

**ASPECTS OF SCIENTIFIC METHODOLOGY WITH
SPECIAL REFERENCE TO EVOLUTIONARY BIOLOGY**

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ABSTRACT

A critical examination of Popper's falsificationism as a methodological criterion of demarcation led to the development of a supplementary means of distinguishing science from pseudo-science. The discipline is made the unit of appraisal and its pattern of historical development is used as the indicator of demarcation. Results of a test of this indicator against astrology and physical optics accord with our basic judgments of these disciplines. The indicator effectively reveals that scientific creationism is pseudo-science, and that evolutionary biology is genuine science.

Three fundamental approaches to scientific investigation, viz. verificationism, falsificationism and multi-cornered testing (MCT) are contrasted. MCT is distinguished by competition between hypotheses, which makes it more informative than at least the naive versions of the other two approaches. While competition does not produce immediate victors, it does make demands on theories, which can be augmented by prescribing a series of independent tests. The comparative method implies the existence of two types of evidence. Common evidence is that which is predicted or explained by two or more rival hypotheses. Discriminatory evidence favours one rival over the others.

It is argued that in both the fields of species biology and speciation there have been instances of over-relying on common evidence, of indistinctly defining alternative hypotheses, of not following their logical consequences and of not using existing discriminatory evidence to adjudicate between these hypotheses. Species concepts and definitions of modes of speciation are evaluated. Normative principles are suggested for defining species and other important terms in evolutionary biology, and for testing species concepts and modes of speciation. The advantages and limitations of a historical indicator of

demarcation and the merits and principles of the comparative approach to method are discussed and illustrated using the analogy of a mathematical game.

Scientific creationism is shown to have a coating of scientific method, but to have systematically violated fundamental methodological principles. Darwin's method in contrast, had a comparative structure, and distinguished between common and discriminatory evidence. While there are methodological problems in evolutionary biology, these are shown to be minor in comparison to that found in scientific creationism.

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DECLARATION

I declare that this thesis is my own, unaided work. It is being submitted for the degree of Doctor of Philosophy at the university of the Witwatersrand, Johannesburg. It has not been submitted before for any degree or examination at any other university.

M. L. Anderson

Signed on this 17 day of NOVEMBER 1988.

Table of Contents

Abstract	ii
Declaration	iv
Acknowledgements	v
Table of Contents	vi
List of Figures	xi
List of Tables	xiii
Chapter 1: Introduction	1
Chapter 2: Demarcation, Evolutionary Biology and Creationism	9
The problem of demarcation.....	9
Popper's route to falsifiability	9
Beginning with basic judgments.	11

Comparing obvious sciences with obvious pseudo- sciences	11
Falsificationism: characterizing science by its method	13
Towards an indicator of demarcation	20
Bypassing the difficulties with falsificationism	20
The historical development of physical optics: does it display exclusionary convergence?	27
The historical development of astrology: does it display stasis in theoretical diversity?	29
Is evolutionary biology a science?	31
Is creationism a science?	34
Chapter 3: Multi-cornered testing: the method of theory comparison.	41
Towards a criterion for evaluating method	43
The problem of additional unforeseen explanations	47
The problem of ad hocery	48
The problem of universality	51
The problem of complementarity	53
The problem of the theory dependence of observation	53

The problem of the status of basic statements	57
Applying MCT	59
Chapter 4: Methodology and the species problem	61
The major species concepts	61
Step 1: Recognizing common evidence.....	62
Step 2: Distinctly defining the alternatives.....	65
Normative principles in defining	66
Two ways of defining	66
From classes to individuals.....	69
Species concepts and definitions: an evaluation.....	70
Universality and the species problem	73
Step 3: Developing multi-cornered predictions.....	76
The temporal aspect of the speciation process.....	78
The Recognition Concept	78
The Isolation Concept.....	79
The Morphological Concept	81

Step 4: Adjudicating between alternatives.....	83
Chapter 5: Methodology and speciation	87
Background to speciation	88
Step 1: Recognising common evidence.....	90
Step 2: Distinctly defining the alternatives.....	97
Variations in the meaning of 'sympatric' and 'allopatric'	98
Consequences for testing.....	99
An evaluation of definitions	101
Step 3: Developing multi-cornered deductions	103
Normative principles in testing modes of speciation.....	105
Applying normative principles.....	106
Step 4: Adjudicating between alternatives.....	118
Chapter 6: Demarcation and method in a simulation game.....	121
How to play Target	122
Similarities between Target and science	124
Target and the problem of demarcation	124

Discussion: role of target in demarcation	128
'Target and 'level of agreement' as an indicator of demarcation	129
Target and methodology	131
Evaluating methodological criteria	131
Illustrating the interdependence between observation and theory	137
Illuminating distinctions using 'Target'.....	140
Chapter 7: Conclusions	142
The problem of demarcation.....	142
The problem of method	149
A priori considerations	149
The advantages of the comparative approach.....	151
A Posteriori considerations	153
Darwin's method.....	153
Scientific creationist's method.....	158
Method in evolutionary biology	168
References	175

LIST OF FIGURES

Figure 2-1. The relationship between the connection and methodological theses.	15
Figure 2-2. Negative feedback in science.	24
Figure 3-1. The structure of multi-cornered testing.	50
Figure 3-2. The relationship between observation, theory and the real world.	56
Figure 6-1. A few steps in Target.....	123
Figure 6-2. Negative feedback in Target.	125
Figure 6-3. The relationship between calls, responses and the target number.....	139
Figure 7-1. The comparative structure to Darwin's argument.....	157

Figure 7-2. Concepts of evidence.....159

Figure 7-3. A graphic representation of the fossil evidence for
evolutionary theory and scientific creationism.....166

LIST OF TABLES

Table 2-1. Expected patterns in the history of genuine sciences and pseudo-sciences.....	25
Table 4-1. Species concepts in the form of conditionals.....	74
Table 4-2. Some predictions of the Isolation and Recognition concepts of species.....	77
Table 5-1. The deductions in Lewis' (1961) argument.....	108
Table 5-2. The predictions of Lambert et al (1984).....	113
Table 5-3. Williams's (1973) deductions.....	115
Table 5-4. Multi-cornered predictions between allopatric and sympatric speciation.....	117
Table 6-1. Expected patterns in the history of "science" and "pseudo-science" in Target	127
Table 7-1. Some "predictions" from evolution and scientific creationism.....	146

CHAPTER 1: INTRODUCTION

Evolutionary theory has been called the supreme integrative principle of biology. In the face of its importance, it is disturbing that its credentials have recently been disputed even by respected authors. Popper, who has since recanted (Popper 1980), claimed that Darwinism was a metaphysical rather than a scientific research programme (Popper 1976:172). Others have claimed that that evolutionary theory does not make genuine predictions (Scriven 1959, Popper 1976). Some have argued that the theory of natural selection is tautologous (e.g. Macbeth 1971:65) and that much of evolutionary theory is based on circular argument (Peters 1976). Kerkut (1960:7) claimed that the General Theory of Evolution consists of a set of unverifiable assumptions. Denton (1985:77) says that Darwin's General Theory is a "highly speculative hypothesis". Many biologists have accepted the criticisms says Ruse (1981), to the point that the British Museum of Natural History decided, for a time, to reword their displays so as not to create the impression that evolution was a fact.

Since then it has become apparent that the original criticisms of evolutionary theory far from spell the demise of evolutionary theory. Some have been based on a misunderstanding of evolution (Ruse 1977, Gould 1983, von Schilcher and Tennant 1984:91-105, Rosenberg 1985:216-219), of scientific method (Caplan 1977) or of both (Kitcher 1982, Williams 1973, 1982, 1985, Futuyma 1982). The tautology objection has, I think, been adequately answered (Gould 1977:40-48, 1983, Lewontin 1978, Thompson 1981:51-73). That the scientific status of such a well established theory could even be questioned

suggests the need for a continued look at the problem of demarcation- that is, the problem of distinguishing between science and pseudo-science.

This need is further suggested by the fact that although most critics agree that the so called 'creation science' or 'scientific creationism' is pseudo-science (Feyerabend (1981:145) is an exception), they do not always agree as to why. Scientific creationists uphold a particular literalistic interpretation of the biblical account of origins, which they present as a "scientific model." According to this view the universe came into being in six days of twenty-four hours between six and ten thousand years ago. Much valuable work has been done in appraising creation science (e.g. Kitcher 1982, Futuyma 1982, Root-Bernstein 1983, Godfrey 1983). However, conflicting claims are sometimes made against it. Judge Overton (1984:380) ruled that the position was unfalsifiable and so a pseudo-science. Laudan (1982:150), a philosopher of science, asserts that creation-science far from being unfalsifiable, is actually false. Dolby (1987) says that it is a science, but an archaic and corrupting sort.

Advocates of creation science are themselves unsettled over its credentials. Thus we have them claiming (Bird 1979) that "special creation can be presented from a strictly scientific standpoint...", and yet because it deals with past events it is not really a scientific theory but a "model".

Kitcher (1982:124-126) distinguishes between three types of creationism. Let us call the first type religious creationism. Here no attempt is made to justify belief in a creation on scientific grounds. A second type which will be called simply creationism does seek after justification on scientific grounds. The claim here that there is no purely naturalistic account of origins which is scientifically acceptable. The third type is scientific creationism or creation science. Scientific creationism must be distinguished because it is only one variant of creationism. My concern will not be with religious creationism at all. The object of chapter 2

will be to determine the scientific status of evolutionary biology and creationism.

To achieve this a prior question must be answered: What is it about science that makes it scientific? This is known as the problem of demarcation. Lakatos (1978), Losee (1980:178-193), Laudan (1983) and Dolby (1987) review proposed solutions to the problem. The search has been for some criteria that are jointly necessary and sufficient for a system to be classed as a science.

Despite its long history, we do not seem very close to a solution. Many suggestions have been made. Aristotle thought that genuine scientific knowledge had the status of necessary truth (Losee 1980:14). Bridgeman's early operationism claimed that genuinely scientific concepts must be connected to measuring procedures (Losee 1980:178-182). The criterion of the Logical Positivists was verifiability: genuinely scientific statements must be empirically verifiable (Oldroyd 1986:232). The conventionalist approach was elitism: it was for the scientific establishment to say what was genuinely scientific or not (Lakatos 1978). For Popper (1972a:40-41) the criterion was falsifiability: a scientific or empirical system of statements is one in which its logical form is such that it is possible to refute the system by experience. Each proposal has its problems, to the point that many philosophers of science believe that, at the moment, there exists no decisive, universal characterization of science (Lakatos 1978:107-120, Laudan 1983).

Then there is the question of how to characterize the problem of demarcation. Bartley (1968:44-49) says that Popper did not separate out metaphysics and pseudo-science in his quest to solve the problem. Many philosophers of science feel that the problem of demarcating between science and metaphysics is not a very important one. Laudan (1983) says that the important question is whether or not an enterprise is legitimate.

We will not be concerned with non-science at all, but with science and pseudo-science. Furthermore, the first object will be to distinguish merely between science and pseudo-science rather than to demarcate between these in the traditional sense. There will be no attempt, then, to decisively characterize these in terms of method. This is not to say that method is unimportant. In later chapters we will be concerned with method, but under a different heading. The object there will be with discerning the difference between good and bad method. Why this course is taken will become clear later. We will see that the great diversity in science makes it difficult to demarcate on methodological grounds.

Many authors have argued, rather convincingly I think, that evolutionary biology is a genuine and successful science (Ruse 1982, Futuyma 1982) and that creation science is pseudo-science (Kitcher 1982, Root-Bernstein 1983) or at least very bad science (Dolby 1987). Ruse (1982) and Futuyma (1982) argue that evolutionary biology is good science for several reasons. Not only is it testable, it has come through the tests very successfully. It explains much and is very fruitful, having spawned new and productive lines of research. In contrast, creation science fails very badly, and also for several reasons. Some parts of creation science are false (Gould 1983). It claims, for instance, that barring extinction, all life forms were present throughout the earth's history. The fossil record contradicts this position of course. Other parts are unfalsifiable. There is the claim that the stars were created with their light rays so as to give the universe an apparent age (Morris 1976:65,66). It has been repeatedly pointed out that claims like these are extremely problematic from a scientific perspective, whether we regard them as strictly unfalsifiable or as explanatorily thin fairy tales (Smith and Ward 1984).

The sciences and pseudo-sciences are what they are for many reasons. This makes it difficult to find a common denominator for each which distinguishes them. One objective of chapter 2 is to show that a common denominator can be found, if we look at something other than method. I suggest that we look at the

effects these methods have on the history of a discipline. In doing this we shall discover a way of telling genuine science and pseudo-science apart without having to give a characterization of their methods. In other words the aim is to develop an indicator rather than a criterion of demarcation; to develop a kind of litmus paper which can be dipped into a discipline to test its scientific credentials. We shall find that this indicator has limitations but also some special advantages.

This indicator is the behaviour of a discipline over time. It will be argued that a genuine science is characteristic in showing considerable reduction in theoretical diversity over time, whereas in a pseudo-science the degree of theoretical diversity remains relatively constant.

This indicator will be applied to physical optics, as an example of an obvious science, and astrology as an obvious pseudo-science to check its adequacy. The scientific credentials of evolutionary biology and creation science will be tested in terms of this indicator. The conclusion I will reach is that the former is a genuine science, and the latter a pseudo-science.

Once evolutionary biology is identified as a genuine science, a second question emerges and that concerns method. Do evolutionary biologists practice good method? Criticism of the methodology of evolutionary biologists has come from other workers in the field (e.g. Futuyma and Mayer 1980, Paterson 1981) and even from respected authors outside the field (e.g. Macbeth 1971). Paterson (1981) says that there is a need "for a more consistent logic if we are to reduce dispute and disagreement and improve understanding in evolutionary biology." Much of the disagreement is not over what the evidence is, but over how to interpret it. Thus what some authors (e.g. Tauber and Tauber 1977a) take as evidence for sympatric speciation another (Hendrickson 1978) finds inconclusive. What one author (e.g. Mayr 1963:17) takes as evidence for the Biological Concept of Species, another finds unsatisfactory (Paterson 1985).

The second main aim of the thesis is to determine whether evolutionary biologists have practiced good method. Before this can be done an adequate criterion for evaluating method is needed. Much has been said on this subject by philosophers of science. The goal of chapter 3 is to arrive at a very basic criterion for evaluating method, and one which can be applied readily to actual research approaches.

For the sake of simplicity, discussion of method will take place within a purely deductive framework, although the points made probably apply more widely. My main object is to show that there are three distinct procedures within such a framework. These are verificationism, in which a hypothesis is supported by seeking to confirm it; falsificationism, in which a hypothesis is tested by trying to refute it; and what I will call multi-cornered testing (MCT), in which rival hypotheses are forced to confront each other. More sophisticated versions of verificationism and falsification involve theory comparison. The contrast made here will be between MCT and what are known as the naive versions.

Now it might be questioned whether all this needs to be said. Naive verificationism and naive falsificationism have been discredited (Popper 1972b:35-37, Lakatos 1970:93-116). Many philosophers of science assume that a significant part of scientific method consists in choice of the best theories, and have focused on how to explicate formally theory comparison (Andersson 1978, Radnitsky and Andersson 1978, Glymour 1983, van Fraassen 1983).

The approach taken here is justified because, as we shall see, some of the problems in biological research are the result of a confounding of the three basic methods. It will be argued that this has led to the practice of supporting conflicting hypotheses with the same evidence. I will show that the distinctiveness of the three has been blurred because of a preoccupation with logical validity as the basic criterion for evaluating method. I suggest that a more appropriate criterion in science is how much an argument tells us. On the basis of

this criterion I show that MCT is not only distinct, but is the better of the three. The concept is an old one. One purpose of chapter 3 is to rid the concept of notions discredited by contemporary philosophers of science, to emphasize the distinctiveness and superiority of this procedure against the alternatives, and to express the concept in a way that readily communicates to a biologist.

In the next two chapters, practices within selected fields in evolutionary biology are evaluated according to the criteria established in chapter 3. Chapter 4 deals with the species problem and chapter 5 with speciation. The main finding is that rival hypotheses are often supported by an inadequate type of evidence; a type of evidence which can be interpreted to favour two or more of the rivals. It is submitted that the way forward is to test hypotheses comparatively. Evidence often exists that discriminates between hypotheses without its significance being fully appreciated. For the comparative approach to work it must be recognised that there are different types of evidence, alternative hypotheses must be distinctly defined, and the logical consequences of these rigorously followed.

In chapter 6 a mathematical game is described which is useful in illustrating principles of demarcation and of method. The points of similarity between the analogy and science are discussed. The game can be used to simulate both science and pseudo-science. In so doing it suggests interesting features about each.

In the final chapter I discuss the limitations of an indicator of demarcation showing that these are offset by its special advantages. Next, multi-cornered testing is evaluated according to two sets of criteria. Firstly, whether it can be given an *a priori* justification i.e. whether it can be justified in principle; secondly, whether it can be given an *a posteriori* justification i.e. whether there are any cases where the method has been practiced and has lead to successful research. Finally, some concluding remarks are made about the methods used in evolutionary biology. The great success of the discipline shows

that its methods are on the whole good. However, there are places where less than the best in method is used. I show why this has happened and suggest solutions. It is argued that the way forward is to seek not just for evidence that will support a theory, but for evidence that will favour the better theory over its rivals.

CHAPTER 2: DEMARCATION, EVOLUTIONARY BIOLOGY AND CREATIONISM

The problem of demarcation

Popper's notion of falsifiability is the most universally accepted criterion of demarcation, at least amongst scientists. Biologists have often criticized rival theories on the grounds that they are unfalsifiable (Kitcher 1985:58-61). We have already seen (p.1) that Popper (1974:172) used the criterion to classify Darwinism as a metaphysical research programme rather than a scientific one, but has since recanted (1980). A notion with such potentially far-reaching consequences must be carefully examined. The object of this first section is to describe Popper's criterion and his route to it. Next, I will discuss the criticisms that can, and have, been levelled at his proposals with the purpose of discovering their merits and limitations and what it is about the demarcation problem that make it so difficult. Possible solutions to the problem will arise from this discussion.

Popper's Route To 'Falsifiability'

The central problem of interest to Popper was how to distinguish between science and pseudo-science (Popper 1972b:33). His quest began with certain basic judgments: Einstein's Relativity and Newtonian Mechanics are examples of what is obviously science while Marx's theory of history, Freud's psycho-analysis and Adler's theory of individual psychology are suspect. Becoming increasingly dissatisfied with the latter three theories, Popper wondered what these had in common which distinguished them from the former

two theories. He concluded that it was their apparent explanatory power. "The world was full of verifications" of these theories. In contrast, he noted that relativity theory could be refuted by the failure of occurrence of a single prediction. It was subject to great risk. He concluded that it must be possible for an empirical scientific system to be refuted by experience. Tautological claims would be excluded from science since these are necessarily true and could never be falsified.

Popper immediately saw that this requirement, which concerns the logical structure of statements in a theory, is necessary but not sufficient for the theory to count as scientific. The reason is that it would fail to exclude those pseudo-sciences which tenaciously hang on to falsifiable but false claims. He therefore gave the additional requirement that science should be characterized by its method; by its theories being boldly subjected to test: "It is the boldness of a conjecture which takes a real risk - the risk of being tested, and refuted, the risk of clashing with reality... thus my proposal was, and is, that it is this... boldness, together with the readiness to look out for tests and refutations, which distinguishes 'empirical' science from non-science, and especially from pre-scientific myths and metaphysics" (Popper 1976:9,81). His proposal has at least three fundamental components:

- a) He begins with basic judgments, deriving his criterion from these.
- b) He compares obvious sciences to suspect ones.
- c) His falsificationism criterion is a methodological one.

We will see that each of these components has serious limitations.

Beginning with basic judgments.

Popper did not only rely on basic judgments in developing his falsifiability criterion. His account also depends on an asymmetry between verification and falsification. He noticed that universal statements could never be verified, but could be falsified by a single contrary instance. Nevertheless, Popper does admit that, historically, he began with basic judgments. The problem with this approach is that it is somewhat circular. One decides beforehand what is science and pseudo-science, and then derives a criterion which is used to distinguish science from pseudo-science. Popper's approach is puzzling because he is a demarcationist and as Lakatos points out "... for demarcationists, a theory may be pseudo-scientific even though it is eminently 'plausible' and everybody believes in it and it may be scientifically valuable even if it is unbelievable and nobody believes in it" (Lakatos 1980:109). Freud's psycho-analysis may be scientific even though Popper doesn't think it is. If an astrologer was asked to devise a criterion for demarcation he might begin with the basic judgment that astrology is the science of all sciences and exclude relativity!

There are serious limitations, then, in deriving a criterion of demarcation from basic judgments since these may be wrong. This does not mean that they are valueless. They are useful in checking whether a criterion, already supported on other grounds, correctly distinguishes the obvious sciences from pseudo-sciences. However, even if we grant that Popper was right in his basic judgments, there are still difficulties.

Comparing obvious sciences with obvious pseudo- sciences

If we must begin with basic judgments it is important to make the right comparison. The comparison that Popper made is risky because the possible diversity in science is likely to be ignored. It is unsafe to regard Einstein's Relativity and Newtonian Mechanics as representative of all of science. It is

unsafe because there may be other genuinely scientific disciplines which are not testable in exactly the same way as the hard sciences. Ghiselin (1969:30-31) for example has shown that Darwin's geological theories were testable as a whole, even though certain subsidiary statements were not. An innocuous unfalsifiability of this sort is also found in Meteorology and Geomorphology (Lagerspetz 1969).

The importance of appreciating diversity in science can be illustrated with the help of some taxonomy. Imagine a taxonomist setting out to discover the distinctive feature of the class mammalia. He chooses the orang-outan as the epitome of what is mammal and a lizard as an obvious non-mammal. He notices that the orang-outan does not lay eggs whereas lizards do. He concludes that the hallmark of mammalia is viviparity. He would be wrong of course. The duck-billed platypus is a mammal and yet lays eggs. The problem with our imaginary taxonomist's approach is that the diversity in the class mammalia is not appreciated. To arrive at an adequate distinguishing feature all recognized members must be considered. This was not Popper's method. Only the pride of the physical sciences was examined. A common denominator was not sought between all fields reckoned to be sciences.

The consequence of our imaginary taxonomist's approach is a criterion that is too narrow, requiring the rejection of genuine members of mammalia. The strict version of Popper's criterion has the same result. Strict falsifiability (i.e. that every statement in a theory must be directly testable) is not characteristic of all fields reckoned to be sciences. The narrow criterion would force us to exclude meteorology and geomorphology. Popper (1974:981) has worried over this problem, and has accordingly adjusted his original position. As a suggested improvement to his criterion, a theory should be regarded as scientific to the degree to which it is testable. This would place meteorology a little lower than physics on the scientific scale since meteorological theories are notoriously difficult to test. The problem is that the causal connections in meteorological

systems are extremely complex; one has to take into account a large number of relevant conditions. In spite of this the enterprise is apparently legitimate. If so, then Popper's criterion might help in ranking the sciences, if we really want to do this, but does not help us judge between genuine sciences and pseudo-sciences. Laudan (1983) agrees that the important question to ask of a discipline is whether or not it is legitimate. The third feature of Popper's proposal that we will consider is that his criterion is a methodological rather than an epistemic one.

Falsificationism: characterizing science by its method

For Popper science is distinctive in its method. Scientific theories are boldly subjected to test. In this section we will see that:

- i) Falsificationism is an element, but not the essence of science.
- ii) There are times when it may be admissible to evade falsification.
- iii) It is easy for pseudo-sciences to pay lip-service to what is ostensibly scientific method.

For these reasons falsificationism, on its own, does not adequately distinguish science from pseudo-science. Many philosophers of science are saying that the prospect of discovering some criterion of demarcation is bleak, if not impossible. In a latter section, I suggest that these difficulties may be circumvented by altering our approach to the problem. Instead of searching for a criterion of demarcation it may be more fruitful to search for an indicator of demarcation.

Falsificationism is an element, but not the essence of science

First, it will be necessary to review Popper's proposal. One of Popper's (1979:106-108) major contributions is his distinction of three worlds. These are: World 1 - the world of material things; World 2 - the world of subjective beliefs; and World 3 - the world of ideas, art, science etc. In demarcation we are concerned with appraising the products of knowledge i.e. we are concerned with distinguishing between domains within World 3. Let us set up a basic statement about what science is. A statement that should be fairly well agreed upon is that science is characterized by the world having some say in shaping out theories, or to use Popperian terminology: World 3 is in some way influenced by World 1. There is a connection between the real world and our ideas about it.

Now what is important to note here is that Popper does not begin with the connection that scientific theories should have with the real world, but the manner in which they are tested. These two theses I will call the *connection thesis* and the *methodological thesis*. However, a theory as a whole may be constrained by the evidence, even though some of its constituent hypotheses are difficult to boldly subject to test. For example, there is no doubt that speciation occurs even though in some cases it is difficult to decide between alternative modes. These two theses must be distinguished, therefore, and the connection thesis may be more fundamental than the other. This point is diagrammatically represented in figure 2-1.

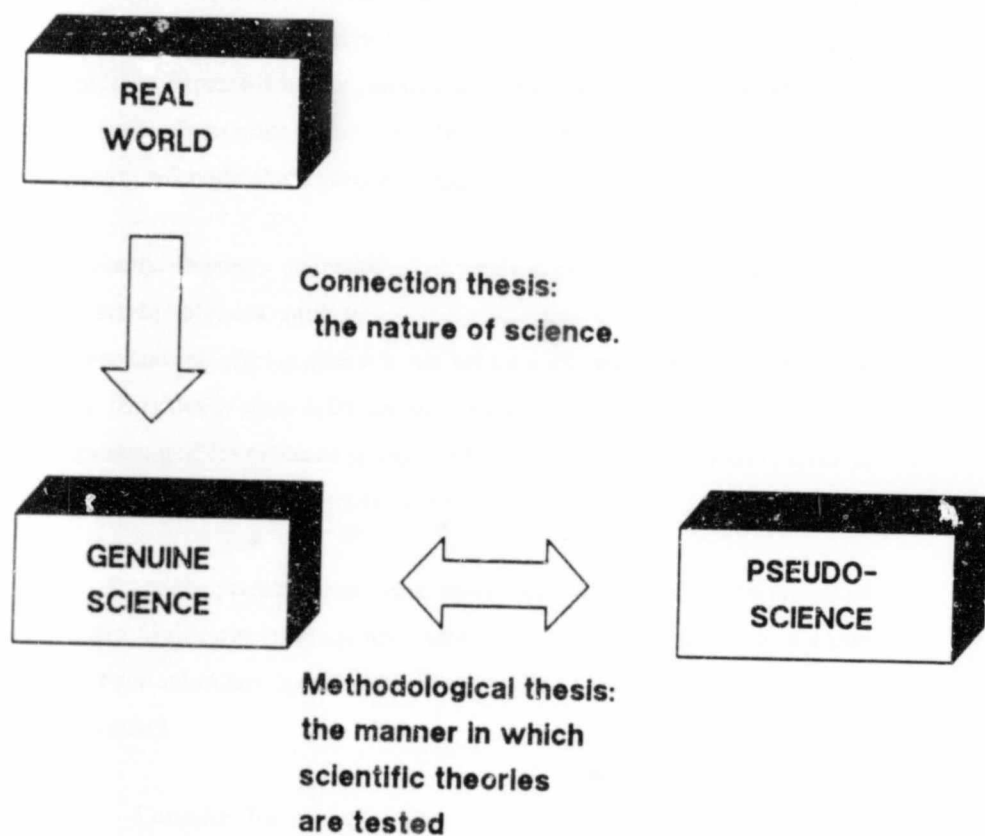


Figure 2-1. The relationship between the connection and methodological theses.

If our criterion of demarcation is merely a tool for distinguishing science from pseudo-science, rather than making a fundamental statement about what science is, we should rather speak of an indicator of demarcation. The reason is, as I will show below, that there is no necessary relationship between what a science is, as expressed by the connection thesis, and Popper's methodological thesis. Popper's criterion does not really isolate what is scientific about science and pseudo-scientific about pseudo-science.

Firstly, Popper's methodological thesis does not isolate what is wrong with pseudo-sciences such as creation science. Creation science is not pseudo-science simply because it is not boldly subjected to test, but for several reasons. Only one of these is its appeal to miracle (Gould 1984:127). Another is their distorting of the evidence (Hardin 1983). As we have already seen, some of their claims are unfalsifiable while others are false.

Secondly, perfectly genuine sciences may not quite follow the pattern of testing that falsificationism requires. Certain theories may be testable as a whole and yet have subsidiary hypotheses which are not as testable. Are they thereby less scientific?

Consider, for example, Darwinian histories. Kitcher (1985:58-75) distinguishes two types. Minimal Darwinian Histories specify the sort of changes that take place along an evolving lineage. Ambitious Darwinian Histories try to identify the causes of the evolutionary changes. Now Kitcher points out that the former are more easily confirmed than the latter. It is often hard to test between rival Ambitious Darwinian Histories.

Ghiselin (1969:31) reports that many of the attacks on Darwinism are based on the demand that the parts be as testable as the whole. It has become evident that this demand is unreasonable. Quite acceptable tests of a statistical kind can be made of evolutionary theory as a whole (Ghiselin 1969:66, Williams

1973, 1985, Kitcher 1985:58-75, Rosenberg 1985:216-219). The important thing is to ask the right sort of questions. They should not be about individuals, but about populations (Williams 1973). To take a simple example: given two parents with sickle-cell anemia a genetic counsellor could not predict whether their first-born would be affected. However, geneticists can, and have, successfully predicted the proportions of homozygotes and heterozygotes in a population.

This is not to say that nothing of falsificationism can be saved. It has its place: it is just not the whole story. Kitcher (1985:61) says it well: "employing the falsifiability criterion to settle methodological issues [he means naively] in ... the theory of evolution ... is about as profitable as trying to perform delicate surgery with a rusty kitchen knife."

There are times when it may be legitimate to evade falsification

This is something that has been accepted by Popper and has been convincingly demonstrated by many philosophers of science (e.g. Duhem 1968:173, Quine 1953:41, Kitcher 1982:42-45, 1985:59-61). Lakatos (1970:100-101) expresses the argument well. Imagine a physicist accepting Newtonian Mechanics and some initial conditions, and from these plotting the path of a planet P_1 . Then the planet is observed to deviate from the predicted path. Does this constitute a falsification of Newton's theory? Lakatos says no, not necessarily. It could always be argued that another planet P_2 is perturbing the orbit of P_1 . Even the most admired theories, then, fail to forbid any observable states of affairs. One could always invoke a hitherto unknown cause to explain the deviation from the expected.

Furthermore, we surely do not want to label Newtonian Mechanics as pseudo-science because it is not strictly falsifiable. The important question seems to be whether there actually exist causal factors which are influencing the

expected results. If there are, the deviation from the expected would not constitute a falsification.

Many or most would agree that there is something right about falsificationism; we should be willing to give up theories in the face of contrary evidence. However, it is not legitimate to elevate it to an inviolable rule. Below is an attempt to isolate what it is about science that makes it too simple to accept falsificationism, at least of the naive kind. We have noted that a theory can always be saved from falsification by the introduction of *ad hoc* hypotheses. The central problem shifts, then, towards the character of the *ad hoc* hypothesis. Popper (1972a:81-84) accordingly provides additional rules for distinguishing between genuine and *ad hoc* adjustments to theories; for distinguishing between hypotheses introduced just to save a theory and hypotheses which add empirical content to the theory. In chapter 3 it is argued that it is very difficult, if not impossible to distinguish these on *a priori* grounds even though serious attempts have been made to do this (Leplin 1982). I submit that it is safest to make a judgment with the benefit of hindsight.

Let me elaborate using Lakatos' example. The path of a planet P₁ is plotted on the basis of Newtonian Mechanics and initial conditions. The planet deviates from the calculated path. So it is proposed that a planet P₂ perturbs P₁. Does this constitute an *ad hoc* hypothesis? We do not want to say so if planet P₂ actually exists and is exerting a gravitational pull on P₁. But this is precisely what we do not know; at a given moment we require knowledge which is not available to determine whether a hypothesis is *ad hoc* or genuine.

Now, it is not that nothing of falsificationism can be saved. Lakatos (1970:100) must surely be wrong when he says that scientific theories cannot forbid any observable states of affairs since, in the words of Ruse (1982), Newtonian mechanics forbids planets to move in square orbits. However it does

seem that falsification requires hindsight. We cannot demand instant falsification.

Pseudo-scientists can easily pay lip-service to scientific method

This does not, of course, invalidate falsificationism as a criterion of demarcation. What it does mean is that at the practical level we have to be able to distinguish between genuine and fake attempts at pursuing falsificationism. Philosophers of science regard astrology as a typical pseudo-science. Yet the methods of astrologers are sometimes very similar to those found in the unequivocal sciences (Eysenck 1984). For evolutionists creation-science is a pseudo-science yet Laudan (1982:151) notes that scientific creationists have sometimes turned back on their original views. He says further (1982:153) that it is very easy for creation-scientists to specify in advance under what conditions they would give up their views. All they need do is to make it so that these conditions are very unlikely to ever occur. They could say that they would give up their theory if a clear intermediate between apes and men could be found in the fossil record. Of course this is based on a misunderstanding of evolutionary theory. Now, a deeper analysis of these pseudo-sciences should reveal that it is only in appearance that they mimic scientific method. But these considerations have to be taken into account to exclude safely certain pseudo-sciences on methodological grounds. The object of the next section will be to see if these difficulties can be circumvented by devising a supplementary indicator of demarcation.

Towards an indicator of demarcation

Bypassing the difficulties with falsificationism

We have seen that one problem with Popper's criterion of demarcation is that at any given moment it is impossible to decide whether a hypothesis is *ad hoc* or not. With hindsight the decision is easy. Most people today would clearly recognize phlogiston theory as *ad hoc*. This is because they have had the benefit of twentieth century scientific knowledge, which was not available at the time phlogiston theory seemed a genuine alternative. This seems to have led philosophers of science, particularly Lakatos (1970) to suspect that when devising a criterion of demarcation one should not focus on a theory's status at any moment, but on the performance of a series of theories.

Lakatos (1970) distinguishes between what he calls progressive and degenerating problemshifts. A progressive problemshift, in contrast to the other, is a research programme which is fruitful, in which new facts are being discovered etc. This criterion has been used with some success to exclude creationism (Kitcher 1982:124-164). It does seem promising to alter the unit of appraisal.

Another difficulty in devising a criterion of demarcation is that there is some diversity in science. Even if there were such a thing as the 'essence of science' it is possible that this would be indefinable. It may well be that such an essence is an attitude held by a community of scientists. These difficulties may be circumvented by using the outcome of method (whatever this may be) rather than the methods themselves as the criterion.

It seems, then, that our indicator of demarcation should include time, and should be based on the outcome of method. What indicator would serve this purpose?

First, we need to decide on a unit of appraisal. For the Logical Positivists it was the statement. For Popper it was a system of statements or a theory. For Lakatos it was a series of theories. I propose that we take the trend further and make the unit of appraisal the discipline. For our purposes, a discipline can be defined as an area of study with all the theories that have ever been proposed in it. Examples are physical optics, astrology, psychoanalysis, geomorphology and parapsychology. My thesis is that the pattern of theory succession in a discipline is a good indicator of its scientific or pseudo-scientific status.

My argument is in the classical Realist tradition (there are, in fact, several contemporary versions (Haack 1987)). The classical Realist argues for the existence of a real world on the grounds that this would explain the success or progress of science. In this argument the success of science is the given. The conclusion is the existence of an independent physical world. If we make the real world the given, then the success of a discipline will mark it out as genuinely scientific. Several authors have suggested progress as a criterion of demarcation (e.g. Lakatos 1970:116-119). There are, however, problems with this notion.

One of these is the difficulty in defining 'success' or 'progress.' Another is that the arguments for Realism are not conclusive (Laudan 1981a, Putnam 1982). For example, Laudan (1981a) says that it has sometimes happened that very successful theories, successful in accounting for a large number of facts, have later turned out to be false. These false theories were thought to refer to parts of a real world. Might not contemporary and successful theories actually not genuinely refer to the real world?

To these sorts of objections there have been responses and counters to these responses (e.g. Hardin and Rosenberg 1982, Laudan 1984). We will not pursue this discussion. Suffice to say that the adequacy of our indicator of demarcation does depend on the cogency of the Realist argument, and that there have been attempts to develop a form of Realism of the type needed here (e.g.

Almeder 1987); one which retains the classical belief in the existence of a real world, but which takes into account the criticisms of the anti-realists.

Our argument begins with certain basic assumptions (as opposed to basic judgments) about the nature of scientific disciplines versus pseudo-scientific ones. The question now is whether one can predict what the behaviour of their theories would be like? If this behaviour is markedly different, then a useful indicator of demarcation will have been found. Later we will use basic judgments to test the adequacy of this indicator. This approach is two pronged. Traditionally demarcationists fell into two camps. Prescriptive demarcationists set up an *a priori* criterion which had to be satisfied before something can count as a science. Descriptive demarcationists dissected out the essential elements of what they took to be science (Dolby 1987). This account uses both approaches. Circularity is avoided because I use basic judgments as a check and not as the foundation stone for the indicator.

The first step is to make a fundamental statement about the nature of science that must be true of anything that one would want to claim to be science. Here is the statement: Theories in the genuine sciences are constrained by the real world through experience. This statement lies at the heart of many claims about science. Ruse (1982:132), for example, says: "It seems fairly clear, that what distinguishes science from non-science is the fact that scientific claims reflect, and somehow can be checked against empirical experience - ultimately, the data that we get through our senses." It can be traced in Popper's (1972a:41) claim: "It must be possible for an empirical scientific system to be refuted by experience." Ultimately, it can be reduced to the claim that scientific disciplines are subject to the selective pressures of the real world; the products of knowledge (world 3) are connected to the real world (world 1). Although this statement tells us something about what science is, it does not help us distinguish science from pseudo-science; it may however, help us predict something about the nature of the products of knowledge which would be peculiar to science. I submit that it

can; that it predicts a distinctive historical pattern of behaviour for scientific disciplines.

If a discipline's theories are being genuinely constrained by the real world, then one would expect that in each successive stage of its development the range of potentially acceptable theories would be progressively reduced. Bechtel (1984) would agree. He suggests that "progress could be recognized as occurring within a science if selection forces continually narrowed the domain of acceptable theories." The beginnings of science will be marked by great theoretical diversity; widely contrasting theories will enjoy equal favour. The reason is that there is not enough feedback from the real world to exclude many alternatives. In time, with incoming data, progressively more and more alternatives can be excluded. Under the influence of the real world scientific disciplines will operate as a negative feedback or self-correcting system (Sparkes 1981, Blackhowitz 1987) and will appear to be goal orientated (see figure 2-2). A number of authors have given a similar description of this process using biological evolution as an analogy (e.g. Toulmin 1967, Popper 1974:413-463, Lewontin 1970).

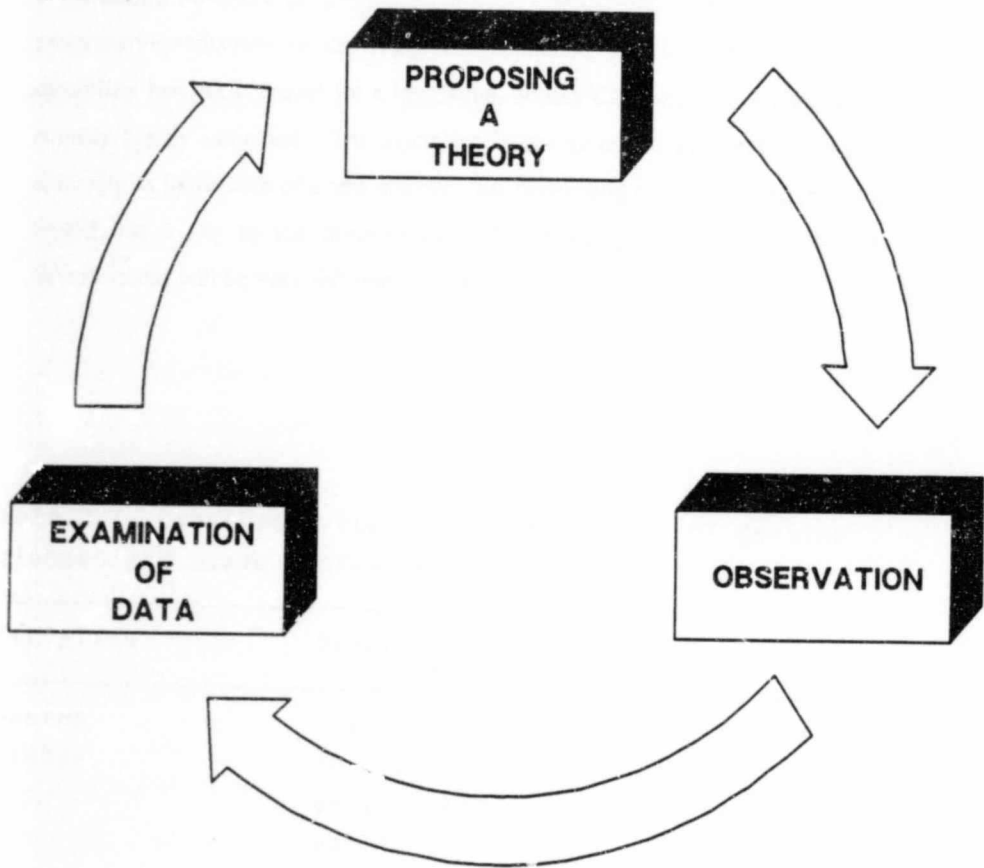


Figure 2-2. Negative feedback in science.

Let us contrast this pattern to what we would expect from a pseudo-science. In this case data from the physical world would not have a constraining influence on proposed theories. Consequently there would be no progressive reduction in the range of acceptable theories. Even after the discipline has been around for a long time, widely differing theories would be equally highly esteemed. The discipline is characterized by great theoretical diversity as in the case of a new science. So, depending on whether the physical world has a say in the theories of a discipline, its pattern of historical development will be very different. This is represented in table 2-1.

Table 2-1. Expected patterns in the history of genuine sciences and pseudo-sciences.

Discipline	Premiss	Expected pattern
Genuine science	Theories are constrained by the real world.	Diminishing theoretical diversity.
Pseudo science	Theories are not constrained by the real world.	Stasis in theoretical diversity.

This feature of the sciences is useful to us. Presented with a several disciplines claiming to be sciences, we will be able to identify the genuine ones. Genuinely scientific enterprises will be marked by an ongoing reduction in theoretical diversity. This historical pattern will be called exclusionary convergence. Pseudo-sciences, in contrast, will be marked by great theoretical diversity and stasis. The indicator of demarcation used here is that of success, but success is seen here as the ability to exclude a greater and greater range of possible theories.

Traces of the idea of exclusionary convergence can be found in the writings of many philosophers of science. Peirce (1940:50) mentions the elimination of ideas in a process he likens to Darwinian evolution. Kuhn (1962:10-22) sees this pattern, but only in a particular stage in the development of science- the route from Multi-paradigmatic Science to Normal Science. Lakatos' interest in the historical development of science is seen in his altering the unit of appraisal from the single theory to a series of theories (Lakatos 1970:118-119).

With Lakatos we see some attempt to demarcate using the pattern in a succession of theories. Progressive problemshifts are research programmes, involving a series of theories, which are fruitful. However, Lakatos' unit of appraisal is still much restricted in time. As mentioned earlier, we will use the discipline as a unit of appraisal. Furthermore, unlike Lakatos, we will not be attempting to demarcate in the traditional sense of giving a definitive characterization of science and pseudo-science.

There are both advantages and limitations to this approach. We saw earlier that a problem with using scientific method as a criterion is that pseudo-scientists can pay lip service to method. This may not always be easy to detect. Using a historical criterion avoids this difficulty because pseudo-scientists cannot alter the history of a discipline. Note too that the success of science is defined in a very particular way; the exclusionary convergence pattern was not expressed in terms

of truth or truthlikeness. The usual description of convergence is that current theories better approximate the truth than their predecessors. Not that truth is unimportant but there are serious difficulties in incorporating this concept in our understanding of convergence (Laudan 1981a). With our historical indicator we can meet the urgent need for identifying the genuine sciences without a criterion for truth.

While the indicator of demarcation was not derived from basic judgments about which disciplines are scientific and which are pseudo-scientific, these judgments can be useful in checking the adequacy of this indicator. In the next section I will briefly describe the history of a classical science and pseudo-science to determine whether each conforms to the pattern expected by our indicator of demarcation. First, a consideration of physical optics.

The historical development of physical optics: does it display exclusionary convergence?

If the indicator of demarcation is sound, we would expect the initial stages of the development of physical optics to be characterized by the occurrence of a number of competing explanations given to account for the nature of light, where these explanations differ widely. This is expected because there has been little feedback from the real world, so that almost any explanation can account for the "facts". This is exactly what we find. Kuhn (1962:12) states: "No period between remote antiquity and the end of the seventh century exhibited a single generally accepted view about the nature of light. Instead there are a number of competing schools and sub-schools, most of these espousing one variant or another of Epicurean, Aristotelian or Platonic theory.

One group took light to be particles emanating from material bodies; for another it was a modification of the medium that intervened between the body and the eye; still another explained light in terms of an interaction of the medium

with an emanation from the eye; and there were other combinations and modifications besides."

In short, there existed widely differing explanations of the nature and behaviour of light. Now according to our indicator of demarcation, if physical optics is indeed a science, and we are quite certain it is, then later in its development it should display an ongoing limiting of the range of acceptable explanations. It will display exclusionary convergence; more and more theories will in time become clearly unacceptable. This is what we find.

In the nineteenth century there came to be two schools of thought over the nature of light. One school considered light to be particulate, the other took it to be wave-like. All agreed that light originated from the sun, reflecting off material objects and not usually emanating from them. What we find then is some disagreement about the nature of light, but a definite consensus about what is strictly unacceptable. There is a narrowing down of the number of recognized theories. The wide range of optical theories in vogue before the seventeenth century came to be discarded by nineteenth century scientists.

Physical optics seems to follow a specific historical pattern, described as exclusionary convergence, and indicative of scientific disciplines according to our indicator. As physical optics is quite obviously a science, the proposal is supported. There must be little doubt that Cell Biology is a genuine science. Interestingly enough, Bechtel (1984), whose concern was with distinguishing between good and bad science, has described the history of Cell Biology and it conforms to the pattern we would expect. He says that "within fields like cell theory I think we can see a process of theory change where successive theories do seem to approximate our current ones more than their predecessors did." The question now is whether our indicator clearly identifies a pseudo-science such as astrology.

The historical development of astrology: does it display stasis in theoretical diversity?

For many philosophers of science today, astrology presents itself as a typical pseudo-science. A useful check of the adequacy of our indicator would be to see what it does with astrology. First, astrology will be defined. Then the indicators of a science will be briefly described. Lastly, astrology will be evaluated in terms of our indicator.

Astrology has been defined as "the science of learning man's character and destiny through a study of the governing forces radiating from the planets..." (Norvell 1970). It would be a major task to examine the whole of astrology, as a wealth of material has been written on the subject. This discussion will be restricted to one aspect, the nature of the supposed cosmic influences that control the destiny of man.

If astrology is indeed a science, it should show a reduction in theoretical diversity with time. Since astrology is one of the oldest disciplines, and since the nature of these cosmic influences is such a fundamental issue, and if it is a science, then certain basic alternatives as to the nature of the cosmic influences should have come to be excluded. For example at the most basic level it should be known whether the cosmic influences are supernatural or natural: Astrologers are undecided. Sometimes they speak as though the cosmic influences are natural: Raman (1972:48) ascribes wavelength, intensity and frequency to planetary vibrations. At the same time he is not past considering the cosmic forces to have a divine element. He does not find it unreasonable to believe that the stars are "the bodies of Great Beings replete with wisdom" (Raman 1972:44). At this fundamental level astrologers are not agreed.

They are not even decided on whether to call the cosmic agents influences. One author writes: "The present day astrologer does not think in terms of light

rays or influences coming direct from each planet... the essential correspondence is one of space-time synchronization" (Mayo 1964:2).

Astrology has had centuries to come to an agreement about what is admissible and inadmissible. Instead it presents itself as a potpourri of widely contrasting ideas, simply because it has failed to exclude any alternatives. It is unchanging. Astrologists freely admit this fact, actually trying to use it to establish the scientific credentials of the discipline. This can be seen from the following statement by the astrologer Norvell (1970): "In the very fact that astrology has come down through the centuries unchanged except for the addition of new knowledge lies the greatest proof of its verity and force."

From the point of view of our indicator, these words far from vindicating astrology's claims to be a science, condemn it. This fact supports the proposal as an effective indicator of demarcation.

That astrology does not show a reduction in theoretical diversity is a prompt for us to look deeper. Its behaviour is symptomatic of a deeper problem: astrology is not a science because its notions are not subject to the dictates of the real world; therefore it cannot exclude any alternatives. This fact leads to its characteristic vagueness and lack of agreement as well as its characteristic pattern of historical development.

Having tested our indicator by examining the history of an obvious science and pseudo-science, it seems clear that it is an effective indicator of demarcation. It now remains to evaluate the credentials of evolutionary biology and creationism in terms of this indicator.

Is evolutionary biology a science?

My focus is not on any particular theory, but on the history of the discipline as a whole. If evolutionary biology is indeed a science, then we would expect its history to follow a pattern of decreasing theoretical diversity. It is the broad trend that we are interested in, so only a very brief history will be given below.

Before Darwin there had been many views concerning the origin and diversity of life on earth. The fundamental tenets of these ideas varied enormously. Views ranged from naturalistic to supernaturalistic, and from a static cosmic order to one of great flux and there were various combinations of these.

Within supernaturalism there was diversity. In some creation myths the universe was said to have a beginning (James 1969); for the Jains the universe was eternal (James 1969:45). The order of creation also differed. For example the Crow Indians believed that the earth developed from water, while the Pawnee Indians believed that water came into existence after the earth (James 1969:5-6). In the *Popol Vuh*, the sacred writings of the Quiches, it is said that the various animals were rejects from attempts by the gods to make man. In other myths man is created first (James 1969).

Some tried to combine a naturalism and supernaturalism. For example, Gregory of Nyssa (331-396) held that God gave matter its fundamental properties and allowed these to develop into the completed forms. Similarly, Augustine (353-430) and Bruno (1548- 1600) tried to combine a concept of creation and change (Thomson 1895:215). Augustine held that a germ of each species developed by secondary causes into mature forms (Butler 1931:XVII). These gave a supernaturalistic account for the origin of the material of life, but a naturalistic account for change and diversity in the living world.

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