his reply to Chalmers, denies that fundamental laws are true descriptions of capacities at work in the world, and argues that they are instead "licences to export information about capacities from ideal, simple circumstances to complex worldly ones". The reason that capacities are not truthmakers for laws is that in moving from the isolated controlled conditions of the laboratory to the complex messy situations of the real world:

> we don't know how the capacities which we have identified in the laboratory will react when interfered with by a worldly mix of other capacities. Lacking that knowledge, we make do, by assuming that capacities will behave invariantly and are therefore representable as laws. But, strictly, we are not entitled to this assumption.[15]

The idea is that since we can only uncover the capacities in particular controlled situations, we may have knowledge that those capacities exist, but have no knowledge of how those capacities will interact with other capacities in a more complex, worldly environment. The standard example to illustrate this point concerns variable capacities, that is, capacities whose instantiation depends upon their interactions with other capacities. For example, a sample of gas obeys the Boyle-Charles law in standard conditions, but fails to obey it at extremely low temperatures. A container of explosives may have a capacity to explode in its standard state of storage in the chook shed (where many explosives are apparently kept in Australia), but if those explosives become damaged or wet they will lose that capacity. A river has the capacity to flow towards a lower point on the Earth's surface, but will not flow if it is dammed, or frozen solid.

Thus we need to know more than the capacities present in a situation in order to predict an effect. In order to predict which capacities will actually manifest themselves, and how and to what extent they will manifest, we need to know the background conditions, as well as how the particular capacities that are present interact with each other. But our knowledge of the interaction of capacities is very limited, since it is derived from experimental situations that aim to minimise or eliminate interfering factors. Experiments can inform us of the existence of capacities, and how they behave in a small range of circumstances. But they cannot give us universal law-like generalisations about how those capacities will behave in the real world, when they interact with the myriad of other capacities that are present.

[15] Clarke 1995, p. 153.

In his reply to Clarke, Chalmers acknowledges this fact, that capacities interact differently in isolated, experimental situations to the way they interact in the real world. He explains this difference as being due to the capacities themselves being variable – the actual capacities vary according to the conditions they are present in:

> the crux of the matter is that [Clarke and Cartwright] have in mind a world in which the capacities that act in accordance with fundamental laws in idealised, experimental situations actually change outside of those situations. Fundamental laws lie because they attribute an unchanging behaviour to capacities which in fact change when interacting with other capacities.[16]

Chalmers then goes on to claim that this notion of a variable capacity, whilst being unproblematic in itself, does pose further questions concerning what makes the capacity variable. The variability of the capacity is, for Chalmers, a further fact to be investigated and explained, and "the scientific way is to attempt to explain changing capacities by appealing to unchanging ones at a deeper level."[17]

A distinction between *static* and *interactive* views on capacities can be helpful here, as a way of illustrating the conflict between Chalmers and Clarke. Chalmers assumes that capacities are static: they exist independent of interaction. On this view a variable capacity is variable because the capacity itself changes in different circumstances – thus Chalmers' desire to search for the underlying, fundamental causes of this change. In contrast, an interactive view on capacities sees the variability of a capacity not being due to the capacity itself varying, but being due to a different *expression* of that capacity under different conditions. The capacities themselves do not change when interacting with other capacities; they just manifest themselves differently in different situations. This is the view that Clarke adheres to. In Clarke's view, although we do uncover causal capacities under experimental conditions, we do not know enough about the way such capacities will interact and interfere with other factors in worldly situations to discover anything like fundamental laws. Thus, *contra* Chalmers, there is nothing profound about what makes a capacity variable – it is variable because it expresses itself differently in different contexts, when interacting with other capacities in complex ways. Certainly we should endeavour to investigate the complex

[16] Chalmers 1996, p. 151.
[17] *Ibid.*

expression of capacities, but it is not at all clear that this would involve unchanging capacities at a deeper level.

Unfortunately, Clarke gives no clear account of why knowledge of capacities derived from an experimental situation does not provide knowledge of a law, namely the *ceterus absentus* law that, in the absence of interfering factors, stuff with capacity X will tend to act according to its capacity. These are the types of laws that Chalmers appeals to in his defence of a realist interpretation of fundamental laws. Chalmers even uses such a view to embrace Cartwright's patchwork metaphor. He does not exclude the possibility of a messy world with a patchwork of irregular regions in which "capacities will be instantiated and inter-related in quite different ways in such highly differing regions, but not in a way that need threaten the claim that fundamental laws are true."[18] But on this view, what are the fundamental laws now telling us? They tell us how capacities manifest in abstract, isolated circumstances, such as are approximated in controlled experiments. As such, these fundamental laws have little power in more complex worldly contexts.

Looking at Cartwright's more recent (1999) clarification on what she means by the term 'capacity' further tells against Chalmers' position. Her point regarding the "open-endedness" of capacities, that "objects with a given capacity can behave very differently in different circumstances"[19], shows that she would clearly resist any attempt to derive fundamental laws from capacity claims. Since "capacities ... are not restricted to any single kind of manifestation"[20] any attempt to impose regularity on a capacity claim would be fruitless. Having a capacity to behave in such-and-such a way does not entail that the behaviour is law-like, regular, or even predictable. It merely means precisely what it states – the ability, perhaps even the tendency, to behave in a particular way. This irregular, potentially unpredictable, open-endedness of capacities shows that there is no real sense in which a capacity claim can entail the truth of a corresponding fundamental law.

The debate between Clarke and Chalmers is not only illuminating for what it tells us about capacity claims, it is also interesting because of the way these authors sidestep an extremely important issue. Throughout their discussions Clarke and Chalmers have entirely ignored the important distinction between what happens in experimental situations and what is described

[18] *Ibid.*
[19] Cartwright 1999, p. 59.
[20] *Ibid.*

by models.[21] By conflating these two points Clarke has missed a significant weakness in Chalmers' argument. The issue has to do with the relationship between abstracted idealised models and the real-world systems these models are simulating. Even though it may be that the fundamental laws are true in the models, there is no sense in which they are true even in carefully isolated experimental situations. The important difference between experimental situations and idealised situations is that although we aspire to set up the experimental situation to be as close to the idealised situation, we can never actually reach that ideal. We posit models in which there are strict laws that explain tendencies or capacities, but these models are not true representations of any real system, since in the model we are fully abstracting away from interfering causes, something you cannot fully do even in the best of controlled experiments. It is only in the models that the fundamental laws govern the capacities that are picked by those models, and thus *contra* Chalmers, the fundamental laws do not describe capacities actually at work in the world – they only describe capacities as they work in the *models*.

Making this distinction clear explains how knowledge of capacities does not require us to take fundamental laws as being true descriptions of those capacities. Instead, at best, we can export information about those capacities into complex worldly situations hoping that capacities will behave in an analogous way to the way they behave in idealised situations. The idea is that via controlled experiments we can uncover knowledge of the capacities present in nature. That knowledge is very limited: we know the capacities exist, yet we know little about how those capacities can be expressed. An idealised representation of the controlled experiment, in which interfering factors are fully eliminated, can be helpful as a tool for understanding how the capacity would be expressed in these ideal circumstances. Such a model can help inform us about the capacities, but only in a very narrow way. The *ceterus absentus* law that corresponds to the expression of a capacity in an idealised model tells us little, if anything, about how that capacity will behave in the real world. At best it gives us an approximation of the effects in controlled circumstances.

What emerges from the debate between Clarke and Chalmers is a recognition of the central importance of capacities and local causal facts. Worrying about the reality of fundamental laws, or whether the existence of capacities entail such laws, is not the most important issue

[21] Note that the term 'model' as used here refers quite generally to theoretical, mechanical or computational models, rather than to the formal definition of model as given by the semantic view of theories.

to focus on. What is more important is to study how theory, experiment and modelling can reveal capacities and inform us about how they are expressed in different contexts. This knowledge of capacities, which works from the bottom-up, beginning locally and expanding to more complex, more general contexts, is what underpins scientific explanation and prediction.

In the rest of this chapter I look more closely at this issue of the relationship between theory, model and reality. In doing so I take a slightly different focus, looking in detail at the modelling methods in conservation ecology discussed in earlier chapters – in particular at the Population Viability Analysis (PVA) techniques used to model Leadbeater's Possum (Lindenmayer and Possingham 1994; Possingham and Davies 1995). When discussing the types of examples that Cartwright and Chalmers tend to focus on – controlled experimental set-ups in physics – the distinction between experiment and model may not be all that great. The model is just a mathematical representation of the ideal case, which the experiment tries to approximate as closely as possible. However, when it comes to the more complex, stochastic modelling in ecology these issues are far more complicated, and the distinction between model and modelled situation is far more involved.

4. *Abstract Models and Controlled Experiments*

For Cartwright, investigating the interplay between model and experiment is crucial for an adequate analysis of science. In general a model is an idealisation of an experimental set-up, where interfering factors are abstracted away so that *ceterus paribus* conditions prevail. Of course, a real experiment can never be fully isolated from interfering factors, so *ceterus* will never be *paribus*. However, an experiment aims to try to get as close as possible to the ideal. Yet the relationship between experiment and model is not just one-way. In practice, the results derived by calculation from a model are compared with those measured in experiment, with the model being modified so that its results agree with observation. Often this involves adding what may seem to be arbitrary, *ad-hoc* additions to the model, which do not seem to accord with any physical process. However, Cartwright claims, their addition to the models is quite justified.

Cartwright illustrates this process, in HLPL, with the example of a small signal amplifier.[22] This example highlights the interplay of experimentally derived measurement with theoretically derived figures. In this example a real amplifier is represented theoretically by two different models, each of which can be applied to give rough estimates of the transistor parameters, without actual measurements being taken. These theoretical results are obtained by substituting known values into general equations, which describe the behaviour of such circuits. The problem is that these estimates are often grossly inaccurate, due to locally specific causal features in the actual circuit. To rectify this problem, measurements on an actual small signal amplifier are compared to theoretical values predicted by the models. These measurements are then used to add a correcting factor to the calculations, to bring the calculated values in line with the measured values. In making such an adjustment, the equations in the models are no longer generally applicable, applying only to the specific case.

The significance of this example for Cartwright concerns the truth of fundamental laws. Cartwright argues that even though the calculations in the model use general abstract equations, these equations fall short of representing genuine global laws of nature. The equations can serve as a guide in a specific case, but the corrections needed to make the output of the model accurate can only be determined by taking measurements in the specific context. The conclusion Cartwright comes to here is that the methodology is essentially local, since the corrections are derived from local measurements:

> But the improvements come at the wrong place for the defender of fundamental laws. They come from the ground up, so-to-speak, and not from the top down.[23]

Cartwright then goes on to point out that in these circumstances, as the equations are made more and more precise, they become less and less general:

> When we try to write down the 'more correct' equations, we get a longer and longer list of complicated laws of different forms, and not the handful of simple equations which could be fundamental in a physical theory.[24]

---

[22] Cartwright 1983, pp. 107-112.
[23] *Ibid.*, p. 111.
[24] *Ibid.*, p. 112.

Thus Cartwright argues that the methodology involved in this approach to physical theory is essentially bottom-up and localised, and does not involve fundamental laws of nature. The reference to Duhem here is also quite clear, with Cartwright rejecting the Duhemian view that physical theory is a "summary and classification" of a group of laws established through experiment. She also directly challenges Duhem's view that a model provides a merely superficial understanding of phenomena. Rather than regarding models as "embarrassments", Cartwright places the importance of modelling and experiment at the forefront of scientific discovery. Thus she rejects the ample 'French mind' approach to scientific theory, and instead endorses the deep approach of the English mind.

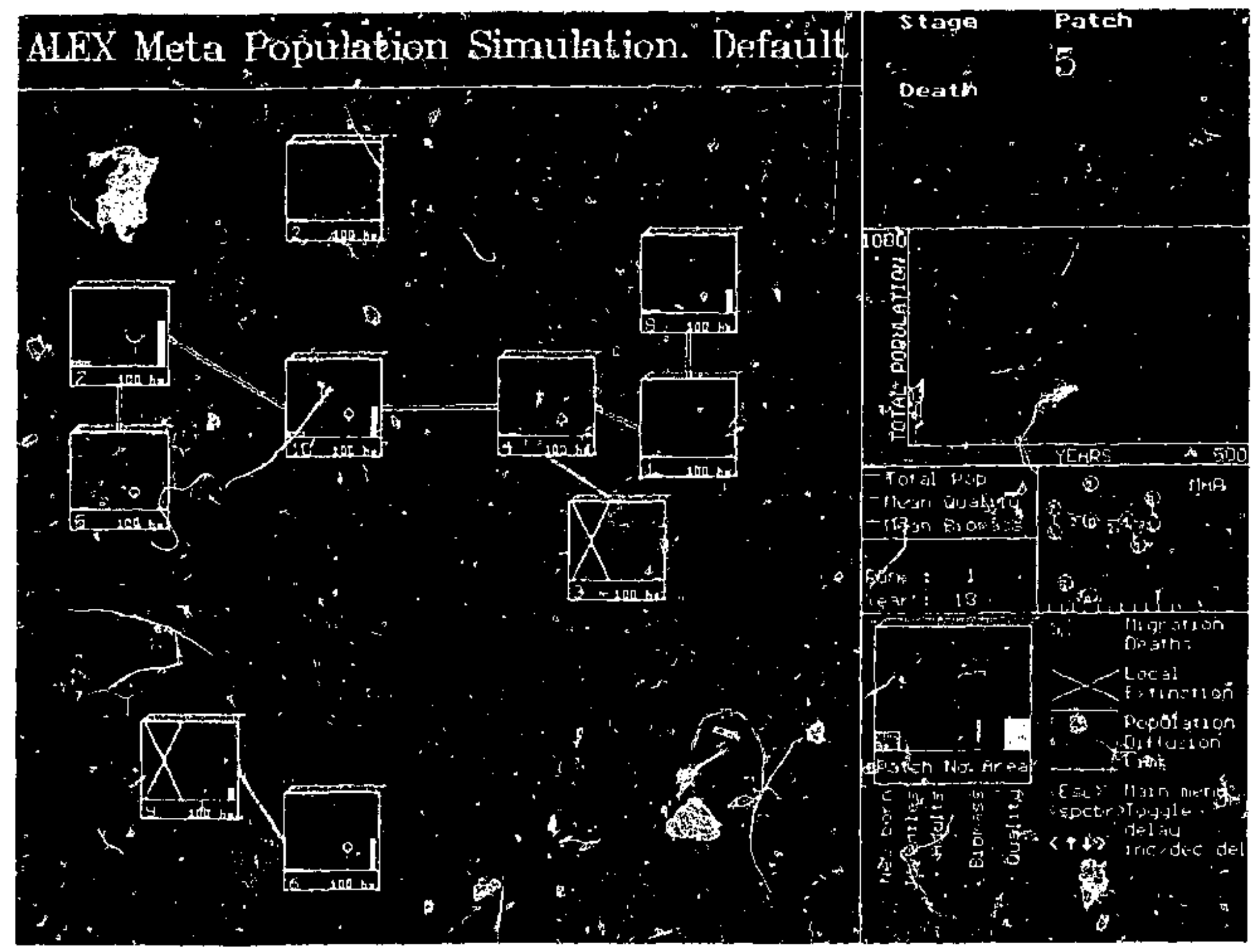
Clearly these ideas accord with what I have argued for in this thesis. Cartwright's approach to empiricism, and her broad disunified view of the sciences, allows for localised approaches to modelling to be seen as powerful approaches for generating scientific knowledge. There are, however, some significant differences between physics and ecology, especially regarding the use of experiment and modelling. The methods involved in Cartwright's examples from physics differ substantially from those used in conservation ecology, such as Population Viability Analysis. Thus it is worth taking a much closer look at the details of PVA modelling methods.

A significant difference between models Cartwright discusses and models in ecology is in the level and type of abstraction and idealization involved in the modelling. The type of idealizations that Cartwright typically talks about are those involved in physics and chemistry: a frictionless plane, a point mass, an ideal gas, a zero-resistance circuit. As Cartwright points out in NCM, these idealizations involve *substituting* idealised properties in the place of actual properties. Abstraction typically involves *subtracting* interfering and irrelevant factors from particular phenomena.[25] In the models used in physics, this leaves us with idealizations such as those listed above, which although abstract are nevertheless quite familiar. In contrast, in the population modelling of Lindenmayer and Possingham's (1994) Leadbeater's Possum PVA, the level of abstraction and idealization is far more severe. The modelling software used, known as ALEX (Analysis of the Likelihood of Extinction), makes a number of significant simplifying assumptions, which are necessitated both by lack of data, and the need to keep the model simple enough to be workable.

[25] See Cartwright 1989a, Chapter 5, and Clarke 1999, Chapter 5, for more detail on the difference between idealization and abstraction.

The model works by simulating populations of Leadbeater's Possum residing in various habitat patches, scattered throughout a region. These patches are representations of actual habitats within the region under investigation. They are distributed in the model so as to reflect their actual distribution throughout the region, with migration corridors between the habitat patches also included in the model.

The representation of habitat and population levels within the model is illustrated in the following screen-shot from the demonstration version of ALEX:[26]



In representing an actual habitat by such a simplified model a large degree of idealization has already occurred. In ALEX there are many other simplifications and idealizations made in order to make the model workable, and to account for the limitations in the data available. The most significant of these include:

[26] The demonstration version of ALEX, along with documentation, can be downloaded from the following URL: http://biology.anu.edu.au/research-groups/ecosys/Alex/ALEX.HTM

- Modelling only one sex – assuming that the fate of females is the only relevant fact for the viability of future populations.
- Assuming that there are only three significant age classes: newborn (animals born that year), juveniles (animals more than one year old who cannot yet reproduce) and adults.
- Assuming that genetic factors have no impact on the short and long-term viability of populations.
- Representing the ecological significance of a particular habitat patch by just three habitat state variables, representing the availability of food and appropriate shelter and nesting sites. In doing so, the influence of factors such as steep topography is ignored, even though such a factor has been shown to be significant.
- Simplifying, and in some cases completely ignoring, the effects of relevant disturbance processes such as fires and logging practices.
- Restricting analyses to individual forest blocks, ignoring the effects of neighbouring forest patches not included in the study.

The model runs by simulating the annual life-cycle of the species, in order to determine the population levels and their distribution across the habitat patches for each year. Lindenmayer and Possingham run their model simulating a few different time scales, ranging from 150 to 750 years. These different time scales allow a number of different scenarios to be explored. As discussed in Chapter Three, this PVA utilises Monte Carlo methods to simulate the essentially random nature of the processes involved: the chances of births and deaths, the possibility of migration, and the chances of catastrophes occurring. Because of this, for each scenario the model is run at least 300 times, in order to minimise the discrepancies which occur because of this stochastic nature of the modelling. If the model was only run once, for example, and a rare global catastrophe such as an enormous fire occurred in that run, this would paint a particularly unrepresentative picture of the survival prospects of the species. Thus running the model a large number of times gives a reliable indication of the chances of survival for the species over the different time scales.

In this modelling the many complex, inter-related processes that contribute to the life of the species are reduced to a small number of processes. Each year is simulated by reducing it to this small series of significant stages, modelled in the following order:

- Biomass – determining habitat quality of each patch (available food and nesting sites) from state variables.
- Births and deaths – births are calculated using a submodelling package; deaths are derived from an annual probability for each age class.
- Diffusion and migration – movement between habitat patches. Diffusion is movement across a corridor between two patches, calculated via a submodel. Migration, also calculated by a submodel, is movement not through a corridor, which occurs when population density in a patch surpasses a particular threshold.
- The effects of catastrophes, such as fire, disease, or climate change. These are modelled according to the particular catastrophe – they can have a local or global effect, they may depend upon habitat variables or population levels, or may be entirely random.

In modelling the processes that affect the life-cycle of the species over a year in such a simplified way there is a large degree of abstraction and idealization. Each of the various submodels that are involved in these stages incorporate significant idealizing assumptions. This occurs, for example, in reducing the amount of available food to a single biomass parameter, or in determining the effect of a global catastrophe via an abstract submodel. The important issue for my concerns is the relationship between the abstracted and idealised components of the models, and the real entities, properties and processes they are attempting to simulate. Obviously the situation is far more complex than in the example of the small signal amplifier, since in the PVA modelling, the components of the models are much further removed from their real-world analogues.

As discussed in Chapter Three, it is more accurate to think of the type of modelling involved in PVA as a form of experiment, rather than as an abstract representation. Viewing the modelling in this way allows us to think of it as a substitute for the sorts of real-world experiments we would have to undertake to come up with the best conservation strategies. In most cases it is impossible to do such field experiments in ecology. For example, we cannot set up an experiment which will determine the optimal reserve design to protect a particular endangered species – there just aren't enough animals left, not enough land, and certainly not enough time. Yet some forms of field experiments are certainly possible in conservation ecology. Thomas Lovejoy's experiment in the Amazon, the Minimum Critical Size of Ecosystems project, is an excellent example of the sort of empirical fieldwork that can be done in ecology.[27] Simberloff and Wilson's (1969) Island Biogeography experiments in the Florida Keys are another good example. This type of work will almost certainly be helpful in determining strategies for preventing future extinctions – in particular, it may give us a broad idea of some of the longer term effects of the sort of rapid habitat depletion that is presently occurring. However, as argued previously, in terms of particular conservation strategies for a particular context, this knowledge will only be of limited use.

Viewing modelling as a form of experiment means that the sort of analysis that Cartwright applies to the small signal amplifier cannot be applied straightforwardly in this case. Since in PVA the model *is* the experiment, the idea that the model can be improved by comparing its output with experimentally discovered results does not apply. This leaves us with the problem of how to adjust the model in order to make it more accurate. In the case of the small signal amplifier real world measurements are used to improve the accuracy of the model. In a similar way, the collection of more thorough ecological and geographical data can help improve the accuracy of the PVA model. But there is a crucial difference between the two cases: *it is impossible to test the accuracy of the predictions made by the PVA model.* This is because PVA modelling simulates population levels over a timespan of tens to hundreds of years, and we can't wait that long. We are just not in a position to compare the predictions of the model with actual outcomes. We cannot wait 150 years and then count the number of possums in the highlands to test our theory. The species is already close to extinction, and we need to choose a course of action *now* in order to prevent this from happening.

Fortunately, there are ways of improving the accuracy of PVA modelling without having to wait for so many years. Part of this process involves understanding the limitations of such modelling, looking at what sorts of predictions can be derived from these models and how reliable they are. Thus, in their Leadbeater's Possum PVA, Lindenmayer and Possingham do not attempt to derive specific predictions of the effects of particular conservation strategies. The best sort of prediction they can make is a *ranking* of the various strategies in terms of their utility for conservation. In taking this approach Lindenmayer and Possingham diverge from some of the original aims of PVA, as outlined by Boyce (1992). Rather than trying to determine a specific probability of extinction for each management option, the Leadbeater's Possum PVA simply aims to compare the effectiveness of each strategy. Thus the fact that

[27] A good account of this project is in Quammen 1996, pp. 449-498.

particular extinction probabilities are not all that reliable is not that important. What is important is that the modelling can successfully simulate population levels in a way that allows a valid and accurate comparison of the options.

Clearly, undertaking more thorough fieldwork, and learning more about the relevant ecology of the species in question, can improve the accuracy of the model's parameters and functions. A better understanding of the properties and processes involved in the life-cycle of the species allows for more accurate modelling of these factors. In doing so it is possible to compare the output of certain aspects of the model with what occurs in the real world, since the time scale involved in these processes is relatively small – months, rather than years. This can be achieved by incorporating submodels within the broader model, to allow for more careful 'tweaking' of these smaller scale processes. The Leadbeater's Possum PVA utilises a number of submodels in this way, such as for determining the number of births each year, and for determining the habitat quality of each patch. The elements of these models are clearly designed to model real world properties and processes. Yet there is no implication that the equations and algorithms within the models are directly representative of actual processes. The representations are highly abstracted, yet they are designed to be more than just empirically adequate, for they aim to capture the effects of underlying causal processes.

For example, in determining the habitat quality of a particular patch, the effect of fires is one of the significant factors. Immediately after a fire the habitat quality becomes very low, due to a severe lack of available food. As the patch regenerates the availability of food increases to a maximum level, about 35 years after a fire. Then, around 65 years after a fire, the availability of food begins to decline until it reaches a minimum after 100 years. These dynamics have been determined by doing fieldwork, assessing the amount of available food in different forest blocks, at different stages of recovery after a fire. They are implemented in the model by equations which represent this process. There is certainly no implication that these equations have anything to do with genuine laws of nature. Yet there is an understanding that they are in some way a representation of an underlying causal process, by which a fire first destroys available food, then promotes abundant regrowth, which reaches a peak before reducing to a stable level. As more fieldwork is done, and these causal processes are further understood, the accuracy of the modelling can be improved.

Another important tool for testing the reliability of a PVA model is the process of sensitivity analysis. This involves testing how sensitive the model is to variations in the input parameters. If small changes in the input parameters result in a wide range of extinction probabilities being output by the model this shows that the model is very sensitive to these inputs. In this case, small uncertainties in these parameters, which are inevitable due to the difficulties in collecting detailed data, can lead to the model being extremely unreliable. However, if the model's output is shown to be robustly stable for small differences in inputs, this shows that the model can reliably be applied even when there is some uncertainty in the parameters. Given that the Leadbeater's Possum PVA merely ranks the different management options, rather than seeks to determine precise extinction probabilities, sensitivity analysis in this case involves determining whether small uncertainties in parameters can result in different rankings. This is achieved by running the model many times, with the particular variables being varied automatically, and the results scrutinised for sensitivity to these variations. Significantly, techniques such as sensitivity analysis are very similar to the sorts of sensitivity checks that are made on experimental set-ups in controlled laboratory experiments. This again emphasises that such modelling should be viewed as a form of experiment, a point very similar to that made by Galison (1997) in his discussion on Monte Carlo methods in atomic physics.

What can be seen from this more detailed look at PVA is that although there are important differences between the use of models in physics and the modelling methods in conservation ecology, there are also important similarities in the ways that the models are constructed, tested and improved upon. By looking more carefully at the relationship between model and reality, and by viewing modelling as a form of 'virtual' experiment, a more accurate picture of the role of such modelling can be painted. Understanding the bottom-up nature of these modelling methods can help to explain how these models can be representative of real phenomena, while involving a large degree of abstraction and idealization. Looking at the components of a PVA model, in particular the equations and algorithms of the model and its submodels, as being somewhat abstract representations of underlying causal phenomena, gives us a way of linking the components of the model with their real-world counterparts. By continuing to understand the processes in the real world more precisely, via work in the field, the model can be constantly improved. This can occur by: improving the accuracy of the equations and algorithms used; designing the model to be more robust and less sensitive to uncertainties in the input data; being more precise in the selection of relevant properties and

processes to model; and by being more careful about the abstractions and idealizations involved in the modelling.

Of course, ecology is a science in its relative infancy, and thus there are still many problems and weaknesses with these types of modelling methods. In general, these approaches are still extremely difficult, extremely expensive, and can only deliver somewhat vague predictions that are often plagued by substantial uncertainty and error. One of the major problems with PVA modelling is that the inherent complexity of the method often means that adequate peer review is extremely difficult, if not impossible to achieve. In this respect the criticisms raised by Boyce (1992), that most published PVA studies only include outlines of model structures, are still of serious concern, for they imply that these studies lack the necessary detail for scrutiny. Even in the case of the Leadbeater's Possum PVA, one of the most detailed PVAs published, the literature does not go into nearly enough detail on the algorithms used in the modelling. In particular, there is almost no description of the way Monte Carlo routines are implemented to model the important stochastic processes. Because of this lack of detail it is difficult for independent scientists to assess and review particular work.

The other problems with PVA modelling concern how its results can be used to guide conservation management and policy. Given that the predictions are only vague, and are often deeply infected with uncertainty, the application of PVA analysis to policy initiatives is not a straightforward matter.[28] There is also the problem that PVA studies can only be done for a small number of species at a time. Thus an approach based on PVA may only benefit a few charismatic species, at the expense of the majority of threatened species. In response to these problems Possingham *et al* (2001) advocate the use of PVA as a "decision support tool to help make management decisions."[29] The idea is that PVA can be used as a tool to help determine the most effective conservation strategy to apply in a particular context, exemplified by the way the Leadbeater's Possum PVA ranks the management options. However, for these authors, the initial goal is still the future viability of a particular species, rather than a more generally applicable goal. Thus their approach still suffers from the second problem. Despite this weakness, their strategy can be adapted to apply more broadly. Rather than beginning with narrow concerns, such as the viability of a single species, this decision-making process could begin with broader policy objectives, such as the protection

[28] Burgman & Possingham (2000) provide a good overview of these issues, and explore the ways PVA can contribute successfully to management policy

[29] Possingham *et al* 2001, p. 2.

of biodiversity within a particular region. From these broad objectives, within the particular context (such as competing interests in the region, the conservation budget, and other political concerns), the possible management options are listed. These are then compared via relevant PVA studies, which give a guide as to the best strategy to implement. This process is still costly, time-consuming, and difficult, but it should provide the most effective strategy for good conservation.

There is also the hope that results from one context can help in other contexts – that conservation strategies can be generalised to some degree. Where this is possible it is because of relevant similarities between the cases, which allow strategies from one case to be exported to cases involving similar processes and properties. As such, this involves approaching each case from the bottom-up, looking at the components and mechanisms involved in each case, and assessing the similarities between the cases on that basis. Significantly, this process does not involve any general laws of nature, but instead relies on judgements of similarity between the local factors in the particular cases. For example, the techniques used in the Leadbeater's Possum PVA have been fruitfully applied to species with similar ecological traits in similar circumstances: the Greater Glider (Possingham *et al* 1994); the Yellow Bellied Glider (Goldingay & Possingham 1995); and the Greater Bilby (Southgate & Possingham 1995). All these studies utilised the same computer modelling package (ALEX), which was configured according to the specifics of each case. This shows that although the use of PVA is restricted to a case-by-case basis, successful approaches in one case can be successfully exported to similar cases with relative ease.

5. *Concluding Remarks*

Throughout this thesis I have defended the validity and importance of bottom-up, *localised*, methods in science. Such a methodology is based on the uncovering and modelling of local causal and stochastic processes, involving locally specified capacities, properties and kinds. By rejecting the central importance of fundamental laws, being open to the possibility of disunity, and focussing on solving particular problems in particular contexts rather than developing generally applicable theories, a localised methodology can deliver powerful predictions and detailed explanations. Accepting the validity of such approaches allows scientific discovery to be achieved via process-driven modelling, from the bottom-up, rather than via models structured from the top-down.

By looking at the world from the complex, disunified perspective of the broad English mind, many of the apparent problems with the so-called 'special sciences' disappear. The dearth of laws in these sciences is not seen as a problem at all, since laws are not at all necessary for achieving what we require from our science – localised modelling can give us all we need. By approaching scientific knowledge from the bottom-up we can also avoid the pitfalls of applying apparent laws or generalisations in contexts where they are rendered invalid by the local circumstances.

Taking a localised approach to science can also help overcome many of the difficulties associated with defining natural kinds. Many important natural kinds in biology cannot be defined in a unified way – there is no single, universally applicable definition which covers all the roles that kinds may play in all the contexts they appear in. The kind *species* is a prime example of this, with there being no way to unify the rival definitions of the concept. This would be a problem if we required natural kinds to be universally defined. But by thinking locally, defining problematic kinds, such as *species*, from the bottom-up, on a case-by-case basis, we can achieve a workable and realist conception of these kinds.

Rather than trying to fit the world into a single, all-encompassing model, a localised methodology allows for a more complex, patchwork picture of the world, compatible with Cartwright's conception of the "dappled world". By investigating nature through bottom-up methods, a broad and disunified view of the world can be pieced together. The picture won't be all that neat, and it almost certainly won't be unified, but what it lacks in beauty and simplicity it more than makes up for in fine detail and heuristic power. Piecing the world together in this way, by thinking locally, we can achieve all we desire from science.

Addendum[1]

1. *Localism in Theoretical Ecology*

1.1 Introduction

In Chapter 3, *Local Thinking in Ecology*, I argue that locally based, bottom-up methods in ecology should be viewed as being at least as powerful as traditional top-down approaches, if not more powerful in some contexts. In particular, I conclude that we should see the traditional conception of science as incomplete, such that we should accept local approaches to science as being as valid as traditional global approaches, and that we should be open-minded as to which approach will suit a particular problem in a particular context. I also argue that in some cases the localised bottom-up approach is far more powerful than a top-down approach.

The argument I present for the power of local thinking in ecology involves problem solving in conservation ecology, a distinctively applied science. Given that conservation ecology is a form of ecological engineering it isn't surprising that the local, context-relative and goal-relative factors are of prime importance for successful conservation in a specific context. Just as local factors are of prime importance in other forms of engineering, such as bridge building, in a way that does not challenge the global nature of the laws on which the engineering is based, why assume that the essential localism of successful conservation ecology challenges a global approach to a more fundamental theoretical ecology which underpins the applied science? Thus it might be thought that the local-global issue is in some way prejudged by choosing as the main example one from an applied ecological science.

In response to this I should first point out that the example discussed in Chapter 3, although framed in terms of conservation ecology, begins with a discussion of a significant example from theoretical ecology: MacArthur and Wilson's *Theory of Island Biogeography*. This theory has a detailed theoretical foundation in the dynamics of species migration, competition and extinction, which underlie the species-area relationship that is the mathematical expression of the theory. Yet even purely as a piece of theoretical ecology Island Biogeography is deeply problematic. Certainly the predictive utility of the theory is extremely limited, since particular local factors in each context will tend to override the general species-area relationship. Similarly, any explanatory power the theory has can be reduced to more fundamental local factors: the fact that the species-area relationship is of a particular sort can be better explained by an analysis incorporating local ecological factors. Additionally, the fact that Island Biogeography is of little use in solving the SLOSS problem is further evidence of this point. Thus, for purely theoretical reasons, the theory of Island Biogeography is better seen as a statement of a low-resolution generalisation that acquires its explanatory and predictive value from below rather than from above.

The second significant point is that there is an important difference between the application of theoretical ecology and the application of more fundamental physics in engineering. There is a difference between the use of equations from physics in, say, bridge building, and the use of ecological generalisations in areas of applied ecology such as conservation ecology. When bridges are built the equations of physics form the basis of the calculations upon which bridge engineering is achieved, with the local factors serving as inputs to these equations. In contrast, in conservation ecology, broad ecological 'laws' such as the species-area relationship play *no role whatsoever* in the models used in bottom-up methods such as Population Viability Analysis. Admittedly, some lower level generalisations are used in the models, such as birth and death rates, migration rates and the determinants for habitat quality; however, as discussed in Chapter 3, these are merely low-level generalisations which make no claim other than being generalisations of empirical data – they do not profess to represent genuine regularity or laws of nature. They are merely idealisations that are in some sense justified by the empirical data, statistical generalisations of some underlying causal and stochastic process. Thus it is difficult to see how the global approach contributes in any significant way to these powerful modelling techniques of applied ecology.

To further emphasise this point I now discuss another example from theoretical ecology. This example comes from Community Ecology, the field of ecology that explores the relationships between a number of

[1] This addendum was written to accommodate the amendments requested by one of the examiners of this thesis, in order for the thesis to satisfy the requirements for a PhD.

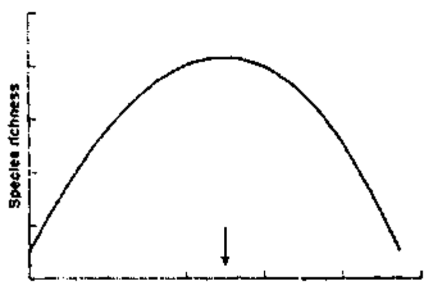
interacting species in a community, as well as the interactions between these complex systems and their particular environments. In many ways this example is structurally similar to the example discussed in Chapter 3 – a theoretical generalisation is shown to have some empirical support, but when examined in more detail it is clear that the basis of the generalisation is from below rather than from above. However, the example is theoretically more complex than that of Island Biogeography, and in the discussion I emphasise the theoretical aspects of the theory rather than just its applications. This will provide further support for the idea that ecological theory is best understood as a bottom-up enterprise.

1.2 Disturbance and Community Richness

In its most general form, a *community* is defined as a collection of populations of organisms bound by a particular locality.[2] The *richness* of a community is defined simply as the number of species that comprise the community.[3] The investigation into the relationship between *disturbance* and *community richness* purports to provide an insight into the factors that contribute to levels of biodiversity, as well as providing us with practical help in terms of achieving maximal biodiversity in a particular community. However, even though there seems to be a fairly general relationship between the amount of disturbance a community is subject to and its level of richness (number of species that comprise the community), this in no way comes close to expressing a robust generalisation of detailed use in explanation or prediction. Where the relationship holds it holds because of local ecological factors, rather than because of the truth of a general relationship: its support comes from below rather than above.

The standard view in Community Ecology used to be that communities existed in an equilibrium state, and that the effect of disturbances was to shift the community away from that equilibrium. The resilience of the community was then determined by its ability to return to the equilibrium state. Connell (1978) challenged this standard view, pointing out that communities rarely, if ever, reach an equilibrium state. Instead, communities are in a constant state of flux, and are often dependant upon that flux to maintain their level of diversity and richness. Since the world is not uniform or stable, ecological communities are dynamic entities, non-uniform, and constantly changing. Thus one of the most important factors that affects the richness of a community is the effect of disturbances such as predation, disease, extremes of climate, and catastrophes such as fires, earthquakes and the impact of human development. When the disturbance regime affecting a particular community changes this will often have a significant effect on the richness of a community. For example, in an ecosystem with a history of regular disturbance, in which many species have evolved to depend upon the conditions created by the disturbance, the cessation of such disturbance will most probably result in the decline and eventual extinction of those species. Similarly, if disturbances increase this will be likely to have a detrimental effect on many species.

Because of consideration such as these, Connell proposed the *intermediate disturbance hypothesis* (*IDH*). The *IDH* states that the biodiversity of a community is maximised at an intermediate level of disturbance, as illustrated in the following diagram.[4] Connell also presented ample empirical evidence to support the *IDH*, citing studies of forest communities in Uganda and Nigeria, as well as personally testing the hypothesis in Queensland rainforest communities and on coral reefs at Heron Island, Queensland.



The intermediate disturbance hypothesis of species diversity. This model predicts that biodiversity is maximized at some intermediate level of disturbance. (red arrow). (Modified from Huston 1979.)

These ideas were further developed by Huston (1979), who explored the details of non-equilibrium population dynamics, applying the Lotka-Volterra competition equations to assess the effect of competition between species. Huston modelled communities of six species under three sets of conditions: with no disturbances; with a moderate number of disturbances; and with frequent disturbances. The results of the modelling showed that with few disturbances competitive exclusion takes place rapidly and extinctions are very likely. With a high level of disturbance the species are ill-equipped to recover between disturbances, so extinction is rapid. It is at intermediate frequencies of disturbances that diversity is maintained at a higher level, and for longer. Thus Huston's simple model provided further theoretical support for the *IDH*.

---

[2] See Allen 1998, pp. 317-329 for a more thorough discussion on the definition of *community*.

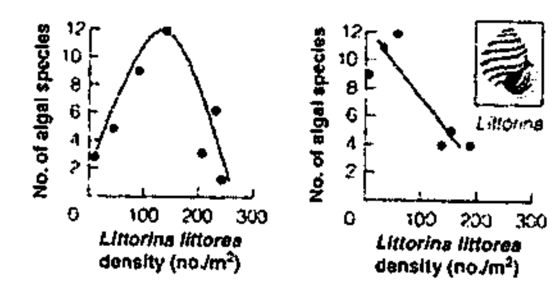
[3] Note that the *richness* of a community does not necessarily reflect its *biodiversity*. Biodiversity calculations normally take into account not just the number of species, but also the distribution of species abundances within a community. Thus a community with a large number of species may still have low biodiversity if most of those species are extremely rare. See Begon *et. al.* 1986 pp. 594-597 for more detail on biodiversity calculations.

[4] This diagram, and those that follow, are all taken from Krebs 2001, pp. 453-4.

The work of Lubchenco (1978) also provided support for the *IDH*, as well as showing its limitations. Lubchenco showed that richness of algal communities (number of algal species present in a community, in this case defined by confinement to a tidal rock pool) was crucially dependent upon the effect of predation by the periwinkle snail *Littorina littorea*. When *L. littorea* were present in low densities species richness was low, because the dominant algal species, the green *Enteromorpha*, out-competed the weaker red and brown algae. When *L. littorea* were present in high densities species richness was also low, because the periwinkle snail consumed all the palatable algal species to extinction. At intermediate densities both competitive exclusion and over-predation were prevented and many species coexisted.

A survey of a number of high-tide pools showed that the algal communities conformed to these patterns, as represented in the left-hand graph on the following diagram. This curve shows further evidence for the *IDH*.

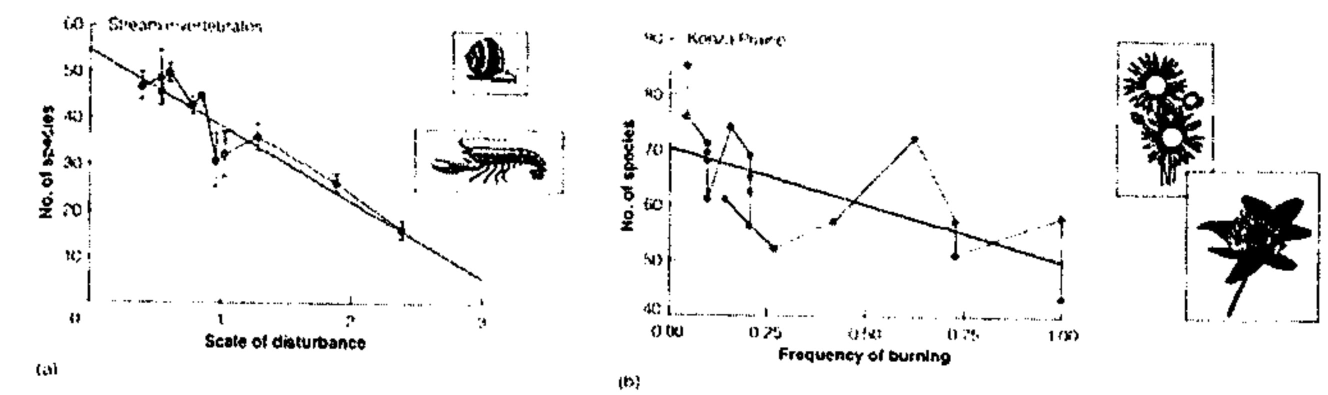
However, surveys taken on similar algal communities on emergent rocks in the low intertidal zone did not show the same pattern. For these communities a low density of the predator *L. littorea* correlated with high richness. The reason for this is that in this environment the multitude of red and brown algae species predominate, and the periwinkle snail's preference for green algae has little bearing on community richness.



The effect of periwinkle snail grazing on the diversity of algae in (a) high-tide pools and (b) on emergent rocks in the low intertidal zone in Massachusetts and Maine. The intermediate disturbance hypothesis applies only in the tide pools. (From Lubchenco 1978.)

Lubchenco's work showed that the validity of the *IDH* depends crucially upon local ecological factors. Under conditions where increased disturbance limits the dominant species in the community the *IDH* seems to hold, whereas under different conditions and different types of disturbance the *IDH* is falsified. Thus it is only with locally particular ecological knowledge of the structure of the community that the *IDH* can be successfully applied. But in that case the *IDH* is merely a descriptive summary of these ecological properties – it does not supply any further explanation for the particular community structure. Thus if the *IDH* applies it does so because it is supported from below (because of the local factors) rather than from above (because it is a general law).

Two further examples of the failure of the *IDH* are presented by Krebs (2001), as illustrated below:



The effect of disturbance on species richness in two contrasting communities. (a) Aquatic invertebrates in streams on the South Island of New Zealand. The scale of disturbance is a composite measure of variation in temperature, stream flow, and bottom stability. (b) Plant species richness in tallgrass prairie in Kansas. The frequency of burning is the probability of being burned each year between 1972 and 1990. Because species richness declines with the amount of disturbance, the intermediate disturbance hypothesis does not apply to either of these communities. ((a) From Death and Winterbourn 1995. (b) From Collins et al. 1995.)

Thus despite the widespread evidence for the *IDH*, it is not always the case that maximum biodiversity is produced when there are intermediate levels of disturbance. The *IDH*, although shown to apply in many different cases, is not a general hypothesis. The implication of this fact for applications of the *IDH* are significant. Since the hypothesis is not a robust generalisation applicable to all communities, it cannot be applied generally. The validity of the *IDH* is crucially dependant upon the local ecological factors which affect the community. As Krebs (2001) states:

> The intermediate disturbance hypothesis is an attractive hypothesis for the maintenance of high species diversity in communities, but it does not apply to all communities, and further work is needed to delimit its range of application. In particular, land managers should not assume the validity of the intermediate disturbance hypothesis in making management plans for national parks or other protected areas.[5]

Since, as the *IDH* hypothesis stands at present, there are no general schemes for delimiting the cases in which the *IDH* applies (and good reason to believe there could never be such general schemes), the only legitimate way of interpreting such a hypothesis is by thinking locally.

2. *Clarifying the Local-Global Distinction*

Initially I characterise the local view as being based on locally specific models, derived from causal and stochastic processes, involving 'case study' oriented approaches that are 'bottom-up' structures based on local causal and stochastic facts.[6] However, as I have characterised it, localism has not been all that rigorously defined. In particular, the definition is more by way of contrast with what I term the 'global view' rather than an explicit account of the 'local view'. As such the contrast leaves open a number of possibilities, as noted by Sterelny[7]: the use of models rather than theories; testing via computer models rather than "wet" experiments and/or analytic techniques; an interest in the ability to predict and manipulate the outcomes of a specific situation rather than the ability to generalise across a large number of somewhat similar situations; the idea that the hard work/interesting science is to determine initial conditions and parameter values for a model rather than in determining the basic form of the equations; the distinction between tacit, know-how understanding (local, English, ample) and explicit knowledge-that understanding (global, French); the distinction between holistic understanding, and understanding via functional decomposition of the elements in the system; the rejection of the idea that special sciences reduce, in any interesting sense, to more fundamental ones. Note that each of these possibilities involve quite different conceptions of the contrast between global and local thinking, and do not necessarily form a package deal.

Thus, to clarify localism, it is the use of *models* rather than *theories* that is definitive of the local approach. This distinction between model and theory is just Duhem's distinction between the approaches of the ample English mind and the deep French mind, as discussed in detail in chapter 2. It is also precisely the distinction endorsed in the U.S. National Academy of Sciences and National Research Council report (Orians *et al*, 1986), as discussed in Chapter 3. From this definitive contrast the other conceptions as listed above may apply to a greater or lesser degree, depending upon the nature of the particular models. In particular, the question of reduction of 'special sciences' to 'more fundamental' sciences becomes one which concerns the basis of the models involved. Importantly, interesting reductions may be possible, and holistic understanding may also be possible on the local view, but they are not necessarily possible.

Clearly this local approach is compatible with Cartwright's view of science as discussed in Chapter 5, since Cartwright emphasises that science is about model construction and the interplay between these models and empirical data. Thus Cartwright's ideas fit neatly into the localist picture. However it might be thought that Cartwright's metaphysics is incompatible with the local view, since Cartwright argues that science is concerned with the hunt for causal capacities of kinds, and these capacities are supposed to be held universally. In response, I argue that this global interpretation of Cartwright is deeply mistaken. For although Cartwright does argue that capacities are universally held properties of things, the capacities themselves as best understood from a local perspective. This is because the manifestation of the capacities is something that may only be fully understood by thinking locally:

> Disposition terms, as they are usually understood, are tied to one-to-one law-like regularities. But capacities, as I use the term, are not restricted to any single kind of manifestation. Objects with a given capacity can behave very differently in different circumstances.[8]

Given that the behaviour of capacities is something that varies according to the circumstances, their manifestation may not be amenable to any lawlike explanation. Capacities may, of course, manifest in a regular way, but they do not necessarily do so. To claim they do would amount to being guilty of what Cartwright terms "fundamentalism". Thus although Cartwright endorses an essentialist view about kinds, it is a view that is clearly incompatible with a fully global approach to science. Cartwright is essentially a localist.

[5] Krebs, 2001, p. 453.
[6] p. 5.
[7] From the examiners report.
[8] Cartwright 1999, p. 59.

3. *Metaphysics and Methodology*

In this thesis I largely endorse both Cartwright's approach to scientific methodology and Dupré's "promiscuous" pluralist approach to natural kinds. Yet Cartwright and Dupré hold views that, on the surface at least, seem to be quite incompatible. The first tension is that Cartwright's approach is methodological rather than a metaphysical, whereas Dupré clearly engages in some speculative metaphysics. The second clash is that between Cartwrightian essentialism and a Dupreved views of kinds: Cartwright endorses an essentialist position, where kinds are understood as possessing particular causal capacities, whereas Dupré explicitly rejects any essentialist view of kinds. There are thus two points that need further elaboration: the relationship between methodology and metaphysics; and reconciling Cartwright and Dupré on natural kinds.

In Chapter 1 I state that I shy away from bold metaphysical claims of disunity, such as those typically held by Dupré. Instead all I wish to do is account for the *possibility* of such metaphysical disorder. As such, the localism I advocate is primarily a methodological rather than a metaphysical thesis. It is the methodology of a scientific investigation and the corresponding construction of scientific theories and models that primarily concerns me. In doing so I side with Cartwright's epistemological approach to science, as outlined in Chapter 5. However, despite my ambivalence towards pure metaphysics, the discussion of kinds in chapter 4, and Cartwright in chapter 5, clearly introduce important metaphysical motifs into the thesis. This is most significant in my adoption of an explicitly realist approach to natural kinds. So how can this dabbling in metaphysics be reconciled with my metaphysical agnosticism?

The answer to this question involves looking again at how Cartwright reconciles her empiricism with her explicit appeal to the metaphysics of causal capacities. This involves the recognition that Cartwright's philosophy of science is not abstract, but is rooted in the everyday practice of scientists. The metaphysics is just what emerges from her account of how science works and how scientific knowledge claims are constructed and justified. To emphasise this it is worth repeating Cartwright's comments on this matter:

> I think the reason I want to pursue these metaphysical views – that capacities are basic, not laws – is not as a piece of metaphysics, that I want somehow to get the ontology of the world right, but rather that I think that certain ways of picturing the world lead to certain scientific methodologies, and that some are better than others.[9]

Thus taking an approach that places methodology prior to metaphysics does not necessarily rule out all possibility of making metaphysical claims. As such, there are important methodological reasons for endorsing a realist view of natural kinds, since realism is itself an important methodological aim of science.[10]

Reconciling Cartwrightian essentialism and a Dupreved views of kinds involves taking a closer look at both Cartwright and Dupré's views, to see that the apparent tension is illusory. Firstly, as discussed in the previous section, although Cartwright may seem to endorse a simple, global essentialist view on kinds, it is clear upon further examination that her notion of capacity is a distinctly local concept. Thus Cartwright's essentialism does not involve any deeply universal assumptions, and so is amenable to a genuinely pluralist approach to natural kinds. Secondly, although Dupré explicitly rejects an essentialist view of natural kinds, there is a way of reconciling his anti-essentialism with a form of essentialism. This involves endorsing the concept of *causal homeostasis*, as outlined by Boyd (1989, 1991, 1999). This approach captures the essentialist intuition that there is something special underlying a classing which is a genuine natural kind, whilst maintaining the intuition that kinds can be united in a vague way via possession of a significant number of the properties in a common property cluster. This approach fits in excellently with Dupré's approach to natural kinds, since it does not involve what Dupré considers problematic about essentialism – there are no particular properties that provide the essence of the kind. It also satisfies the essentialist intuition that there is some matter of fact that unites the members of a kind. Finally, since these matters of fact are real properties and processes, it is a realist conception. Thus endorsing a *causal homeostatic* account of natural kinds allows a way of accommodating all of what Cartwright and Dupré require from natural kinds without sacrificing realism.

Given this 'more nuanced' discussion of Cartwright and Dupré, it is clear that there are no deep conflicts between their views on methodology, metaphysics, and the nature of natural kinds.

[9] Pyle (ed.) 1999, p. 208.
[10] See Chapter 4, section 2.3, pp. 102-5 for more on this point.

4. Bibliography Addendum

Allen, T. F. H. 1998. "Community Ecology." From Dodson, S. I., T. F. H. Allen., S. R. Carpenter, A. R. Ives, R. L. Jeanne, J. F. Kitchell, N. E. Langston, and M. G. Turner. *Ecology*. Oxford: Oxford University Press. pp. 315-383.

Begon, M., Harper, J. L., and C. R. Townsend. 1986. *Ecology: Individuals, Populations and Communities*. Oxford: Blackwell.

Collins, S. L., Glenn S. M., and D. J. Gibson. 1995. "Experimental analysis of intermediate disturbance and initial floristic composition: Decoupling cause and effect." *Ecology*. 76: 486-492.

Connell, J. H. 1978. "Diversity in Tropical Rain Forests and Coral Reefs." *Science*. 199: 1302-1310.

Death, R. G. and M. J. Winterbourn. 1995. "Diversity patterns in stream benthicinverterbrate communities: The influence of habitat stability." *Ecology*. 76: 1446-1460.

Huston, M. 1979. "A General Hypothesis of Species Diversity." *American Naturalist*. 113: 81-101.

Krebs, C. J. 2001. *Ecology: The Experimental Analysis of Distribution and Abundance*. (Fifth Edition) San Francisco: Addison-Wesley.

Lubchenco, J. 1978. "Plant species diversity in a marine intertidal community: importance of herbivore food preference and algal competitive abilities." *American Naturalist*. 112: 23-39.

Rosenzweig, M. L. 1999 "Species Diversity." From McGlade, J. (ed.) *Advanced Ecological Theory: Principles and Applications*. Oxford: Blackwell. pp. 249-281.

White, P. S. and J. Harrod. 1997. "Disturbance and Diversity in a Landscape Context." From Bissonette, J. A. (ed.) *Wildlife and Landscape Ecology: Effects of Pattern and Scale*. New York: Springer. pp. 128-159.

Bibliography

Ackermann, R. 1989. "The New Experimentalism." *British Journal for the Philosophy of Science*. 40: 185-90.

Allen, T. F. H. and T. W. Hoekstra. 1992. *Towards a Unified Ecology*. New York: Columbia University Press.

Armstrong, D. M. 1983. *What is a Law of Nature?* Cambridge: Cambridge University Press.

Ayrton, M. 1977. *Archilochos*. London: Secker & Warburg.

Bacon, F. 1960. *The New Organon*. Translated by Spedding, Ellis & Heath. Indianapolis & New York: Bobbs-Merrill Company.

Beatty, J. 1997. "Why Do Biologists Argue Like They Do?" *Philosophy of Science*. 64 (Proceedings): S432-S443.

Bell, R. P. 1969. *Acids and Bases: Their Quantitative Behaviour*. (Second Edition) London: Methuen and Co.

Bertalanffy, L. v. 1968. *General System Theory*. New York: George Brazillier.

Berlin, I. 1978. *The Hedgehog and the Fox: An essay on Tolstoy's view of history*. New York: Simon & Schuster.

Berlin, I. 1998. *The Proper Study of Mankind: An Anthology of Essays*. London: Pimlico.

Bhaskar, R. 1975. *A Realist Theory of Science*. (1997 Edition) London: Verso.

Bigelow, J. and R. Pargetter. 1990. *Science and Necessity*. Cambridge: Cambridge University Press.

Blackstone, W. T. 1973. "Ethics and Ecology." *Southern Journal of Philosophy*. 11: 55-71.

Boyce, M. S. 1992. "Population Viability Analysis." *Annual Review of Ecology and Systematics*. 23: 481-506.

Boyd, R. 1989. "What realism implies and what it does not." *Dialectica*. 43: 5-29.

Boyd, R. 1991. "Realism, Anti-Foundationalism and the Enthusiasm for Natural Kinds." *Philosophical Studies*. 61: 127-148.

Boyd, R. 1999. "Homeostasis, Species, and Higher Taxa." From Wilson, R. A. (ed.) 1999. *Species: New Interdisciplinary Essays*. pp. 141-185. Cambridge, Mass.: MIT Press.

Boyd, R., P. Gasper, and J. D. Trout. (eds.) 1991. *The Philosophy of Science*. Cambridge, Mass.: MIT Press.

Brandon, R. N. 1997. "Does Biology have Laws? The Experimental Evidence." *Philosophy of Science.* 64 (Proceedings): S444-S457

Brennan, A. A. 1986. "Ecological Theory and Value in Nature." *Philosophical Inquiry.* 8: 66-96. Reprinted in Elliot (ed.) 1995, pp. 188-214.

Brown, J. H. 1981. "Two decades of homage to Santa Rosalia: Towards a general theory of diversity." *American Zoologist.* 21: 877-88

Brown, J. H. 1995. *Macroecology.* Chicago: University of Chicago Press.

Budiansky, S. 1995. *Nature's Keepers: The New Science of Nature Management.* New York: The Free Press.

Burgman, M. and H. P. Possingham. 2000 "Population viability analysis for conservation: the good, the bad and the undescribed." In Young, A. G and G. M. Clarke (eds.) *Genetics, Demography and Viability of Fragmented Populations.* pp. 97-112. London: Cambridge University Press.

Callicott, J. B. 1982. "Hume's *Is/Ought* Dichotomy and the Relation of Ecology to Leopold's Land Ethic." *Environmental Ethics.* 4: 163-174.

Callicott, J. B. 1986. "The Metaphysical Implications of Ecology." *Environmental Ethics.* 8: 301-316.

Carnap, R. 1966. *Philosophical Foundations of Physics: An Introduction to the Philosophy of Science.* New York: Basic Books.

Carnap, R. 1995. *The Unity of Science.* (Reprint of 1934 Edition) Bristol: Thoemmes Press.

Cartwright, N. 1983. *How the Laws of Physics Lie.* Oxford: Clarendon Press.

Cartwright, N. 1989a. *Nature's Capacities and their Measurement.* Oxford: Clarendon Press.

Cartwright, N. 1989b. "Capacities and Abstractions." From Kitcher, P. and W. C. Salmon (eds.) *Scientific Explanation, Minnesota Studies in the Philosophy of Science, Volume 13.* pp. 349-356. Minnesota: University of Minnesota Press.

Cartwright, N. 1994. "Fundamentalism vs. The Patchwork of Laws." *Proceedings of the Aristotelian Society.* 94: 279-292. Reprinted in Cartwright 1999, pp. 23-34.

Cartwright, N. 1999. *The Dappled World.* Chicago: University of Chicago Press.

Chalmers, A. 1993. "So the Laws of Physics Needn't Lie." *Australasian Journal of Philosophy.* 71: 196-205.

Chalmers, A. 1996. "Cartwright on Fundamental Laws: A Response to Clarke." *Australasian Journal of Philosophy.* 74: 150-152.

Chalmers, A. 1999. *What is This Thing Called Science?* (Third Edition) St Lucia, QLD: University of Queensland Press.

Charles, D. and K. Lennon, (eds.) 1992. *Reduction, Explanation and Realism.* Oxford: Clarendon Press.

Clarke, S. 1995. "The Lies Remain the Same. A Reply to Chalmers." *Australasian Journal of Philosophy.* 17: 152-155.

Clarke, S. 1998. *Metaphysics and the Disunity of Scientific Knowledge.* Aldershot: Ashgate.

Collier, J. D. 1992. "Critical Notice: Paul Thompson, *The Structure of Biological Theories.*" *Canadian Journal of Philosophy.* 22 (2): 287-298.

Cracraft, J. 1983. "Species Concepts and Speciation Analysis." From R. Johnston (ed.) *Current Ornithology* 1983. 1: 159-187. New York: Plenum Press. Reprinted in Ereshefsky (ed.) 1992, pp. 93-120.

Claridge, M. F., H. A. Dawah and M. R. Wilson (eds.) 1997. *Species: The Units of Biodiversity.* London: Chapman and Hall.

Colwell, R. K., 1992. "Niche: A Bifurcation in the Conceptual Lineage of the Term." In Keller & Lloyd (eds.) 1992, pp. 241-248. Cambridge, Mass.: Harvard University Press.

Cooper, G. 1998. "Generalizations in Ecology: A Philosophical Taxonomy." *Biology and Philosophy.* 13: 555-586.

Davenport, G. (tr.) 1980. *Archilochos, Sappho, Alkman.* Los Angeles: University of California Press.

Dawkins, R. 1982. *The Extended Phenotype.* Oxford: Oxford University Press.

Dawkins, R. 1993. "Gaps in the Mind." From Cavalieri, P. and P. Singer (eds.) 1993. *The Great Ape Project.* pp. 80-87. London: Fourth Estate.

de Queiroz, K. 1992. "Phylogenetic Definitions and Taxonomic Philosophy." *Biology and Philosophy.* 7: 295-313.

de Queiroz, K. and J. Gauthier. 1992. "Phylogenetic Taxonomy." *Annual Review of Ecology and Systematics.* 23: 449-480.

Diamond, J. 1975. "The island dilemma: lessons of modern biogeographic studies for the design of natural reserves." *Biological Conservation.* 7:129-146.

Duhem, P. 1906. *The Aim and Structure of Physical Theory.* 1951 translation by Philip P. Wiener. Princeton: Princeton University Press.

Duhem, P. 1996. *Essays in the History and Philosophy of Science.* Translated and edited by Roger Ariew and Peter Barker. Indianapolis: Hackett Publishing Company.

Dupré, J. (ed.) 1987. *The Latest on the Best.* Cambridge, Mass.: MIT Press.

Dupré, J. 1993. *The Disorder of Things: Metaphysical Foundations of the Disunity of Science.* Cambridge, Mass.: Harvard University Press.

Dupré, J. 1996. "Metaphysical Disorder and Scientific Disunity." From Galison, P. and D. Stump (eds.) 1996. *The Disunity of Science: Boundaries, Contexts, and Power*. pp. 101-117. Stanford: Stanford University Press.

Ehrlich, P. R. 1987. "Ecology and Resource Management – Is Ecological Theory Any Good in Practice?" In Roughgarden, May and Levin (eds.) 1989. *Perspectives in Ecological Theory*. pp. 306-318. Princeton: Princeton University Press.

Elliot, R. (ed.) 1995. *Environmental Ethics*. Oxford: Oxford University Press.

Ereshefsky, M. (ed.) 1992. *The Units of Evolution: Essays on the Nature of Species*. Cambridge, Mass.: MIT Press.

Ereshefsky, M. 1992. "Eliminative Pluralism." *Philosophy of Science*. 59: 671-690.

Ereshefsky, M. 1998. "Species Pluralism and Anti-Realism." *Philosophy of Science*. 65: 103-120.

Ereshefsky, M. 1999. "Species and the Linnaean Hierarchy." In Wilson, R. A. (ed.) 1999. *Species: New Interdisciplinary Essays*. pp. 285-305. Cambridge, Mass.: MIT Press.

Feigl, H. and G. Maxwell. (eds.) 1962. *Scientific Explanation, Space, and Time. Minnesota Studies in Philosophy of Science, Volume III*. Minnesota: University of Minnesota Press.

Feyerabend, P. 1993. *Against Method*. (Third Edition) London: Verso.

Flannery, T. 1994. *The Future Eaters*. Melbourne: Reed Books.

Galison, P. 1987. *How Experiments End*. Chicago: University of Chicago Press.

Galison, P. 1996. "Computer Simulations and the Trading Zone." From Galison, P. and D. Stump (eds.) 1996. *The Disunity of Science: Boundaries, Contexts, and Power*. pp. 118-157. Stanford: Stanford University Press.

Galison, P. 1997. *Image and Logic*. Chicago: University of Chicago Press.

Galison, P. and D. Stump (eds.) 1996. *The Disunity of Science: Boundaries, Contexts, and Power*. Stanford: Stanford University Press.

Garfinkel, A. 1981. *Forms of Explanation*. New Haven: Yale University Press.

Gasper, P. 1991. "Causation and Explanation." In Boyd, Gasper, and Trout. (eds.) 1991. *The Philosophy of Science*. pp. 289-297. Cambridge, Mass.: MIT Press.

Ghiselin, M. T. 1974. "A Radical Solution to the Species Problem." *Systematic Zoology*. 23: 536-544. Reprinted in Ereshefsky (ed.) 1992, pp. 279-291.

Ghiselin, M. T. 1987. "Species Concepts, Individuality, and Objectivity." *Biology and Philosophy*. 2: 127-143.

Giere, R. N. 1999. *Science Without Laws*. Chicago: University of Chicago Press.

Goldingay, R. and Possingham, H. P. 1995. "Area requirements for viable populations of the Australian gliding marsupial, *Petaurus australis*." *Biological Conservation*. 73: 161-167.

Golley, F. 1987. "Deep Ecology From the Perspective of Environmental Science." *Environmental Ethics*. 9: 45-55.

Golley, F. 1993. *A history of the ecosystem concept in ecology: more than the sum of the parts*. New Haven: Yale University Press.

Goodman, N. 1983. *Fact, Fiction and Forecast*. (Fourth Edition) Cambridge, Mass.: Harvard University Press

Gotelli, N. J. and G. R. Graves. 1996. *Null Models in Ecology*. Washington: Smithsonian Institution Press.

Gould, S. J. 1970. "Dollo on Dollo's Law: Irreversibility and the Status of Evolutionary Laws." *Journal of the History of Biology*. 3: 189-212.

Grene, M. 1980. "A Note on Simberloff's 'Succession of Paradigms in Ecology'." *Synthese*. 43: 41-45.

Griesemer, J. R. 1992. "Niche: Historical Perspectives." In Keller & Lloyd (eds.) 1992, pp. 231-240. Cambridge, Mass.: Harvard University Press.

Griffiths, P. (ed.) 1992. *Trees of Life: Essays in the Philosophy of Biology*. Dordrecht: Kluwer.

Griffiths, P. 1996. "Darwinism, Process Structuralism, and Natural Kinds." *Philosophy of Science*. 63 (Proceedings): S1-S9.

Griffiths, P. 1997. *What Emotions Really Are*. Chicago: University of Chicago Press.

Griffiths, P. 1999. "Squaring the Circle: Natural Kinds with Historical Essences." In Wilson, R. A. (eds.) 1999. *Species: New Interdisciplinary Essays*. pp. 209-228. Cambridge, Mass.: MIT Press.

Hacking, I. 1983. *Representing and Intervening*. Cambridge: Cambridge University Press.

Hacking, I. 1991a. "A Tradition of Natural Kinds." *Philosophical Studies*. 61: 109-126.

Hacking, I. 1991b. "On Boyd." *Philosophical Studies*. 61: 149-154.

Hacking, I. 1995. *Rewriting the Soul: Multiple Personality and the Sciences of Memory*. Princeton: Princeton University Press.

Hacking, I. 1999. *The Social Construction of What?* Cambridge, Mass.: Harvard University Press.

Haila, Y. 1997. "Trivialization of Critique in Ecology." *Biology and Philosophy*. 12: 109-118.

Haraway, D. 1976. *Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth Century Developmental Biology*. New Haven: Yale University Press.

Hawking, S. 1988. *A Brief History of Time*. London: Bantam Books.

Hempel, C. G. 1948. "Studies in the Logic of Explanation." *Philosophy of Science*. 15: 135-75.

Hempel, C. G. 1962a. "Deductive-Nomological vs. Statistical Explanation." In Feigl, H. and G. Maxwell (eds.) *Scientific Explanation, Space, and Time, Minnesota Studies in Philosophy of Science, Volume III*. pp. 98-169. Minnesota: University of Minnesota Press.

Hempel, C. G. 1962b. "Explanation in Science and in History." In Colodny, R. G. (ed.) *Frontiers in Science and Philosophy*. pp. 7-33. Pittsburg: University of Pittsburg Press.

Hempel, C. G. 1965. *Aspects of Scientific Explanation*. New York: The Free Press.

Hempel, C. G. 1965. *Philosophy of Natural Science*. Englewood Cliffs, N. J.: Prentice-Hall.

Hennig, W. 1966. *Phylogenetic Systematics*. Urbana: University of Illinois Press.

Hoyningen-Huene, P. 1989. *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*. Chicago: Chicago University Press.

Huggett, R. J. 1995. *Geoecology: An Evolutionary Approach*. London: Routledge.

Huheey, J. E., E.A. Keiter, and R. L. Keiter. 1993. *Inorganic Chemistry: Principles of Structure and Reactivity*. (Fourth Edition) New York: HarperCollins College Publishers.

Hull, D. L. 1974. *The Philosophy of Biological Science*. Englewood Cliffs, N. J.:Prentice-Hall.

Hull, D. L. 1978. "A Matter of Individuality." *Philosophy of Science* 45: 335-360. Reprinted in Ereshefsky (ed.) 1992, pp. 293-316.

Hull, D. L. 1988. *Science as a Process*. Chicago: Chicago University Press.

Hull, D. L. 1999. "On the Plurality of Species: Questioning the Party Line." Wilson, R. A. (ed.) 1999. *Species: New Interdisciplinary Essays*. pp. 23-48. Cambridge, Mass.: MIT Press.

Hume, D. 1888. *A Treatise of Human Nature*. (1960 facsimile reprint, edited by L. A. Selby-Bigge.) Oxford: Oxford University Press.

Jackson, F. and P. Pettit. 1990. "Program Explanation: A General Perspective." *Analysis*. 50 (2): 107-117.

Jackson, F. and P. Pettit. 1992a. "Structural Explanation and Social Theory." In Charles D. and K. Lennon. (eds.) 1992. *Reduction, Explanation, and Realism*. pp. 97-131. Oxford: Clarendon.

Jackson, F. and P. Pettit. 1992b. "In Defence of Explanatory Ecumenism." *Economics and Philosophy*. 8: 1-21

Jaki, S. L. 1984. *Uneasy Genius: The Life and Work of Pierre Duhem*. The Hague: Martinus Nijhoff Publishers.

Kauffman, L. H. 1994. *Knots and Physics*. (Second Edition.) Singapore: World Scientific.

Kauffman, S. 1995. *At Home in the Universe*. Oxford: Oxford University Press.

Keller, E. F. and E. A. Lloyd, (eds.) 1992. *Keywords in Evolutionary Biology*. Cambridge, Mass.: Harvard University Press.

Kingsland, S. E. 1995. *Modelling Nature: Episodes in the History of Population Ecology*. (Second Edition) Chicago: University of Chicago Press.

Kitcher, P. 1981. "Explanatory Unification." *Philosophy of Science*. 48: 507-531.

Kitcher, P. 1984. "Species." *Philosophy of Science* 51: 308-333. Reprinted in Ereshefsky (ed.) 1992, pp. 317-341.

Kitcher, P. 1989. "Some Puzzles About Species." In Ruse, M. (ed.) 1989. *What the Philosophy of Biology Is*. pp.183-208. Dordrecht: Kluwer.

Kitcher, P. 1993. *The Advancement of Science*. Oxford: Oxford University Press.

Kitcher, P. and W. C. Salmon (eds.) 1989. *Scientific Explanation, Minnesota Studies in the Philosophy of Science, Volume 13*. Minnesota: University of Minnesota Press.

Kripke, S. 1972. "Naming and Necessity". In Davidson, D. and G. Harman (eds.) *Semantics of Natural Language*. Dordrecht: D. Reidl. Revised and enlarged edition, 1980. Oxford: Basil Blackwell Publisher.

Kuhn, T. 1970. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Kuhn, T. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.

La Follette, M. C. (ed.) 1983. *Creationism, Science and the Law: The Arkansas Case*. Cambridge, Mass.: MIT Press.

Lane, R. 1997. "The species concept in blood-sucking vectors of human diseases." In Claridge, M. F., H. A. Dawah and M. R. Wilson (eds.) 1997. *Species: The Units of Biodiversity*. pp. 273-289. London: Chapman and Hall.

Laudan, L. 1982. "Science at the Bar – Causes for Concern." *Science, Technology, & Human Values 7*. 41: 16-19. Reprinted in Ruse (ed.) 1988. pp. 351-355.

Laudan, L. 1990. "Normative naturalism." *Philosophy of Science*. 57: 44-59.

Leopold, A. 1949. *A Sand County Almanac*. Oxford: Oxford University Press.

Levins, R. and R. Lewontin. 1980. "Dialectics and Reductionism in Ecology." *Synthese*. 43: 47-78.

Levins, R. and R. Lewontin. 1985. *The Dialectical Biologist*. Cambridge, Mass.: Harvard University Press.

Lewontin, R. 1991. *Biology as Ideology: The Doctrine of DNA*. New York: HarperPerennial.

Lewis, D. K. 1973. *Counterfactuals*. Cambridge, Mass..: Harvard University Press.

Lewis, D. K. 1973. "Causal Explanation." *Journal of Philosophy*. 70: 556-567. Reprinted in *Philosophical Papers Volume II*. New York: Oxford University Press.

Lewis, D. K. 1999. *Papers in Metaphysics and Epistemology*. Cambridge: Cambridge University Press.

Lindenmayer, D. 1996. *Wildlife and Woodchips*. Sydney: UNSW Press.

Lindenmayer, D. and H. Possingham. 1994. *The Risk of Extinction*. Canberra: CRES.

Lindenmayer, D. and H. Possingham. 1995. "The conservation of arboreal marsupials in the montane ash forests of the central highlands of Victoria, South-Eastern Australia. VII. Modelling the persistence of Leadbeater's possum in response to modified timber harvesting practices." *Biological Conservation*. 73: 239-257.

Lindenmayer, D. and M. Taylor [online] 1995-1999 *The Leadbeater's Possum Page* http://incres.anu.edu.au/possum/possum.html

Lotka, A. J. 1925. *Elements of Physical Biology*. (Reprinted 1956.) New York: Dover.

Lovelock, J. 1979. *Gaia*. Oxford: Oxford University Press.

Lloyd, E. 1988. *The Structure and Confirmation of Evolutionary Theory*. New York: Greenwood Press.

MacArthur, R. and E. O. Wilson. 1967. *The Theory of Island Biogeography*. Princeton: Princeton University Press.

Manly, B. F. J. 1991. *Randomization and Monte Carlo Methods in Biology*. London: Chapman & Hall.

Marietta, D. E. 1979. "The Interrelationship of Ecological Science and Environmental Ethics." *Environmental Ethics*. 1: 195-207.

Mayden, R. L. 1997. "A hierarchy of species concepts: The denouement in the saga of the species problem." In Claridge, M. F., H. A. Dawah and M. R. Wilson (eds.) 1997. *Species: The Units of Biodiversity*. pp. 381-424. London: Chapman and Hall.

Maynard Smith, J. 1974. *Models in Ecology*. Cambridge: Cambridge University Press.

Mayo, D. and R. Hollander. 1991. *Acceptable Evidence: Science and Values in Risk Management*. New York/Oxford: Oxford University Press.

Mayo, D. 1996. *Error and the Growth of Experimental Knowledge*. Chicago: University of Chicago Press.

Mayr, E. 1942. *Systematics and the Origin of Species*. New York: Columbia University Press.

Mayr, E. 1948. "Sibling or Cryptic Species among Animals." In Mayr, E. 1976. *Evolution and the Diversity of Life*. pp. 509-514. Cambridge, Mass.: Harvard University Press.

Mayr, E. 1963. "Species Concepts and Their Application." From chapter 2 of *Populations, Species, and Evolution*. 1963. Cambridge, Mass.: Harvard University Press. Reprinted in Ereshefsky (ed.) 1992, pp. 15-25.

Mayr, E. 1976. *Evolution and the Diversity of Life*. Cambridge, Mass.: Harvard University Press.

Mayr, E. 1987. "The Ontological Status of Species: Scientific Progress and Philosophical Terminology." *Biology and Philosophy*. 2: 145-166.

Mayr, E. 1988. *Towards a New Philosophy of Biology*. Cambridge, Mass.: Harvard University Press.

McIntyre, L. 1997. "Gould on Laws in Biological Science." *Biology and Philosophy*. 12: 357-367.

McIntosh, R. P. 1980. "The Background and Some Current Problems of Theoretical Ecology." *Synthese*. 43: 195-255.

McIntosh, R. P. 1985. *The Background of Ecology: Concept and Theory*. Cambridge: Cambridge University Press.

McIntosh, R. P. 1987. "Pluralism in Ecology." *Annual Review of Ecology and Systematics*. 18: 321-341.

Mishler, B. and R. Brandon. 1987. "Individuality, Pluralism, and the Phylogenetic Species Concept." *Biology and Philosophy*. 2: 397-414.

Mishler, B. and M. Donoghue 1982. "Species Concepts: A Case for Pluralism." *Systematic Zoology*. 31: 491-503. Reprinted in Ereshefsky (ed.) 1992. pp. 121-138.

Moeller, T. 1982. *Inorganic Chemistry: A Modern Introduction*. New York: Wiley.

Morrison, R. T. and R. N. Boyd. 1983. *Organic Chemistry*. (Fourth Edition). Boston: Allyn and Bacon.

Nagle, E. 1961. *The Structure of Science*. London: Routledge.

Newton-Smith, W. H. (ed.) 2000. *A Companion to the Philosophy of Science*. Oxford: Blackwell.

Oppenheim, P. and H. Putnam. 1958. "Unity of Science as a Working Hypothesis." In Feigl, H., M. Scriven, and G. Maxwell (eds.) *Minnesota Studies in the Philosophy of Science. Volume II*. pp. 3-36. Minneapolis, MN: University of Minnesota Press.

Orians, G. H., J. Buckley, W. Clark, M. Gilpin, C. Jordan, J. Lehman, R. May, G. Robilliard, D. Simberloff, W. Erckmann, D. Policansky, and N. Grossblatt. 1986. *Ecological Knowledge and Environmental Problem Solving: Concepts and Case Studies*. Washington D.C.: National Academy Press.

Overton, W. R. 1982. "United States District Court Opinion: *McLean v. Arkansas.*" In Ruse M. (ed.), 1988. *But Is It Science?* Buffalo, NY: Prometheus Books

Peters, R. H. 1991. *A Critique For Ecology*. Cambridge: Cambridge University Press.

Pettit, P. 1993. *The Common Mind.* New York: Oxford University Press.

Pimm, S. L. 1991. *Balance of Nature*. Chicago: University of Chicago Press.

Plato. 1953. *The Dialogues of Plato.* (Fourth Edition) Translated by B. Jowlett. Oxford: Oxford University Press.

Plato. 1961. *The Collected Dialogues of Plato.* Hamilton, E. and H. Cairns (eds.) Princeton: Princeton University Press.

Popper, K. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.

Possingham, H. P., Lindenmayer, D. W., and T. W. Norton. 1993. "The role of PVA in endangered species management." *Pacific Conservation Biology.* 1: 39-45

Possingham, H. P., Lindenmayer, D. W., Norton, T. W and I. Davies. 1994. "Metapopulation viability analysis of the Greater Glider." *Biological Conservation.* 70: 227-236.

Possingham, H. P. and I. Davies. 1995. "ALEX: A model for the viability analysis of spatially structured populations." *Biological Conservation.* 73: 143-150.

Possingham, H. P., I. Davies and I. Noble. [online] Last updated 1999. *Introduction to ALEX: Analysis of the Likelihood of Extinction - a metapopulation modelling program.* http://biology.anu.edu.au/research-groups/ecosys/Alex/ALEX.HTM

Possingham, H. P., Ball, I. and S. Andelman. 2000. "Mathematical methods for identifying representative reserve networks." In Ferson, S. and Burgman, M. (eds.) *Quantitative methods for conservation biology*. pp. 291-305. New York: Springer-Verlag.

Possingham, H. P., Lindenmayer, D. W., and G. N. Tuck. 2001. *Decision Theory for Population Viability Analysis.* In Beissinger, S. R. and D. R. McCullough (eds.) 2001. *Population Viability Analysis*. Chicago: University of Chicago Press.

Putnam, H. 1962. "What Theories are Not." In Nagel, E., P. Suppes and A. Tarski (eds.) 1962. *Logic, Methodology and Philosophy of Science.* Stanford: Stanford University Press. Reprinted in Putnam, H. 1975. *Mathematics, Matter and Method. Philosophical Papers Volume 1.* pp. 215-227. Cambridge: Cambridge University Press.

Putnam, H. 1975. *Mind, Language and Reality. Philosophical Papers Volume 2.* Cambridge: Cambridge University Press.

Pyle, A. (ed.) 1999. *Key Philosophers in Conversation.* London: Routledge.

Quammen, D. 1996. *The Song of the Dodo*. New York: Scribner.

Quine, W. v. O. 1953. "On What There Is". In *From A Logical Point Of View*. pp. 1-19. New York: Harper & Row.

Quine, W. v. O. 1969. "Natural Kinds." In *Ontological Relativity and Other Essays*. pp. 114-138. New York: Columbia University Press.

Real, L. A. and J. H. Brown (eds.) 1991. *Foundations of Ecology: Classic Papers with Commentaries*. Chicago: University of Chicago Press.

Reed, E. S. 1992. "Knowers Talking About the Known: Ecological Realism as a Philosophy of Science." *Synthese* 92: 9-23.

Reisch, G.A. 1998. "Pluralism, Logical Empiricism, and the Problems of Pseudoscience." *Philosophy of Science*. 65: 333-348

Robertson, N., D. P. Sanders, P. D. Seymour and R. Thomas. 1996. "A new proof of the four colour theorem." *Electronic Research Announcements of the American Mathematical Society*. Vol. 2, No. 1: 17-25 [online]. Available from http://www.ams.org/journals/era/1996-02-01/

Rolston, H. III. 1975. "Is There an Ecological Ethic?" *Ethics*. 85/2: 93-109.

Rolston, H. III. 1985. "Duties to Endangered Species." *Bioscience*. 35: 718-726. Reprinted in Elliot (ed.) 1995, pp. 60-75.

Rolston, H. III. 1988. *Environmental Ethics: Duties to and Values in the Natural World.* Philadelphia: Temple University Press.

Rosenberg, A. 1985. *The Structure of Biological Science*. Cambridge: Cambridge University Press.

Rosenberg, A. 1994. *Instrumental Biology or the Disunity of Science*. Chicago: Chicago University Press.

Ruse M. 1973. *The Philosophy of Biology*. London: Hutchinson & Co.

Ruse M. (ed.) 1988. *But Is It Science?* Buffalo, NY: Prometheus Books

Ruse, M. (ed.) 1989. *What the Philosophy of Biology Is.* Dordrecht: Kluwer.

Saarinen, E. (ed.) 1982. *Conceptual Issues in Ecology*. Dordrecht: D. Reidel

Sagoff, M. 1985. "Fact and Value in Ecological Science." *Environmental Ethics*. 7: 99-116

Sagoff, M. 1988a. *The Economy of the Earth: Philosophy, Law and the Environment*. Cambridge: Cambridge University Press.

Sagoff, M. 1988b. "Ethics, Ecology, and the Environment: Integrating Science and the Law." *Tennessee Law Review*. 56: 77-229.

Schaffner, K. 1993. *Discovery and Explanation in Biology and Medicine*. Chicago: University of Chicago Press.

Schilpp, P.A. (ed.) 1974. *The Philosophy of Karl Popper.* La Salle: Open Court Press.

Schutz, B. 1985. *A First Course in General Relativity.* Cambridge: Cambridge University Press.

Shepard, P. and D. McKinley (eds.) 1969. *The Subversive Science: Essays Toward an Ecology of Man.* Boston: Houghton Mifflin.

Shrader-Frechette, K. S. 1989a. "Scientific Method and the Objectivity of Epistemic Value Judgements." In Fenstad, J., Hilpenen, R. and I. Prolov (eds.) 1989. *Logic, Methodology and the Philosophy of Science.* pp. 373-389. New York: Elsevier Science Publishers.

Shrader-Frechette, K. S. 1989b. "Idealized Laws, Anti Realism, and Applied Science: A Case in Hydrogeology." *Synthese.* 81: 329-352.

Shrader-Frechette, K. S. 1995. "Practical Ecology and Foundations for Environmental Ethics." *The Journal of Philosophy.* 12: 621-635

Shrader-Frechette, K. S. and E. D. McCoy. 1990. "Theory Reduction and Explanation in Ecology." *Oikos.* 58: 109-114

Shrader-Frechette, K. S. and E. D. McCoy. 1993. *Method in Ecology: Strategies for Conservation.* Cambridge: Cambridge University Press.

Shrader-Frechette, K. S. and E. D. McCoy. 1994a. "How the Tail Wags the Dog: How Value Judgements Determine Ecological Science." *Environmental Values.* 3: 107-120.

Shrader-Frechette, K. S. and E. D. McCoy. 1994b. "Applied Ecology and the Logic of Case Studies." *Philosophy of Science.* 61: 228-249.

Shafer, C. 1990. *Nature Reserves: Island Theory and Conservation Practice.* Washington D.C. : Smithsonian Institution Press.

Shaffer, M. L. 1990. "Population Viability Analysis." *Conservation Biology.* 4: 39-40.

Simberloff, D. S. and E. O. Wilson. 1969. "Experimental Zoogeography of Islands: The Colonization of Empty Islands." *Ecology.* 50: 278-296. Reprinted in Real & Brown (ed.) 1991, pp. 861-879.

Simberloff, D. S. and L. G. Abele. 1976. "Island Biogeography Theory and Conservation Practice." *Science.* 191: 285-286.

Simberloff, D. 1980. "A Succession of Paradigms in Ecology: Essentialism to Materialism and Probabilism." *Synthese.* 43: 3-39.

Simberloff, D. 1980. "Reply." *Synthese.* 43: 79-93.

Simberloff, D. 1988. "The Contribution of Population and Community Biology to Conservation Science." *Annual Review of Ecology and Systematics.* 19: 473-511.

Sismondo, S. 2000. "Island Biogeography and the Multiple Domain of Models." *Biology and Philosophy.* 15: 239-258.

Smart, J. J. C. 1963. *Philosophy and Scientific Realism.* London: Routledge and Kegan Paul.

Smart, J. J. C. 1968. *Between Science and Philosophy.* New York: Random House.

Sober, E. 1990. "The Poverty of Pluralism: A Reply to Sterelny and Kitcher." *Journal of Philosophy.* pp. 151-158.

Sober, E. 1993. *The Philosophy of Biology.* Oxford: Oxford University Press.

Sober, E. 1997 "Two Outbreaks of Lawlessness in Recent Philosophy of Biology." *Philosophy of Science.* 64 (Proceedings): S458-S467.

Sobol', I. M. 1994. *A Primer for the Monte Carlo Method.* Boca Raton: CRC Press.

Sokal R. and T. Crovello. 1970. "The Biological Species Concept: A Critical Evaluation." *American Naturalist.* 104: 127-153. Reprinted in Ereshefsky (ed.) 1992, pp. 27-55.

Sokal, R. and P. Sneath. 1963. *Principles of Numerical Taxonomy.* San Francisco: Freeman.

Soulé M. and B. A. Wilcox (ed.) 1980. *Conservation Biology: An Evolutionary-Ecological Perspective.* Sunderland: Sinauer Associates.

Soulé, M. (ed.) 1986. *Conservation Biology: The Science of Scarcity and Diversity.* Sunderland: Sinauer Associates.

Soulé, M. (ed.) 1987. *Viable Populations for Conservation.* Cambridge: Cambridge University Press.

Southgate, R. and H. P. Possingham. 1995. "Population viability analysis of the Greater Bilby, lagotis macrotis." *Biological Conservation.* 73: 151-160.

Stanford, P.K. 1995. "For Pluralism and Against Realism about Species." *Philosophy of Science* 62: 70-91.

Sterelny, K. 1996. "Explanatory Pluralism in Evolutionary Biology." *Biology and Philosophy.* 11: 193-214.

Sterelny, K. and P. Griffiths. 1999. *Sex and Death.* Chicago: Chicago University Press.

Suppe, F. (ed.) 1977. *The Structure of Scientific Theories.* Illinois: University of Illinois Press.

Suppe, F. 1989. *The Semantic Conception of Theories and Scientific Realism.* Urbana/Chicago: University of Illinois Press.

Suppes, P. 1993. *Models and methods in the Philosophy of Science: Selected Essays.* Dordrecht: Kluwer.

Taylor, P. W. 1986. *Respect For Nature: a theory of environmental ethics.* Princeton: Princeton University Press.

Templeton, A. 1989. "The Meaning of Species and Speciation: A Genetic Perspective." From Otte and Endler (eds.) *Speciation and its Consequences.* 1989. pp. 3-27. Sunderland: Sinauer Associates. Reprinted in Ereshefsky (ed.) 1992, pp. 159-183.

Thompson, P. 1987. "A defence of the semantic conception of evolutionary theory." *Biology and Philosophy.* 2: 26-32.

Thompson, P. 1989. *The Structure of Biological Theories.* Albany: State University of New York Press.

't Hooft, G. 1997. *In Search of the Ultimate Building Blocks.* Cambridge: Cambridge University Press.

van Der Steen, W. and H. Kamminga. 1991. "Laws and Natural History in Biology." *British Journal for the Philosophy of Science.* 42: 445-467.

van Fraassen, B. 1977. "The Pragmatics of Explanation" *American Philosophical Quarterly.* 14: 143-150. Reprinted in Boyd, Gasper & Trout. (ed.) 1991, pp. 317-327.

van Fraassen, B. 1980. *The Scientific Image.* Oxford: Clarendon Press.

van Fraassen, B. 1989. *Laws and Symmetries.* New York: Oxford University Press.

Van Valen, L. 1976. "Ecological Species, multispecies and oaks." *Taxon.* 25: 233-239. Reprinted in Ereshefsky (ed.) 1992, pp. 69-77.

Volterra, V. 1926. "Fluctuations in the abundance of a species considered mathematically." *Nature.* 118: 558-560. Reprinted in Real and Brown (eds.) 1991, pp. 283-5.

Waters, C. K. 1998. "Causal Regularities in the Biological World of Contingent Distributions." *Biology and Philosophy.* 13: 5-36.

Weinberg, S. 1992. *Dreams of a Final Theory.* New York: Pantheon Books.

Weinert, F. 1999. "Theories, Models and Constraints." *Studies in History and Philosophy of Science.* 30/2: 303-333.

Wilcox, B. A. 1980. "Insular Ecology and Conservation." In Soulé M. and B. A. Wilcox (eds.) 1980. *Conservation Biology: An Evolutionary-Ecological Perspective.* pp. 95-117. Sunderland: Sinauer Associates.

Wilkerson, T. E. 1998. "Natural Kinds" *Philosophical Books.* 39 (No. 4): 225-233.

Williams, M. B. 1970. "Deducing the consequences of evolution." *Journal of Theoretical Biology.* 29: 343-85.

Wilson, E. O. (ed.) 1988. *Biodiversity.* Washington D. C.: National Academy Press.

Wilson, E. O. 1975. *Sociobiology: The New Synthesis.* Cambridge, Mass.: Harvard University Press.

Wilson, E. O. 1992. *The Diversity of Life.* London: Penguin Books.

Wilson, R.A. 1996. "Promiscuous Realism." *British Journal for the Philosophy of Science.* 47: 303-316.

Wilson, R. A. (ed.) 1999. *Species: New Interdisciplinary Essays.* Cambridge, Mass.: MIT Press.

Yin, R. K. 1994. *Case Study Research: Design and Methods.* (Second Edition) Thousand Oaks, California: Sage.