# 3 Hypotheses and Geomorphological Reasoning

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*Science does not rest upon rock-bottom. The bold structure of theories rises, as it were, above a swamp, but not down to any natural or 'given' base; and when we cease our attempts to drive our piles into a deeper layer it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure, at least for the time being.*

*(Popper, 1959, p. 111)*

ABSTRACT

Geomorphology is a way of thinking about the surface of planet Earth. Controlled experimentation, in the manner of pure physics, is not possible for most geomorphological concerns. Thus, much of conventional analytical philosophy of science, which is based on the exemplar of experimental physics, fails to portray important aspects of geomorphological reasoning. This is particularly true of hypothesizing, which was recognized by Gilbert, Chamberlin, and Davis as a central methodological concern of geomorphology. Geomorphological reasoning largely relies upon retroductive inference, which Charles S. Peirce described as 'the spontaneous conjectures of instinctive reason'. Because it reasons from real effects to real causes, eventually colligating (bring together) facts under a conceptual scheme (hypothesis), retroduction bridges the gulf between nature and mind. Geomorphological indices, such as landforms and sediments, are signs for which causative processes are inferred retroductively. Though superficially similar to lucky , 'guessing', retroductive inference succeeds in generating fruitful hypotheses (some of them outrageous) because the human mind is instinctively attuned to certain aspects of nature. This instinctive propensity in science to 'guess right', which Galileo called *il lume naturale,* may derive from fundamental properties of the universe and mind that modern cosmologists have named the 'anthropic principle'.

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## INTRODUCTION

Geomorphology is a way of thinking (*Logos*) about the surface (*morphos*) of planet Earth (Gaia). It is also viewed as the body of knowledge or facts about Earth's surface. The distinction in these two meanings has consequences for the conduct of the science. As a body of facts geomorphology assumes monolithic proportions, since the potential facts are presumed to be endless in number and bewildering in complexity. It is obvious to many scientists that the appropriate task in the face of this complexity is to simplify. This can be done by developing a scheme of classifying the numerous individual facts into a smaller number of categories. This is one type, not the only type, of synthesis, whereby entities are combined in wholes, which, in this case, are conceptual categories. Alternatively or subsequently, simplification can arise by establishing a small number of basic principles from which it should be possible, at least in theory, to deduce the details of landform and process complexity on Earth's surface. This activity is analysis, the separation of the extremely complex (presumably intractable) whole into its component parts in order to study them in simplified form and thereby understand.

Inference is the logical process in which some conclusion is reached from a set of statements. Analytical inference derives its conclusion from premises that are assumed to be true. Synthetic inference begins with factual statements and reasons toward the conclusion. Analytical inference corresponds fairly well to the logic of deduction. Much more controversy surrounds synthesis, which many philosophers of science have equated to the logic of induction. Induction is the reasoning from individuals or particulars to general or universal statements about them. This is not the only kind of synthetic reasoning, as will be shown in this chapter.

Closely related to the idea of simplifying complexity is reductionism, or reductivism, which is a philosophical belief in one type of methodology or one science that encompasses the principles applicable to all phenomena. From such principles it should be possible to deduce all observed entities. If geomorphology is viewed as a bewildering complex of facts, an analyst will find great appeal in this philosophy. Thus, a reductionist might conceive of geomorphology as a science for which one should establish simplifying and generalizing principles from which one might then construct model simulations of landscape evolution and/or process operation. This viewpoint also leads to an emphasis upon readily measurable processes, which are those operating over the relatively small spatial and temporal scales that are most easily accessible for the presumed verification or falsification of theoretical models (Baker 1988a).

Modern philosophy of science reflects the general concerns of Western philosophy as a whole. Until about 1960 it would have been rather easy to divide those concerns between analytical philosophy and continental philosophy. Existentialism was the predominant form of the latter, and logical positivism (or logical empiricism) was a predominant form of the former. Since 1960 both these ideologies have fallen into disfavor, but the analytical versus continental distinction has remained. Most philosophy of science is in the analytical tradition, and much of what many scientists themselves know of it is in the older (discredited) logical empiricist form, which held science to deal with problems of fact while philosophy deals with problems of methodology and conceptual analysis. The closely related logical positivist school held to a single, identifiable scientific methodology and to various principles of verification and cognitive meaning, all based on strict rules of logic. It is not difficult to see why the science of physics, particularly classical mechanics, became an exemplar or paradigm for analytical philosophy (Frodeman 1995). Physicists seek the timeless, invariant laws of nature. The laws of physics make predictions that can then be verified against the facts of nature. This verification is possible because nature can be studied objectively in appropriately isolated systems known as 'controlled experiments'.

Since 1960 analytical philosophy has moved in many new directions. Kuhn (1962) shocked the reductive and logical sensibilities of the logical empiricists with his notions of relativism and the role of the community of scientists in determining research paradigms. Popper (1959) and Quine (1953), among others, showed that several basic concepts of logical positivism, including the verification principle, were untenable. Nevertheless, assumptions of the logical positivist program remain, if not universally among analytical philosophers, still rather prominent in the thinking of many scientists who write about the foundations of their subject. For example, highly reductive analytical philosophers have proclaimed a new field of cognitive science that aspires to combine the conceptual skills of philosophy with artificial-intelligence modeling and experimental psychology.

Analytical philosophy of science devotes great emphasis to the methodological treatment of laws, theories, and hypotheses. These three methodological categories can be considered ideas in a hierarchical order in which laws are 'better established' than theories, and theories are better established than hypotheses. Notice that this view preserves the logical empiricist concept of justifying knowledge objectively. In this treatment, laws and theories, which find their most elegant expression in mathematical physics, receive inordinate attention. In contrast, geomorphology, like geology, can be considered to be a science of hypothesis, which simply means to a reductionist that geomorphological theorizing has not advanced to the point where sophisticated theorizing and the establishment of fundamental laws have been achieved. Shamos (1995, p. 96) makes the point this way:

> ... In general, as a science matures, it passes first through a purely descriptive stage, then proceeds to an experimental stage, and finally, as meaningful patterns are seen to emerge, to a theoretical stage, where one usually finds it highly productive to use the language of science (mathematics) to describe natural phenomena and uncover new knowledge. Physics and chemistry are already at this final stage; biology and Earth science, involving as they do far more complex systems, have been slower to mature to this level.

## GEOMORPHOLOGY AS A WAY OF THINKING

There is no science in a body of absolute facts. Science is a process of thought and observation directed toward understanding, which is a faculty of mind that allows human beings to grasp reality and thereby cope with the real world. Reductionism, objectivity, and simplification are means for achieving this goal, but they are not the only means, nor do they necessarily have special privilege in this task. Though reductionism can proceed with an especially effective mode of logic, deduction, its tools are highly limited for relating to the messy complexity of the real world, as encountered in sciences like geomorphology for which experimental access to nature's reality is very limited. Indeed, there are sound logical reasons why in Earth science controlled experiments are inadequate to the task of verifying or validating the predictive consequences of theoretical models (Oreskes et al. 1994). More practical issues in regard to controlled experimentation in geomorphology are discussed by Ahnert (1980) and Church (1984).

Thinking about Earth's surface involves a developmental process of growth in that thought. In contrast, reductionism involves a narrowing of thought. If the research aim is reductionistically to deduce or predict certain phenomena from first principles, then the obvious task of the field researcher is to measure those phenomena very carefully in order to check and refine the theory. In this way the complexity of the field problem is reduced to manageable proportions and to a focus on phenomena relevant to key scientific questions. The alternative might be presumed to be a chaotic program of pointless observation, perhaps motivated by a faith that some order will be discerned through induction.

Induction, as noted above, is one type of synthesis in which inference is from particular instances of something to some general or universal statement about those instances. For Francis Bacon, and many other early philosophers of science, induction was a method of scientific discovery. One began with data (particulars in the above definition) and worked toward high-level principles (universal statements about those data). Clearly, science, notably physics, generates such universal statements, and it does so by reference to data through scientific experiments. All science seems to be decidedly inductive. The inductive method was critically assessed by the great empiricist philosopher David Hume. Hume reasoned that it was impossible to ever justify a law by experiment or observation. Just because one sees the sun rise each day does not, of itself, require that it will rise the next day. This is the famous logical problem of induction, which Sir Karl Popper (1959, p. 54) summarized in terms of three seemingly incompatible principles:

> ... (a) Hume's discovery ... that it is impossible to justify a law by observation or experiment, since it 'transcends experience'; (b) the fact that science proposes and uses laws 'everywhere and all the time'... To this we add (c) *the principle of empiricism* which asserts that in science, only observation and experiment may decide upon the *acceptance or rejection of* scientific statements, including laws and theories ...

Popper claimed to solve the problem of induction via his principle of falsification. This allowed principle (a) to remain compatible with principles (b) and (c). As he states (Popper 1959, p. 54):

> ... the acceptance by science of a law or theory *is tentative only;* which is to say that all laws and theories are conjectures, or tentative *hypotheses* (a position which I have sometimes called 'hypotheticism'); and that we may reject a law or theory on the basis of new evidence, without necessarily discarding the old evidence which originally led us to accept it ...

Popper's insight is widely regarded as one of the great achievements for the analytical approach to philosophy of science (Lindh 1993). Popper further reasoned that, since science cannot advance by inductive confirmation, the appropriate mode of advancement is by the imposition of bold conjectures (Popper 1969). These are hypotheses about the world that require completely new models for their scientific exposition. However, as psychological matters of creative thought by individual scientists, the origin of these conjectures lies outside of analytical philosophical discourse. It follows, of course, that such philosophy can tell us little about the growth of geomorphological thought, since that thought depends on the origin of hypotheses, as argued by Gilbert (1886, 1896) and by Chamberlin (1890).

One notes in Popper's work the epistemological distinction between the context of discovery and the context of justifying knowledge, the latter being the sole philosophical concern. However, in the last 25 years there has been a reexamination of discovery by analytical philosophers. Kantorovich (1993) divides current thinking on this matter into two camps. The predominant view is that discovery is not a logical matter (Laudan 1980), as Bacon proposed for induction. Instead, discovery is studied in a historicist/particularist manner, in which individual scientific case studies are examined for their lessons about the scientific process. A great many social and ideological factors are found to play a role. Another, growing, view holds discovery to be a matter for cognitive science, eventually seeking to model the process via computerized artificial intelligence.

Quite peripheral to the mainstream philosophy of science, described above, is the resolution of the problem of induction nearly a century before Popper's work. The resolution is controversial, as is the work of the logician who accomplished it, Charles S. Peirce, who is now widely regarded as 'the one truly universal mind that nineteenth-century America produced' (Dusek 1979). Peirce's life contains many ironies that delayed external recognition of his philosophical importance (Brent 1993), but he was well known to the scientists of his own day (Fisch 1980), and his relevance to our own time is increasingly being recognized (Ochs 1993; Hausman 1993). Peirce is of particular interest because of his direct connection to the late nineteenth-century American geomorphologists G.K. Gilbert, T.C. Chamberlin, and WM. Davis (Baker in press). All these scientists wrote highly influential papers on the nature of geomorphological reasoning, and these show the possible influence of Peirce's philosophy (Baker in press).

W.H. Davis (1972, p. 34) describes Peirce's resolution of the problem of induction:

> The point [Peirce] made is essentially this: contrary to Hume and contrary to practically every epistemologist since him *scientific reasoning does not depend upon induction at all!* Nor does it depend upon anything so simple as our ability to take habits. The rising of the sun as an example of inductive reasoning is drastically misleading by its simplicity. Scientific reasoning, indeed all of our reasoning, depends upon the mind's ability to have insights, to see things coherently and harmoniously, to see laws and principles, in short, to make up hypotheses. Hume has misled generations of philosophers because he utterly ignored the place of hypothesis in human thinking. Perhaps it is enough that he should have seen the vast importance of the law of association. But when someone grasps the principle behind the workings of some machine or of some feature of nature, he is *not merely being impressed by a succession of regularities,* he is not merely gaining a habit. He is having an insight, seeing principles, grasping interconnections. This is the feature of our mental life which was so wonderfully emphasized by Peirce, but Whewell, long before, saw the same truth.

Peirce credited Cambridge mineralogist William Whewell as one of the few logicians to have properly understood the logic of scientific inference. Whewell wrote extensively on history and philosophy of science in the early nineteenth century (Fisch 1991). His central concern was with the forming of antithetical couplings between (1) the objective facts of nature, and (2) new concepts suggested to scientific minds. Whewell considered this process to be a colligation ('binding together') of existing facts that are unconnected in themselves but get connected through mental concepts. In his treatises on logic, Whewell

(1858, 1860) referred to this process as 'induction', but, as we will see, Charles Peirce has distinguished this form of synthetic inference from the 'induction' of which Hume (and later Popper) are speaking. Peirce accorded it the various names 'hypothesis', 'abduction', 'retroduction', and 'presumption'.

Whewell supplemented his prospective view of hypothesis generation as 'colligation of facts' with a retrospective concept of hypothesis (or theory) appraisal in terms of criteria for 'factual truth'. These included (1) a prospective theory's tendency to converge and to simplify, and (2) the theory's repeated exhibition of explanatory surprise. The latter phenomenon he termed the 'consilience of inductions'. This idea that scientific hypotheses could prospectively approximate truths in nature was criticized vehemently by John Stuart Mill (1846). Whewell tried to support his position with historical case studies (Whewell 1837, 1840), arguing that science in practice works as he described. Mill, in contrast, argued on a detached logical basis that the real work of science lay in the establishment of knowledge by inductive proof. Mill's views were nominalist (discussed in the next section) and empiricist. These were the positions subsequently embraced by Bertrand Russell and other founders of logical empiricism and related strands of modern analytical philosophy. Whewell's ideas were declared defeatist because he made science dependent upon ingenuity and luck. As Wettersten and Agassi (1991, p. 345) observe: '... because philosophers ignored him and scientists did not write histories of philosophy, he was forgotten'.

Another unusual aspect of Whewell's philosophy was his interest in geomorphology and the Earth sciences. Whewell's review of Charles Lyell's *Principles of Geology* contains a devastating critique of a metaphysical principle espoused by Lyell and named 'uniformitarianism' by Whewell. Whewell's logic supported the possibility of an alternative position which he named 'catastrophism'. History shows that Lyell, a former lawyer, was the more skillful advocate of his particular philosophy, the defects of which continue to plague the Earth sciences (Baker 1978). One of the great ironies in history must be that the very scholars who have so long ignored Whewell's insights on science and logic bear the name that he first conferred upon them in his remarkable historical studies: 'scientists'.

## NOMINALISM VERSUS REALISM

Charles Peirce tied his vision of scientific hypothesizing to a kind of scholastic realism, traceable back to the writings of Aristotle and medieval philosophers, such as Duns Scotus. This view holds that universals (including theories, general concepts, and hypotheses) exist independently of our perceptions of them. The alternative, dominating in much of modern analytical philosophy and modern philosophy of physics, is nominalistism, a doctrine holding that generals do not refer to something real, but rather to the names we attach to things. Peirce resurrected older ideas of scholastic realism because of his mathematical explorations of the continuum concept (Ketner and Putnam 1992). Peirce believed his mathematical conclusions justified the view that nature was infused with a very rich kind of logic. To appreciate Peirce's view, however, we must consider ideas that developed during mainstream philosophy of science in the first part of this century.

Logical positivism, the dominating view for twentieth-century philosophy of science up to about 1960, is founded upon a nominalism espoused by Ernst Mach, Karl Pearson, Henri Poincare, and Bertrand Russell. The metaphysics of this position is best expressed in how the logical positivists dismissed any element of reality from all notions of theory, including the laws and hypotheses of science. Eisele (1959, p. 461) describes this as follows:

> ... Poincare felt that science can never reveal any absolute truth concerning nature. It can only set a relationship between a hypothesis and its implication, and the so-called 'laws' are but 'conventions' adopted as a matter of 'convenience' from among many possibilities. In this 'descriptive' theory of science, the natural laws were for Poincare, as they were for Mach and Pearson, merely fictions created by science to organize sense-impressions, and any order therein is imposed by the mind of man. A hypothesis resulting from observed data has no correlative object in nature. It is an intellectual device for stimulating and directing the discovery of further data, according to Poincare.

In this conventionalist view, scientific logic can be conceived as a syllogism of the following form:

Major, premise : All men are mortal.

Minor premise : Socrates is a man.

Conclusion : Socrates is mortal.

In classical physics the major premise, or rule, can be thought nominalistically to correspond to the various conventions, such as Sir Isaac Newton's laws of mechanics. The minor premise, or case, consists of real facts, for this example: the instantaneous relative positions and velocities of all particles at some point in time. Deductive logic allows a conclusion: that various accelerations will follow from the premises.

Poincare was able to describe the method of physics in these terms. One first sets up an axiomatic system from which consequences can be deduced. These are then judged retroactively by testing (experimentation) against what exists independently of us. Objective experimentation leads to further experimentation. Reality exists only in facts, which are organized through generalization. Every generalization is a hypothesis to be tested, and hypotheses are merely intellectual devices for facilitating the discovery of further data; they do not have corresponding objects in nature. Hypotheses, and the scientific theories and 'laws' to which they lead, are conventions adopted for the convenience of facilitating this scientific process. The order embodied in natural laws is an order imposed by the human mind. This is because statements in logic and mathematics are true by definition and not discovered by examining reality. Their application to natural laws, therefore, is definitional convention applied to reality.

The conventionalism of Poincare, Mach, and others was key to the revolution in physics during the early twentieth century. Though relativity and Einstein, an admirer of Mach, are most popularly associated with this revolution, it is quantum mechanics that has proven to be the most interesting in its philosophical implications. The generalization of quantum mechanics known as quantum electrodynamics is arguably the most successful theory in physics, judged in terms of its astonishing predictions and their experimental confirmation. This success is readily interpreted nominalistically. For example, one of the founders of quantum mechanics, Niels Bohr, was once asked whether the equations of this theory could be considered to somehow mirror an underlying, real quantum world. Bohr's answer is a succinct statement of the nominalistic character of the philosophy of physics (Petersen 1985, p. 305): 'There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can say about nature.'

The nominalistic philosophy of physics, and the logical empiricist scientific philosophy that it inspired, holds hypotheses to be useful fictions. They have no basis in reality. This is not the position of Charles Peirce, who made the bold conjecture that something akin to a logical pattern in nature was far richer than the restrictive deductive scheme given above. In his 1898 Cambridge Conference lecture series (Ketner 1992, p. 161) he wrote:

> What is reality? Perhaps there isn't any such thing at all. As I have repeatedly insisted, it is but a retroduction, a working hypothesis which we try, our one desperate forlorn hope of knowing anything ... so far as there is any reality, what that reality consists in is this: that there is in the being of things something which corresponds to the process of reasoning, that the world *lives,* and *moves,* and *has its being,* in [a] logic of things.

Peirce's view was unpopular in his own time and it remains unpopular today. Peirce attributed this unpopularity to the pervasive nominalism in the metaphysical presumptions of his contemporaries. He continues in his 1898 lecture (Ketner 1992, p. 161) as follows:

> I point out that Evolution wherever it takes place is one vast succession of generalizations, by which matter is becoming subjected to ever higher and higher Laws; and I point to the infinite variety of nature as testifying to her Originality or power of Retroduction. But so far, the old ideas are too ingrained. Very few accept my message.

Note that the logic which Peirce ascribes to nature is not the familiar induction or deduction discussed in most philosophy of science. He introduces his new term, retroduction, to describe this logic of hypothesizing.

The importance of the realist/nominalist debate can be made clear in regard to Peirce's view that hypothesizing involves reasoning (retroduction) from effect to cause. In the science of the nineteenth century a careful distinction was often made between (1) causes presumed to have a real existence in nature, and (2) figments of mind, presumed to posit such causes (Laudan 1987). Type (1) causes were named *verae causae*, following Sir Isaac Newton's famous 'Rules of Reasoning' in the *Principia.* The first of these 'rules' stated, 'We are to admit no more causes of natural things than such are both true and sufficient to explain their formation'. Type (2) concepts, which might well be nominalistic, were given the name 'hypotheses'. It is the type (2) concepts of which Newton speaks in this famous passage from the *Principia:*

> I feign no hypotheses *(hypotheses non fingo)* for whatever is not deduced from the phenomenon is to be called a hypothesis, and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena and afterwards rendered general by induction.

The 'hypotheses' described by Newton derive from mind alone; they are not 'inferred from the phenomena'. Peirce recognized Newton's distinction and noted that *verae causae* must be inductions, and are not matters of hypothetic inference. Peirce also agreed with Newton that 'hypotheses' (type 2) are not matters for belief. However, he envisioned scientific hypotheses as part of an inference, retroduction, that transcends the gulf presumed to exist between mind and real causes in nature. This connection was possible because of Peirce's view that all thought is in signs. Peirce's semiotic (theory of signs) is such that a natural continuity exists between the real causes in nature and the interpretations that are eventually made of those causes.

## GEOMORPHOLOGICAL SEMIOTICS

Logic is considered to be the study of rules for exact reasoning. For a nominalist logician, like John Stuart Mill, logic merely gives us a system of names for patterns of inference; there is no reality in the names, though the thought systems described by these names may refer to real objects. For Charles Peirce logic is a formal semiotic. It provides valid patterns (forms) for signs, which are the entities of which Peirce believed all thought to be composed. In Peirce's semiotic (scientific study of signs), he has a rather special meaning for the concept of a sign. The usual meaning is dyadic: a sign is anything that stands for something else. Peirce's semiotic involves a triadic definition of sign. His definition (Peirce 1902a, p. 527) is as follows:

> ... Anything which determines something else (its *interpretant)* to refer to an object to which itself refers (its *object)* in the same way, the interpretant becoming in turn a sign, and so on *ad infinitum.*

This definition highlights in striking fashion Peirce's view that thought is continuous. It cannot be arbitrarily divided with absolute distinctions, as nominalism does when distinguishing between particular things and the universals or generals that those particular things share in common. This nominalistic distinction for Peirce is contrary to the spirit of scientific inference, particularly as it seeks to interpret the continuity between the signs of nature with those of which our thought is composed. The continuity is essential in the retroductive phase of inquiry, when reason is creatively employed to generate a candidate best explanation. Nominalism in logic may be motivated by a sense that some sort of objectivity is needed with regard to testing hypotheses, but it is an inappropriate view to take in the creative phase of hypothesis generation.

Earth scientists make common reference to the sign system of nature using metaphors such as 'what the rocks tell us' or 'conversation with the Earth' (Cloos 1953). It is interesting to contrast these language references to those applied to controlled experimentation by Sir Francis Bacon. Objective control means that questions put to nature (experiment) occur as in an interrogation (Keller 1985). Nature is coerced, presumably to reveal her secrets, when data serve the sole function of verifying or falsifying. The metaphorical function of conversation is denied in such interaction. In the Earth sciences, however, controlled experimentation (interrogation) is largely precluded; one must converse with the Earth, metaphorically speaking.

How does one view this semiotically? In Peirce's semiotics the most sophisticated signs are symbols, for which the interpretant is essential. Mathematics can be considered a symbolic language of great power in deductive systems such as the theories of physics. Indices are signs for which the objects are essential and which have existence independent of their interpretants. Landforms, river alluvium, and radiocarbon samples are all indices in that their objects are the processes which cause them and their interpretants are what geomorphological thought makes of them. Thus, geomorphology can be viewed as a sign system involving natural indices. Geomorphological reasoning involves inferences organized through a formal serniotic, that is a logic, that is embedded in these indices. Similarly, mathematics and mathematical physics emphasize symbolic sign systems in their logical inferences.

It was Peirce's view that scientists, like all human beings, reason within thought, which is a system of signs. This contrasts the more common view, traced back to Descartes, that thoughts occur as clear and distinct ideas within individual minds. For Peirce the act of reasoning within collective thought involves other scientists as well as nature. Communication with other scientists is through familiar signs, called symbols, which include the language systems of words, mathematics, and symbolic logic. Communication with nature is less abstract, but it also includes signs; in this case the signs are indices. All signs have objects and interpretants. It is the challenge of the scientist to appreciate the advantages and disadvantages of these different sign systems for the pursuit of their inquiries. Those operating in one sign system, symbols, for example, may erroneously disparage the logic of those operating in another system, indices, for example. This would have analogous validity to a speaker of Latin disparaging a speaker of Chinese for their communication skills. The advocate of scientific symbolic language is likely to share the same appreciation of indices as the Latin advocate would hold for Chinese.

This brings us to the interesting question of the origin of geomorphological hypotheses. Grove Karl Gilbert (1886, 1896) proposed that hypotheses derive from analogy to antecedent phenomena of the real world. Gilbert's invocation of analogy as a source of hypotheses is very puzzling to a modern analytical philosopher. Kitts (1980) simply argued that Gilbert was totally wrong, and that hypotheses derive from theory. Kitts seems to have been so positive on this point because of the nominalistic logic that underpins much of analytical science. In his popular System of Logic John Stuart Mill (1865) described analogy as '. . . reasoning from particulars to particular'. This would certainly not produce theories, which are generals, not particulars.

In conventional logic (Salmon 1973) analogy is treated as a weak form of induction. The word 'weak' here refers to the strength of its explanatory power. It is important to remember that Gilbert was interested in discovery, not explanation, in the origin of hypotheses, not their justification. Analogy, as Gilbert (1886) clearly describes it, relates consequents (e.g. landforms) to their antecedents (e.g. causative processes). This logical manipulation of indices is handled by the cruder reasoning tool of analogy. Because the goal is not so much to generalize, which is the conventional philosophical goal attributed to induction, analogy functions as a combination of retroduction (inference to a hypothesis) with induction (inference from particulars). Thus, in the method of science described by C.S. Peirce and likely familiar to Gilbert (Baker in press), hypotheses are logically inferred when analogy is employed. In this sense, at least, hypotheses can be considered to originate by analogy, exactly as Gilbert (1886, 1896) proposed, but this is merely one example of the richness of potential reasoning that lies in the semiotic world of natural indices.

## HYPOTHESES AND SCIENTIFIC EXPLANATION

The view that scientific hypotheses can be derived from existing theories has been criticized by the philosopher Paul Feyerabend. Feyerabend (1975) proposes the advocacy of any alternatives that occur to scientists regardless of how outrageous they might seem. Like Davis (1926), he is concerned that normal scientific practice may result in potentially important hypotheses being rejected because they initially appear contrary to the prevailing theoretical structure. Feyerabend presents his own outrageous hypotheses about the nature of science, comparing its practice to that of religion and arguing that there is no such thing as a scientific method (Feyerabend 1975). These latter claims have led to Feyerabend's status as a maverick among philosophers of science. Nevertheless, many of Feyerabend's scientific critiques of philosophy of science accord well with the attitudes of Earth scientists. He argues that much of analytical philosophy has concentrated on trivial issues of logic and theoretical meanings, such as the nature of realism. Earth science gets on very well without these fine points of logic and theory.

Feyerabend's claim that hypotheses do not derive from theory is an important insight, but his proposed alternative that hypothesis choice is a kind of poetry can be rather easily misunderstood. He claims that scientists should choose hypotheses for the pleasure derived from that choice. This would seem the height of irrationality to analytical philosophers. Indeed, Feyerabend's 'anything goes' anarchical approach to scientific method has been seized upon by postmodernist critics of scientific rationalism and objectivity. If science can be 'what you like', then all forms of knowledge have equal status, including mysticism, witchcraft, and voodoo. Feyerabend's insights suggest that a consideration of outrageous hypotheses in geomorphology may reveal more about the reasoning process than will conventional hypotheses of the type that are often rhetorically involved in published papers to provide the appearance of objective scientific methodology. This point will be developed in the next section of this chapter.

Like Feyerabend, C.S. Peirce is also critical of positivists and their logical empiricist successors for arbitrarily separating the context of discovery from the context of justification. Unlike Feyerabend, however, Peirce argued for a rationalist approach, but one in which the human mind is instinctively attuned to nature. Peirce supported his position, much as Whewell had before him, through extensive reference to the history of science. His triadic view of the sign relationship derived from a triadic phenomenology that underpins much of his philosophy. This distinguishes his view of scientific explanation from that commonly assumed in analytical philosophy of science. The prevailing nominalistic, dualistic approach to scientific explanation employs the 'deductive-nomological' model in which explanatory arguments subsume some resulting state of affairs (called the 'explanandum') under some covering laws (called 'explanans'), which act as the controlling state of affairs (Hempel 1966). The perusal of most scientific papers will confirm to their readers that this mode of logic prevails in published scientific explanation. On the other hand, Sir Peter Medawar (1991) argued that scientific papers were nearly all fraudulent. This rather forceful statement derives from the fact that it is a rare paper indeed that conveys the true process by which the science is actually done. Scientific papers provide dualistic models of scientific explanation because they leave out the third element, involving discovery of causative aspects of reality, that was essential to the whole scientific process. Describing the commonsense, acritical logic of scientific discovery is not a socially acceptable mode for presentation in 'scientific' papers.

Peirce (in Hartshorne and Weiss, 1931, p. 36) expresses his triadic notion of explanation by reference to the syllogistic model, noted above:

> ... An explanation is a syllogism of which the major premise, or rule, is a known law or rule of nature, or other general truth; the minor premise, or case, is the hypothesis or retroductive conclusion, and the conclusion, or result, is the observed (or otherwise established) fact.

This approach to science may have influenced the major writers on late nineteenth century method of geomorphology, particularly G.K. Gilbert, T.C. Chamberlin, and WM. Davis (Baker in press). In relation to the deductive-nomological model, noted above, the explanations would have two parts: laws of nature (the major premise) and statements about causative controlling states of affairs (Peirce's 'hypotheses' or retroductive conclusions as the minor premise). This is how von Engelhardt and Zimmermann (1988) introduce Peirce's logic as a major element in Earth science reasoning (though they fail to develop the realism/nominalism issue in regard to this reasoning). It is also interesting to note that retroductive inference, long ignored by analytical philosophers of science, has now emerged as one of the 'hottest topics' in artificial intelligence (Peng and Reggia 1990) and cognitive science research (Josephson and Josephson 1994).

Before providing examples of geomorphological hypothesizing it is important to be clear on Peirce's meaning for retroduction, which he also called abduction. This inference of creative discovery is often confused with induction, so Peirce's distinction of the two is important (in Burks 1958, pp. 136-137):

> ... Nothing has so much contributed to present chaotic or erroneous ideas of the logic of science as failure to distinguish the essentially different characters of different elements of scientific reasoning; and one of the worst of these confusions, as well as one of the commonest, consists in regarding abduction and induction taken together (often mixed also with deduction) as a simple argument. Abduction and induction have, to be sure, this common feature, that both lead to the acceptance of a hypothesis because observed facts are such as would necessarily or probably result as consequences of that hypothesis. But for all that, they are the opposite poles of reason ... The method of either is the very reverse of the other's. Abduction makes its start from the facts, without, at the outset, having any particular theory in view, though it is motived by the feeling that a theory is needed to explain the surprising facts. Induction makes its start from a hypothesis which seems to recommend itself, without at the outset having any particular facts in view, though it feels the need of facts to support the theory. Abduction seeks a theory. Induction seeks for facts. In abduction the consideration of the facts suggests the hypothesis. In induction the study of the hypothesis suggests the experiments which bring to light the very facts to which the hypothesis had pointed. The mode of suggestion by which, in abduction, the facts suggest the hypothesis is by *resemblance,* - the resemblance of the facts to the consequences of the hypothesis. The mode of suggestion by which in induction the hypothesis suggests the facts is by *contiguity,* - the familiar knowledge that the conditions of the hypothesis can be realized in certain experimental ways.

The construction of hypotheses is a matter of scientific attitude. It does not derive from some body of theory, nor does it follow some rigorous method. However, Peirce was nearly unique among philosophers in noting that a method could be discerned in hindsight. His use of the word 'abductive' was a recognition of Aristotle's use of that word in the *Organon*, though Peirce believed a mistranslation of the original document confused logicians on this point for all later history. His description continues (in Burks 1958, pp. 137-138) as follows:

> ... that the matter of no new truth can come from induction or from deduction, we have seen. It can only come from abduction; and abduction is, after all, nothing but guessing. We are therefore bound to hope that, although the possible explanations of our facts may be strictly innumerable, yet our mind will be able, in some finite number of guesses, to guess the sole true explanation of them. That we are bound to assume, independently of any evidence that it is true. Animated by that hope, we are to proceed to the construction of a hypothesis.
>
> Now the only way to discover the principles upon which anything ought to be constructed is to consider what is to be done with the constructed thing after it is constructed. That which is to be done with the hypothesis is to trace out its consequences by deduction, to compare them with results of experiment by induction, and to discard the hypothesis, and try another, as soon as the first has been refuted; as it presumably will be.

The last sentence of this quote describes the essence of scientific method according to Peirce. The goal of discarding hypotheses would seem to be a severe requirement. Consider the dilemma for modern computer modelers. If we hold that models are really hypotheses (Baker 1985), then the task is not so much to make a beautifully elegant theoretical model. Rather, it is to falsify models. This is abuse indeed of our theoretical constructs!

Peirce (in Burks 1958, p. 138) goes on to list the considerations that determine the choice of a hypothesis:

> ... In the first place, it must be capable of being subjected to experimental testing. It must consist of experiential consequences with only so much logical cement as is needed to render them rational. In the second place, the hypothesis must be such that it will explain the surprising facts we have before us which it is the whole motive of our inquiry to rationalize.... In the third place, quite as necessary a consideration as either of those I have mentioned, in view of the fact that the true hypothesis is only one out of innumerable possible false ones, in view, too, of the enormous expensiveness of experimentation in money, time, energy, and thought, is the consideration of economy.

Peirce (1902b, pp. 761-672) also notes that the testing process involves some special considerations in regard to hypothesis choice:

> ... It is desirable to understand by a verifiable hypothesis one which presents an abundance of necessary consequences open to experimental tests, and which involves no more than is necessary to furnish a source of those consequences. The verification will not consist in searching the facts in order to find features that accord or disagree with the hypothesis. That is to no purpose whatsoever. The verification, on the contrary, must consist in basing upon the hypothesis predictions as to the results of experiments, especially those of such predictions as appear to be otherwise least likely to be true, and in instituting experiments in order to ascertain whether they will be true or not.

Thus, it is the surprising results of experiments, surprising except in terms of the hypothesis being tested, that are most important to the verification process. While experiments need not be elaborate or many, Peirce holds it of great importance that they be independent. This means that their results should be capable of explanation only through the hypothesis under investigation and not through some other hypothesis. To achieve this seemingly difficult independence of experiments Peirce directed attention to his famous 'economy of research' (Peirce 1879; Cushen 1967).

## REALITY IN OUTRAGEOUS HYPOTHESES

As stated thus far, geomorphology is considered to be a way of thinking about Earth's surface and its processes, involving signs or indices of those real-world phenomena for which probable causes and/or explanatory concepts (that are real in Peirce's sense of the word) are inferred (through Peirce's 'retroduction') in such a way as to fit formerly known but seemingly unrelated phenomena (Whewell's 'colligation of facts'). Though superficially similar in appearance to lucky guessing, this retroductive process leads uncannily to hypotheses that display the repeated exhibition of explanatory surprise in regard to natural observation of experiments (Whewell's 'consilience of inductions'). The claim has been made that this involves the 'logic' of geomorphology as science. I now want to describe some important examples of geomorphological thinking to illustrate the process. I wish my examples to involve scientific discovery, since that is important to the advancement of science, though hypotheses also obviously aid in the study of what is already known. Kantorovich (1993) notes that discovery can occur either by exposure or by generation. In exposure, some new phenomenon is observed, or information hidden in a set of statements is revealed (as in prediction from a new theory). Hypotheses are most important in discovery by generation, in which new theoretical concepts are developed. New theoretical concepts must replace old ones, but presumably the old concepts are adhered to for good reasons. How does one stimulate progress, getting past what most of the scientific community considers to be good working principles at some moment in time?

Late in life William Morris Davis reflected on this problem (Davis 1926, p. 464):

> ... Are we not in danger of reaching a stage of theoretical stagnation, similar to that of physics a generation ago, when its whole realm appeared to have been explored? We shall be indeed fortunate if geology is so marvelously enlarged in the next thirty years as physics has been in the last thirty. But to make such progress, violence must be done to many of our accepted principles; and it is here that the value of outrageous hypotheses, of which I wish to speak, appears. For inasmuch as the great advances of physics in recent years and as the great advances of geology in the past have been made by outraging in one way or another a body of preconceived opinions, we may be pretty sure that the advances yet to be made in geology will be at first regarded as outrages upon the accumulated convictions of to-day, which we are too prone to regard as geologically sacred.

Davis was sufficiently motivated by this view that, against the judgment of most of his contemporaries, he even proposed a serious contemplation of 'the Wegener outrage of wandering continents'. Wegener's hypothesis was well known to have so many flaws that it was considered untenable by many geologists in the 1920s. The flaws included the physical process whereby it operated and many errors of geological detail that were advanced by its meteorologist-inventor. However, this hypothesis also colligated numerous otherwise inexplicable facts, including the 'fit' of the continents and the distribution of Permian glacial deposits of the Southern Hemisphere. The problems were eventually resolved through the concepts of sea-floor spreading and plate tectonics, the latter being arguably the most successful conceptual scheme to have emerged in the Earth sciences (LeGrand 1988; Menard 1986; Stewart 1990). Wegener's concept was clearly a lucky guess inspired by an array of facts. It was motivated by the need for a theory to explain otherwise unrelated facts, but it flew in the face of existing theory.

As Davis's 1926 paper on 'the value of outrageous geological hypotheses' was being published, another outrage was being perpetrated by J Harlen Bretz, who had hypothesized a catastrophic flood origin for the Channeled Scabland region of the northwestern United States (Bretz, 1923, 1928). Bretz challenged a prevailing uniformitarian view that extraordinary causes, unlike those in operation today, should be precluded from hypothetical consideration. This view derived from the misconception that uniformitarianism constituted a fundamental principle of nature rather than a regulative guide to fallible human reasoning about nature. More will be said about such regulative principles in relation to hypothesizing in a subsequent section of this chapter. It is important here to note that the rationale for serious consideration of cataclysmic flooding as an explanation was based solely on field relationships on the Columbia Plateau (Baker 1978, 1995). Arguments against it were partly regulative and partly theoretical in that it was presumed that such immense flood flows were not physically reasonable and that there was no reasonable source for the vast quantities of water required (Baker and Bunker 1985). Once the field relationships were generally accepted by the scientific community, however, the debate was followed in short order by the demonstrated physical consistency of dynamical cataclysmic flood processes with scabland landforms and sedimentary sequences (Baker 1973a, b).

The reality of the Wegener and Bretz outrageous hypotheses seems to be clear in hindsight. How does one view such hypothesizing as it is unfolding? This may be a more relevant example in terms of geomorphological reasoning in relation to the continuity of thought envisioned by Charles Peirce. My candidate for a presently developing outrageous geomorphological hypothesis, true to the Davis (1926) definition, is the proposal by John Shaw (1994) that a very interesting assemblage of late Pleistocene glacial erosional and depositional features, including tunnel channels, Rogen moraine, and drumlins may be indicative of massive subglacial flooding. It has long been known that a variety of landforms, involving both erosion and deposition, form beneath thick continental glaciers. These landforms have generally been explained by various subglacial deformation processes. However, modern glaciers do not provide satisfactory analogs to the ancient ice sheets that produced these landforms. In Shaw's hypothesis catastrophic subglacial flooding has been posed to explain the spatial and temporal associations of these features. Large-scale, subglacial meltwater floods are inferred to be responsible for certain drumlins (Shaw 1983; Shaw and Sharpe 1987), tunnel channels (Brennand and Shaw 1994), bedrock erosional marks (Shaw 1994), and Rogen moraine (Fisher and Shaw 1992).

The problem of subglacial cataclysmic flooding provides an excellent example of the methodological/philosophical quandaries faced in geomorphology and geology. There are no modern process equivalents to the gigantic late Pleistocene warm-based ice sheets of

North America and Eurasia. There is no possibility of controlled experiments scaled to the dynamics of such ice sheets. Nevertheless, there is a detailed assemblage of landforms and sediments (Shaw, 1994) for which we have no modern analogs. How is science to be done in such circumstances? Theories can predict the landforms by deduction, but which theories apply? Effects can be classified for extrapolation to theoretical generalization, but such verification of theory is logically flawed (Popper 1959). The solution is found in a process of generating hypotheses, as classically argued in geology (Baker 1988b). The goal is to infer cause from effect, or 'consequent from antecedent' as Gilbert (1886) so aptly described it. The logic of this reasoning is neither deductive nor inductive. It is retroductive or abductive (Baker in press), a reasoning central to geology (von Engelhardt and Zimmermann 1988) but greatly misunderstood by the advocates of deductive/ inductive approaches in science, who use the exemplar of experimental/theoretical physics to justify their arguments.

## THE LOGIC OF HYPOTHESES IN THE SCIENTIFIC PROCESS

If hypotheses violate the usual conventions of science, why should they be pursued? Presumably such hypotheses would have a very low probability of confirmation against future experimental tests. There are an immense number of possible outrageous hypotheses. If hypotheses are merely convenient fictions, and do not have some connection to reality, why do outrageous hypotheses, like those described in the previous section, prove very fruitful for further scientific investigation? One might think that there is some probability of the hypothesis being true that recommends itself to the hypothesizer. Tables for hypothesis probability have even been suggested (Strahler 1987).

Peirce's view on the probability of hypotheses is consistent with his view of retroduction as a unique process of discovery, and not something subject to a frequency of confirmation or disconfirmation. A retroductive hypothesis is accepted through a directed scientific process of inquiry involving a great complex of evidence. The process is not the trivial logical comparison of some statement of cases for true/false testing according to a nominalistic logic. This is simply not what scientists do; rather, it is something idealized by philosophers inexperienced in actual scientific practice. As Peirce stated it (in Hartshorne and Weiss 1931, p. 78): 'It is nonsense to talk of the probability of a law, as if we could pick universes out of a grab-bag and find in what proportion of them the law held good.' Peirce's frequentist views of probability were even extended to inductive scientific inference, a view that would be quite controversial to modern philosophers of probability.

Peirce's emphasis on how scientists actually reason can be reconciled with the view that logic is a normative science, referring to how reasoning ought to be done. For Peirce this normative function is divided between a formulated, scientific, and critical logic, which he called the *logica docens,* and an implicit and acritical logic, the *logica utens,* that is instinctive and part of the commonsense reasoning of the inquirer. Peirce believed that the *logica utens* is antecedent to systematic reasoning. Because the instinctive *logica utens* does not serve on all occasions, one must embark upon the *logica docens.* As will be seen below, Peirce hypothesized that the *logica utens* has a very important function in science, allowing the scientist to 'guess right' in the formulation of hypotheses. Guessing right does not mean that one derives the absolutely correct answer by retroduction. It does mean that one reasons to a hypothesis that is fruitful and productive on the path of scientific inquiry. Because it involves reason, Peirce considers these issues to be matters of logical concern. This is the sense in which he views the logic of scientific discovery. Although a few modern analytical philosophers, notably Hanson (1958), tried to argue some aspects of Peirce's logical position, they have generally not accepted his instinctive basis of the *logica utens.* Most philosophical opinion denies that a logic of discovery is possible (Kantorovich 1993), but this opinion generally denies this from the position of nominalistic *logica docens.*

It is interesting that when Peirce considered the eventual testing of hypotheses, not to be confused with the retroductive phase of inquiry, his view came to a position remarkably similar to that espoused many years later by Sir Karl Popper. As published by Hartshorne and Weiss (1931, p. 48), Peirce's view is as follows:

> It is a great mistake to suppose that the mind of the active scientist is filled with propositions which, if not proved beyond all reasonable cavil, are at least extremely probable. On the contrary, he entertains hypotheses which are almost wildly incredible, and treats them with respect for the time being. Why does he do this? Simply because any scientific proposition whatever is always liable to be refuted and dropped at short notice. A hypothesis is something which looks as if it might be true and were true, and which is capable of verification or refutation by comparison with facts. The best hypothesis, in the sense of the one most recommending itself to the inquirer, is the one which can be the most readily refuted if it is false. This far outweighs the trifling merit of being likely. For after all, what is a *likely* hypothesis? It is one which falls in with our preconceived ideas. But these may be wrong. Their errors are just what the scientific man is out gunning for more particularly. But if a hypothesis can quickly and easily be cleared away so as to go toward leaving the field free for the main struggle, this is an immense advantage.

The issue here is one of hypothesis selection. Reasoning to a best hypothesis must involve the 'one most recommending itself to the inquirer'. If we view this selection process purely objectively, in which there is not necessarily any immediate connection to nature's reality (though testing may yet reveal one), then one can come to the position that the creative process might well generate an unlimited number of explanatory hypotheses. This multiple generation process is quite valueless. The valuable activity is selection of the most fruitful combinations of explanatory possibilities. The mathematical physicist Henri Poincare argued that such selection occurs according to some inner sense of the inquirer, such as the 'mathematical beauty' or 'symmetry' concepts that are often described in the published reminiscences of famous mathematicians and physicists. This inner sense may be combined with a psychological interplay between the conscious and subconscious. This psychological process involves a period of deep and intense thought which fails to solve an especially difficult problem. The scientist then pursues some activity totally unrelated to the problem, usually something leisurely that involves little thought. During this activity a flash of illumination strikes the mind, resolving the original problem. Poincare (1914) provides a striking example of this in an anecdote about his inability to resolve a mathematical problem. The solution came to him while engaged in a geological excursion!

The selection process for mathematical discovery involves reasoning in symbols that are the most formal and detached from natural connections of any in science. When complex signs, such as the indices relating to real causes in nature, enter the reasoning process, it is not likely that the creative process is exactly the same as used by a mathematical genius. Presumably there are selection criteria available to any scientist that will apply in hypothesis selection.

### Parsimony and Simplicity

The two regulative principles generally invoked for Earth science hypothesizing are uniformitarianism and evolutionism (von Engelhardt and Zimmermann 1988). Evolutionism will receive some consideration under the topic of naturalism. The concern here will begin with uniformitarianism, one of the few Earth science issues to have received appreciable philosophical attention. Like all regulative principles, uniformitarianism has a methodological, or 'weak' form that specifies procedures for reasoning about the Earth, especially hypothesizing. It also has a substantive, or 'strong' form that makes ontological claims about how the Earth actually behaves. Though long recognized (Gould 1965, 1987), the various types of uniformitarianism continue to cause confusion for practicing Earth scientists (Shea 1982). The Channeled Scabland controversy involved a mistaken application of substantive uniformitarianism by those who criticized Bretz for advocating catastrophism (Baker 1981). Ironically Bretz's hypothesis was formulated according to a regulative rule of simplicity (Baker 1987), which is now recognized as the methodological manifestation of uniformitarianism (Goodman 1967; Gould 1987).

The principle of simplicity (methodological uniformitarianism) serves as a means for promoting hypothesis selection from among multiple causal possibilities. This has logical status, as promoted by the famous nominalist medieval logician William of Ockham. The principle may be stated in modern terms as follows (Jeffreys and Berger 1992, p. 64): Among competing hypotheses favor the simplest one. This is also known as 'Ockham's razor' or the 'principle of scientific parsimony'. If logic is totally divorced from connection to real entities, as nominalists presume, then this principle can be globally applied to adjudicating between competing hypotheses, no matter what the causes to which those hypotheses refer. However, the alternative, that the presumption of simplicity implies something real about the nature of the world, does not have to be as drastic as substantive uniformitarian claims that catastrophic processes are precluded. Such an ontological claim can be as simple as our supposition that the laws of mechanisms apply both on Earth and at the orbit of Jupiter. While the success of spacecraft launches attests to this claim, it does not prove it. On the other hand, if miracles can occur, allowing the laws of mechanics to be different at different points in the solar system, a great deal of science will be precluded.

The full argument that simplicity may imply some subtle ontological claims is beyond the scope of this chapter. Sober (1988) provides a good summary of these arguments in relation to issues in evolutionary biology. The key point I want to make here is that regulative principles are not necessarily purely methodological. Arguments that they are so may be developed in the crisp logic of deduction. They are far more difficult to make for the logic of induction, to which David Hume applied an early formulation of uniformity of nature: the presumption that the future (or past) will be like the present. If simplicity or parsimony applies to inductive inference, what principle applies to retroduction, the notion of reasoning to a best hypothesis?

## The Light of Nature

Charles Peirce came to a remarkable conclusion about hypothesis selection. Moreover, if we agree with Peirce that hypothesizing through retroduction is the whole basis of a natural science like geomorphology, then this conclusion also applies to the basis of that science. Here is the statement as Peirce presented it in his 1898 lectures (Ketner 1992, pp. 176-177):

> The only end of science, as such, is to learn the lesson that the universe has to teach it. In Induction it simply surrenders itself to the force of facts. But it finds, at once, - I am partly inverting the historical order in order to state the process in its logical order, - it finds I say that this is not enough. It is driven in desperation to call upon its inward sympathy with nature, its instinct for aid, just as we find Galileo at the dawn of modern science making his appeal to *il lume naturale.* But insofar as it does this, the solid ground of fact fails it. It feels from that moment that its position is only provisional. It must then find confirmations or else shift its footing. Even if it does find confirmations, they are only partial. It still is not standing upon the bedrock of fact. It is walking upon a bog, and can only say, this ground seems to hold for the present. Here I will stay till it begins to give way. Moreover, in all its progress science vaguely feels that it is only learning a lesson. The value of *Facts to it,* lies only in this, that they belong to Nature; and Nature is something great, and beautiful, and sacred, and eternal, and real, - the object of its worship and its aspiration.

Note that Peirce's metaphor of the bog is exactly the same as that quoted by Sir Karl Popper at the beginning of this chapter. The metaphysics of science, accounting for its foundations, is immensely difficult in philosophical terms. What has been remarkable, however, and a great puzzle to philosophers (Gjertsen 1989), is that scientists are able to be amazingly successful in explaining nature without ever resolving these foundational difficulties. This is what is revealed so well in historical study of scientific inquiry.

Peirce was greatly influenced by his historical study of Galileo. He particularly noted Galileo's appeal to the light of nature (*il lume naturale)* as the means of retroductive success, or guessing right (Eisele 1979). Peirce (1891, p. 165) writes:

> A modern physicist on examining Galileo's works is surprised to find how little experiment had to do with the establishment of the foundations of mechanics. His principal appeal is to common sense and *il lume naturale.* He always assumes that the true theory will be found to be a simple and natural one.

Peirce also found that the appeal to nature was often confused with the notion of simplicity. As a logician Peirce must have been interested in this confusion, as he had been in Newton's use of the word 'hypothesis'. Peirce (1908, p. 104) wrote the following description of simplicity as a criterion for hypothesis selection:

> Modern science has been builded after the model of Galileo, who founded it on *il lume naturale.* That truly inspired prophet had said that, of two hypotheses, the *simpler* is to be preferred; but I was formerly one of those who, in our dull self-conceit fancying ourselves

> more sly than he, twisted the maxim to mean the logically simpler, the one that adds the least to what has been observed, in spite of three obvious objections: first, that so there was no support for any hypothesis; secondly, that by the same token we ought to content ourselves with simply formulating the special observations actually made; and thirdly, that every advance of science that farther opens the truth to our view discloses a world of unexpected complications. It was not until long experience forced me to realise that subsequent discoveries were every time showing I had been wrong, while those who understood the maxim as Galileo had done, early unlocked the secret, that the scales fell from my eyes and my mind awoke to the broad and flaming daylight that it is the simpler Hypothesis in the sense of the more facile and natural, the one that instinct suggests, that must be preferred; for the reason that unless man have a natural bent in accordance with nature's, he has no chance of understanding nature at all ... I do not mean that logical simplicity is a consideration of no value at all, but only that its value is badly secondary to that of simplicity in the other sense.

This tendency of the human mind to 'have a natural bent in accord with nature's' is an unusual form of naturalism, much as Peirce's realism is not the usual form of that doctrine. Peirce's ascribing of a major role of Galileo's scientific reasoning to *il lume naturale* (the light of nature) is a very striking statement. Indeed, an anonymous reviewer of an early version of this essay (clearly a philosopher and historian of science) wrote:

> I know Galileo's writings fairly well, along with a good bit of the scholarly literature. I cannot think of any place where such a notion appears, let alone plays a prominent role. A review of several relevant texts has not provided me with any evidence that such a notion played an important role in Galileo's understanding of science.

The review statement is reminiscent of the disbelief some modern historians of science accorded Galileo's writings in which he commonly invokes reasoning *ex suppositione* (hypothesizing from effects to causes). Galileo's logic in such writings seems to come from his interest in Aristotelian and Thomistic writings, rejecting those of Plato and various nominalists, such as William of Ockham. Wisan (1978, p. 47) expressed her disbelief as follows: '... today, of course, everyone knows that one cannot argue rigorously from effects to causes... '.

Of course, Galileo, like Peirce, was not interested in *ex suppositione* reasoning for its value in rigorous argument. He was interested in discovering new explanations, and his remarkable success in that endeavor led Peirce to take Galileo's methodological writings very seriously. McMullin (1978) also recognized the retroductive character of Galileo's inferences, and Wallace (1981) extensively explored the logical basis of his thought in the Aristotelian tradition.

The phrase *il lume naturale* appears prominently in Galileo's most mature work, *Discourse Upon Two New Sciences*, published in 1638. Drake's (1974) well-known English version gives the bizarre translation 'my good sense', but Drake (1974, p. 162) places the original in parentheses, thereby highlighting its unusual invocation. Wallace (1992a) recently traced the source directly, and his translation of Galileo's logical treatises (Wallace 1992b) contains the multiple references to *il lume naturale*. Wallace (1992a) even related how and why Galileo's logical treatises, written early in his career, were ignored and not even translated by modern scholars. Of course, Charles Peirce, who explored the history of logic for planned but uncompleted books, would have been desperately interested in all of Galileo's logical pronouncements. He recognized their importance long before modern scholars did. This failure of modern scholars, until very recently, to understand the Aristotelian realism of Galileo's logic provides a very instructive example of how modern beliefs impact historical inquiry. Peirce would probably have attributed the oversight to the blinding nominalism of the scientific reasoning process presumed by most philosophers and historians of science.

## NATURALISM AND HYPOTHESES

Naturalism is a philosophical position that holds scientific explanations to arise through methods that are continuous from the natural domain of objects and events. Naturalism has become important in various evolutionary epistemologies, such as David Hull's (1988) modeling of the development of science on the natural selection process. Kantorovich (1993) expands Hull's descriptive thesis into a full explanatory theory of science based on naturalistic evolutionary criteria.

The concern here will be more limited, confined to finding the natural basis for *il lume naturale,* the instinct that Peirce claims to provide the basis for retroduction ('guessing right'). Peirce's naturalism has been labeled 'ecstatic' by Corrington (1993). Particularly in his later writings Peirce sees in nature not so much something to be described in an objective sense, as the reductionist program of some scientists would have it, but rather an entity with which the human mind has an affinity that leads to self-understanding. Peirce's emotional arguments in this regard were much criticized during the heyday of logical positivism (Buehler 1939; Goudge 1950). However, the utter purposelessness implied by a reductive scientific program has recently produced a new philosophical debate about science and human values. On one hand, there are critics of science's cold austerity and objectivity who claim that these qualities in our modern technological world are destroying humankind's aesthetic sense (Appleyard 1992). On the other hand, certain scientists are espousing a scientism that denies the commonsense origins of knowledge (Cromer 1993) and glorifies the unnatural nature of science (Wolpert 1992). Much of this polarization centers around the view of science as objective knowledge, theories, and facts about the world, rather than science as a human process, naturally attuned to nature's inspiration. The natural connection in science may be more effectively conveyed by considering the origin of hypotheses, rather than the explanatory power of scientific 'laws'. Sciences of natural indices, like geomorphology, may be more appropriate for its exposition than sciences of detached symbols, like mathematical physics.

Charles S. Peirce made the astonishing proposal that the human mind is naturally adapted to 'guess right', that is, to form highly probable causal hypotheses based on the experience of mental interaction with the world. This mental interaction involves relating to the world in a system of signs. Of course, more sophisticated interactions take place through language, which is itself a system of signs. The role of language is so important that some constructivist philosophers argue that scientific reasoning is necessarily embedded in language. A corollary to this view is the relativist position that the language world of every person precludes any truly objective access to reality. Peirce would vehemently disagree with many current relativist, idealist views of science. He strongly supported the experimental approach in which inductive measurement is matched against deduced consequences of theories. Much more importantly, however, Peirce held that reality influences causal hypothesizing.

The idea of an instinctive basis for a type of inference (retroduction) would be denied by behavioral psychologists, such as Pavlov and Skinner, who argued that young children and animals are conditioned in their responses to the world. Repeated associations of phenomena may give some appearance of causation to sophisticated human adults, as the philosopher David Hume argued, but the ability to infer is presumed to be a later development. Animals and young children, according to the behavioralists, are incapable of hypothetical inferences as Peirce envisioned them. In contrast, however, recent work in cognitive psychology is more consistent with Peirce's naturalistic view of human thought. In experiments with very young infants, Leslie (1982) and Leslie and Keeble (1987) demonstrate that even children at age four-and-a-half months perceive interactions between objects in causal terms. This understanding of causality proceeds in parallel to the acquisition of language skills, and is very advanced before children learn to speak. The understanding of causality is developed to a high degree in play and in pretending.

Even animals may have a natural reasoning instinct, contrary to behavioralist assumptions (Dickinson 1980). In a remarkable set of experiments on laboratory rats, Holland and Straub (1979) show that rats can apply a logic of inference that links two sets of associations to infer a third relationship. While this may fall short of strict causality, it is much more sophisticated than the simple associative reasoning envisioned by Pavlov. Cheney and Seyfarth (1990) document even more sophisticated hypothetical inference by vervet monkeys. Their experiments document true 'if…, then...' inferences by these animals. The implications of these studies are that the human mind has evolved to the present ability to conduct scientific inquiry, and that this process of inquiry is strongly rooted in nature itself. This hypothesis has the merit of explaining a remarkable paradox: How it is that science has been so successful in explaining the natural world. Could its success derive from it being the only method that has emerged (evolved?) from that world?

Socioevolutionary theories of science are currently popular in philosophy of science. For example, Kantorovich (1993) argues that the growth of science is analogous to an evolutionary process in which there is a coevolution of human action (analogous to the evolution of sensorimotor organs) and human understanding (analogous to the brain). He adds to this a shock effect caused by unintended or serendipitous discoveries. One might well extend this evolutionism to Peirce's problem of *il lume naturale.* A candidate for this is gene-cultural evolution, the sociobiological thesis that culture is elaborated under the influence of hereditary learning propensities, while the genes prescribing the propensities are spread in a cultural context (Lumsden and Wilson 1985). An example is the sociobiological argument for an instinctive human fear of snakes. Wilson (1994) notes that phobias are rarely acquired for the really dangerous items of modern life, including guns and speeding automobiles. These have not threatened the human species long enough for acquisition of predisposing genes that will ensure avoidance. Snakes, however, have a long history of close association with humans with likely adverse consequences. Phobia as a means of snake avoidance may have conveyed survival advantages, and gene-based evolution is at least a plausible explanation for the development of that phobia.

Presumably the survival value of 'guessing right' is at least as good a candidate for sociobiological explanation as snake phobia. On the other hand, I doubt that the extreme reductionism of the sociobiologists would have been very appealing to Peirce, who entertained a very different metaphysical view of evolution than these modern treatments.

An alternative to the evolutionary model has been offered by Thomas Nagel (1986), who treats the problem as one of self-understanding. As he defines this (Nagel 1986, p. 78), self-understanding is '… an explanation of the possibility of objective knowledge of the real world which is itself an instance of objective knowledge of that world and our relation to it'. Nagel is critical of any evolutionary explanation, which he considers (Nagel 1986, p. 78) to be '… an example of the pervasive and reductive naturalism of our culture'. He notes that Darwinian natural selection explains selection among generated organic possibilities, but that it does not explain the possibilities themselves. As he puts it (Nagel 1986, p. 78), 'It may explain why creatures with vision or reason will survive, but it does not explain how vision or reason are possible.'

Nagel's language is reminiscent of Peirce in how he marvels at the abilities of human beings to understand the world. He comes to the conclusion that scientific theorizing and its success must involve some essential or fundamental quality of the universe. Evolution might be a method of bringing this forth, but its potentiality must already exist (Nagel 1986, p. 81):

> I don't know what an explanation might be like either of the possibility of objective theorizing or of the actual biological development of creatures capable of it. My sense is that it is antecedently so improbable that the only possible explanation must be that it is in some way necessary. It is not the kind of thing that could be either a brute fact or an accident ... the universe must have fundamental properties that inevitably give rise through physical and biological evolution to complex organisms capable of generating theories about themselves and it.

Nagel's concern with the possibilities of self-understanding is quite analogous to Peirce's concern with the possibility of scientific success through retroduction. Peirce's metaphysics, which is mostly beyond the scope of this chapter, envisions a phenomenology of pure categories that provide the structure through which all perceptions and experiences come to be understood. The first of these categories involves the individual being of things and includes a kind of chance. The chance of this Firstness category is not entirely uncaused and irregular. Instead, it is a spontaneity that is some degree regular and reflects a kind of reason. This is also an absolute chance in which the universe finds beginning; Peirce called it 'tychism'. Peirce would argue that this tychism is what gives reason its tendency toward better explanation. But reason is also tied together by a continuity that involves connection through the other categories of understanding. This continuity he called 'synechism', and he believed it to be displayed in commonsense reasoning, as in the *logica utens.* Because it connects the relating tendencies (Secondness) and the generalizing or law-establishing tendencies (Thirdness) to pure potentiality for developing laws (Firstness), this synechism is exactly with what the mind is presumed to be attuned in hypothesizing.

The above may well confirm the reader of G.K. Gilbert's description of Peirce in a letter cited by Davis (1927, p. 269): '... a man so metaphysical I should never had tho't of going to him with a practical question'. Nevertheless, it is also fascinating that current biological research into self-organization and complexity holds that a kind of order emerges naturally from the most complex of systems (Kauffman 1995). This order presumably derives from forces that operate right at the edge of chaotic behavior. Is this a mathematical manifestation of Peirce's tychism?

The cosmological implications of these principles also have their counterparts in current physical theories of the universe. Among modern cosmologists there is a split between those who see the universe as totally devoid of purpose and those who see it filled with causal necessity. Typical of the former camp is Weinberg's (1993, p. 154) conclusion, ' ... the more the universe seems comprehensible, the more it seems pointless'. In complete contrast is Dyson's (1979) argument of universal growth in richness and complexity. Wheeler (1983) develops similar arguments from the requirement that observers participate in the development of the universe, a creative process that Barrow and Tipler (1986) have named the 'Participatory Anthropic Principle'.

The Anthropic Cosmological Principle was postulated to deal with the physics of explaining the universe and its origins (Hawking 1988). It holds that the universe can in some ways be explained that it must be such as to contain people. Its weak form is similar to a methodological rule (Carter 1974, p., 291): ' ... what we can expect to observe must be restricted by the conditions necessary for our presence as observers'. This is not controversial. The strong version, however, makes a substantive statement about the universe (Carter 1974, p. 294): '. . . the Universe (and hence the fundamental properties on which it depends) must be such as to admit the creation of observers within it at some stage'.

The tentative conclusion is that the importance of the Anthropic Principle for cosmology (Barrow and Tipler 1986) is paralleled by the importance to science of the principle that our reasoning (particularly retroductive hypothesizing) is a part of the nature we seek to fathom. In its strong form, the ontological version, our minds are attuned to nature in some manner, as hypothesized by Peirce, and worthy of further scientific and philosophical inquiry.

## DISCUSSION AND CONCLUSIONS

Modern philosophers of 'science' have generally used the experimental, conceptual sciences as their ideal for discussing the justification of knowledge. Much current interest is devoted to distinguishing the predominant philosophy of physics from the newly emergent philosophy of biology. The Earth sciences, outside of minor interest in the plate-tectonic 'revolution' and the medieval metaphysics of uniformitarianism, have not figured prominently in philosophy of 'science'.

Controlled experiments, at least for the most interesting phenomena, are not possible in much of the Earth sciences, including geomorphology. Instead of the detached objectivity of sterile laboratories, the geomorphologist must be concerned with the open problem of the field, in which nature is 'taken as it is'. Great geomorphologists of the past, including Chamberlin, Gilbert, and Davis, championed the use of hypotheses for understanding the field problem. However, their concept of hypotheses is very different than that of physicists like Henri Poincare, who viewed them as theoretical instruments. The hypothesis derived from theory alone, while fine for a controlled experimental physics of Earth's surface (geomorphysics?), is not appropriate for a holistic understanding of that surface. Hypotheses must have a connection to the reality that they purport to represent, and this connection must be achieved at their inception, not just through some later correspondence between theoretical prediction and objective measurement. This position was argued effectively a century ago by the famous logician C.S. Peirce, who advanced the notion that our minds are constituted in some way to be continuous in thought with aspects of nature. Peirce's hypothesis about hypothesizing is not in itself a complete explanation for how reasoning in a natural science like geomorphology is both possible and successful. Rather, it is a recognition of real world coincidences that cries out for an explanation. In this regard it is a manifestation of what Whewell termed 'consilience' in science.

The guessing instinct that regulates our scientific hypothesizing does not reveal the truth of things; it merely places the inquirer on a fruitftil path of investigation. That path leads to the deduction of consequences and their experimental testing. However, the nominalistic logic that was developed to be effective in these latter activities may not be a good model for the retroductive reasoning that underpins hypothesizing. If we agree that scientific reasoning is a continuous process of inference connecting the real world and concepts of that world, one must be careful with logical tools like Ockham's razor. J Harlen Bretz expressed the commonsense methodology of geomorphologists aptly in a 1978 communication to me: 'I have always used Chamberlin's method of multiple working hypotheses. I applied Ockham's razor to select the most appropriate hypothesis, but always with due regard for possible dull places in the tool.'

What would a science be like that consisted only of theory, or systems of laws about the physical world, plus stark facts about the world's reality? Some analytical philosophies of science seem to imply that these are the sole elements of science conducted objectively within individual scientific minds. The sole scientific communication with nature consists of tests against the real world, and these well-tested theories build up an objective base for coping with the world. There are examples of human beings that function in much this way. These individuals operate within individual minds and are unable to view nature from nature's point of view, nor are they able to see the world from the points of view of other people. These people are able to meet their individual needs and function in realistic terms, but their relation to the collective human mind is severely handicapped. Perhaps the analogy to this tragic human condition, known as autism, should not be stretched too far. The warning it implies to analytical philosophers of science is that science has human, social, and naturalistic elements, if only because it is conducted by human beings. Denial of these elements may be as unhealthy to the human enterprise of science as such denial is to the mental health of individual humans.

Much as Peirce's views were criticized by philosophers earlier in this century, one can argue that the proponents of naturalism and realism in the hypothetical reasoning underpinning geomorphological inference (Gilbert 1886, 1896; Chamberlin 1890; Davis 1926) were philosophically naive (Kitts 1980). Of course, the analytical philosophy of which these geomorphologists were innocent had largely not been formulated when they wrote their seminal papers. Another explanation is that the late nineteenth century geomorphologists were operating in a different philosophical tradition. Frodeman (1995) recently proposed that this tradition may have elements which we can recognize in modern continental philosophy. Alternatively, as argued here, the tradition may relate to positions espoused by William Whewell and especially by Charles Peirce, who envisioned all thought as a system of signs, including a continuity to nature that includes an instinctive capability for producing fruitful hypotheses in scientific investigation. Whatever the view, it is clear that the question of the philosophical foundations to geomorphology is not so clear-cut as some would have us believe. Those foundations do lie upon a bog or swamp of metaphysical concerns, as described by both Peirce and Popper. However, just because we have not yet successfully probed the depths of that swamp does not mean that we should ignore its presence. All science relies upon regulative principles and other metaphysical notions. It is precisely because we know so little of them that they should be thoroughly criticized and alternatives proposed that may seem more in accord with our actual practices of reasoning.

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