SCIENTIFIC RESEARCH—
SOME METHODOLOGICAL PROBLEMS:
With special reference to the Earth Sciences[1]

C. R. Twidale[2]

I

Whenever scientists come together in congenial surroundings, as for instance over dinner, or when the port and cigars stage has been reached, it is not uncommon to play the space-or time-capsule game: which five or six men have contributed most to Western science? Whose works would you send to another planet or bury for future generations to rediscover as representing the best, the most crucial, contributions to Western science? Of course, much depends on the interests and backgrounds of the players, but several names are popular choices. Newton, whom Wordsworth described as "Voyaging through strange seas of thought alone"; Darwin, whose grand synthesis forms the framework of all later biological work; Socrates, an adventurer of the mind who rejected as unsatisfactory the then current explanations of nature because they did not tell him how and why, and for that reason transformed philosophy from the study of nature to the study of men's souls and their interactions in society; and Einstein, for his discovery of the equivalence of energy and matter ($E = MC^2$) as well as his perceptive and frank comments concerning the methods of science, all figure prominently in these short lists of distinguished men of science. Copernicus, too, rates highly for persuading us that the earth is not the center of the solar system, much less the universe, as does Aristotle with his encyclopedic knowledge. James Hutton, of whom it was said that "he discovered...time," and who was an essential precursor to Darwin, as well as being the founder of geological science, is another popular selection. Popper is nowadays often mentioned as are such men as Faraday, Galileo, and so forth.

But Immanuel Kant is rarely cited in such company; yet he made one of the supreme contributions, took a giant step, at least as great as that of Copernicus. For Kant, who lived in Berlin some two centuries ago, challenged the then accepted view of truth, which was that truth was correspondence to reality. For Kant this

---

[1] The basis of an address delivered at the Academy of the New Church, Bryn Athyn, Pennsylvania, 4 October, 1983.

[2] Dr. C. R. Twidale is Reader in Geography at the University of Adelaide, South Australia 5001.

proposition was a gross oversimplification. He was aware that truth is reality as observed and interpreted by human beings, and that there is in our view of reality a crucial contribution from the thinking mind. Different people see objects in different ways, interpret them according to their training and experience, and moreover interpret or explain the same data in a variety of ways, most of them plausible and logical, but nevertheless different. Observation is important, but it is only a first step preceding interpretation and explanation of a reality that is partly mind-dependent.

Moreover, it is not simply a matter of interpretation. It is not like different conductors giving varied interpretations of a piece of music within the limits indicated by the composer's instructions embodied in the score. It is more a matter of seeing the same feature from entirely different points of view. We have different experiences, different trainings. As H. H. Read has said of geologists, "the interpretation of field work is often a personal affair, depending on an observer's character, training and experience." For many of us a man in a black gown and short grey wig is a lawyer, a man with reversed collar a man of religion, but to a native of Timbuctoo they are merely men in curious clothes. We also think differently. There is more than one logic. The human mind is capable of incredible subtlety, insight, inspiration, and imagination; and even in such supposedly detached and impersonal areas as science, it is also capable of neglect and duplicity in their various guises. It is these various inputs of the human mind—the implications of Kant's great insight and perception concerning the effects of human intellect in our view of the natural world—that are discussed here. For science is practiced by humans, and the results of scientific investigation reflect both the noble and the less praiseworthy qualities of the mind.

II

Until about 1600 AD explanations of natural phenomena were made in terms of religion, and particularly in terms of Biblical creation. But since that time men have come more and more to attempt explanations in terms of logical inferences based in observations, and to find explanations that are subject to testing, rather than to accept conventional wisdom or dogma. Of course, just as many fundamentalists today sincerely prefer interpretations based in some authority or another rather than in a reason comprehensible to their own minds (and many are disturbingly intolerant of any other viewpoint), just as there are still flat-earthers despite all the evidence to the contrary, so did what many would term the age of enlightenment begin much earlier than the date cited. The great Greek philosophers of classical times endeavored to explain the natural features they saw and came to appreciate, for instance, the rhythm, importance, and reason for the Nile floods: "Egypt is the gift of the Nile." And during the 12th century two men of religion living in western Europe succinctly summarized the different attitudes of the past and the future, between the Dark Ages and the Renaissance with one, Anselm, saying:

> I must believe in order that I may understand,

and the other, Abelard, stating:

> I must understand in order that I may believe.

Though they did not know it at the time, Abelard's was the voice of the future, for the Renaissance with its exploration and expansion of the known earth, its advances in all of the sciences, and above all the liberation of men's minds from the automatic acceptance of authority and the development of a questing after explanation, marked the beginning of the modern age of science. Then, as now, it was tacitly assumed that all phenomena (with perhaps the exception of men's minds and emotions!) are capable of explanation: if a thing exists it must be possible. Determinacy, the feeling that if anything exists it is susceptible of explanation, was the tacit basis for all investigations of the natural world, the area of endeavor known as natural science.

III

There are various definitions of science and of research, in part reflecting different levels of endeavor, but to all the word 'science' carries with it the implication or connotation of objectivity. Beyond that, science has variously been defined as the sum of universal knowledge; as a knowledge of facts, phenomena and proximate causes, gained and verified by exact observation, organized experiment and correct thinking; and "an imaginative adventure of the mind seeking truth in a world of mystery"; and so on—there are many definitions, most of them including questionable words and phrases (what is a fact? what constitutes "correct" thinking?). And there are, of course, several areas of science—social, natural, physical, biological, earth. But all definitions give the impression of the accumulation and organization of data, and many carry with them the suggestion that interpretations are capable of verification; though oddly enough some of the more elegant sciences are short on factual data. Furthermore, recent work, particularly by Karl Popper, suggests that the quality of verification is illusory, for he convincingly argues that what distinguishes science from non-science is that whereas the latter cannot be wrong, the former can be shown to be incorrect or unlikely. Thus, one may not like a painting, a symphony, a sculpture or a play, but one cannot say that it is wrong. A scientific theory, on the other hand, though it cannot be shown conclusively to be correct, is vulnerable to being shown to be incorrect—it can be and almost certainly eventually will be shown to be wrong to a greater or lesser degree. It is not possible to validate or verify hypotheses, only to show that they are incorrect. And there is nothing quite so sad as a splendid, elegant hypothesis rendered untenable by new data.

Again, there are various levels of research, ranging from a careful search or inquiry, a critical investigation, to the search for laws, and so on. Scientists seek general statements, unifying laws, for fundamental research has about it a touch of universality. It need not be law-giving in the sense of being universally applicable, but it is not derived from special cases, though the latter may display in extreme form the evidence on which general statements may be based. It also provides a framework within which exceptions become comprehensible. Fundamental work is also a building block that opens new avenues of investigation. It is also commonly predictive, and is essentially generic in character. It is not enough simply to gather data and to describe (though both are essential); explanation is an essential feature of true research.

IV

How is research accomplished in the natural sciences? It is widely believed that there is a scientific method. In particular it is believed that scientific discovery results or grows from a reasoned, defined and logically accountable process of thought. It is called *induction.* Induction, of which Francis Bacon was a proponent, is said to begin with the collection of all available (or of all relevant) facts—plain, unvarnished, unembroidered, and above all trustworthy, facts. These are built up by an inevitable process of logical thought into general statements that are the laws of nature so beloved of the publicists.

This picture of scientific process owes its popularity and common currency to J. S. Mill, who wrote about scientific methodology toward the end of the last century. Mill was not himself a scientist (or at least not in the sense of being a natural scientist), and it is true that induction is the byword and bible of non-scientists who have analyzed the great discoveries, usually in retrospect. But having said that, it is also true that scientists themselves are not the best witnesses. Rarely do they describe the thought processes by which new concepts were born; and this may be because there are no steps or sequences, that the so-called methods are in reality haphazard, intuitive or even serendipitous. Moreover, when scientists have attempted to record the sequence of events leading to discovery, they describe what they think ought to have been done rather than what was indeed the case. Darwin, for instance, in an autobiographical sketch stated that he worked on true Baconian principles, and without any theory collected facts on a wholesale scale prior to his discovery of natural selection and evolution. But elsewhere, in the letters he wrote to friends and colleagues during his lifetime, it is clear that he could not resist forming hypotheses on every subject and at every stage, and many times betrayed the haphazard route he followed in his great discovery, in contra-distinction to the method he felt showed him in good light as a rational mind working toward a logically inevitable conclusion. As Einstein has written, scientists are to be judged not by their words but by their deeds. It is notable that now that scientists have begun to write of their "methods" there appears to be no method and workers like Feyerabend argue with some force that science is essentially anarchistic.

In considering induction, it may be as well to ask first how it is known what facts are relevant (unless, that is, some surmise has been made as to a likely conclusion). More fundamentally, what is a fact? The word itself is objectionable because it is emotive. It suggests something hard, solid, tangible, firm. In reality it is a jumble of observation, perception, analysis, and interpretation. All facts are conditioned to a greater or lesser extent by the means, the instrument, by which the observation was made, whether it be a machine or the human eye. As Whewell (indisputably the first scientist since he coined the word in 1840) has said: "Facts cannot be observed as facts except in virtue of the conception which the observer himself unconsciously supplies." No observation is objective: it is always made in response to, and is an interpretation in the light of, a problem, a question, or a hypothesis.

Thus take, for instance, the seemingly simple matter of producing or compiling a map showing the distribution of laterite in, say, Australia. It ought to be a matter merely of plotting known occurrences from say 1: 50,000 maps onto a map at a scale of say 1: 5

million or whatever. The problem is that though laterite has been defined, the definition is not universally agreed upon and has furthermore gradually been extended (or ignored) so that what are thought to be truncated latentes or what might be an immature or incomplete laterite, have been plotted as laterite in the sense that the conditions suitable for lateritization obtained in the areas where they are preserved (for in Australia all the laterite appears to be of ancient origin and not to be forming at the present time). Deposits derived from earlier laterites may be included. From that it is a short step to accepting any bleached, kaolinized material, or any iron-stained rock, as a laterite. Thus unless great care is exercised, and indeed unless one has personal knowledge of all occurrences (which no one has), the map includes a wide variety of materials, with different climatic and geological implications under the one heading.

This exercise represents an attempt to map a concept, viz. laterite, and it is a difficult procedure; but then, what mapping does not involve assumptions, concepts, the making of a legend? Topographic mapping is nowadays relatively straightforward and is done largely with machines; but anything more is tenuous. Even if it is intended to map hills, what is a hill? To the inhabitants of the vast sweeping and extraordinarily flat plains of central and northern Australia, tiny pimples that rise a mere 8-9 m above the plains rejoice in the title of mountains (Mt. Fort Bowen and Mt. Brown in northwest Queensland are examples), and for the folk of central Eyre Peninsula and the Murray Plains the highest point of some almost imperceptible rise warrants the name "hill." On the other hand, in the foothills of the Sierra Nevada, east of Fresno in California, what the locals call the Granite Flats are in reality rolling hills with a relief amplitude of up to 10-12 m per $0.5 km^2$ or so. Impressions and perceptions are relative, and a fact is not as hard and fast as it might seem on first consideration.

In any case, induction is just not how scientists work. Apart from its inherent subjectivity and selectivity, induction ignores the reality that most scientific research leads nowhere, that it leads to faulty conclusions or gives results in areas which were not suspected when the project was conceived and started. These results are contrary to the expectations inherent in inductive method.

The only alternative to induction is commonly—though incorrectly—held to be ***deduction.*** Whereas in induction the assembly of the facts is supposed to lead to a series of logical conclusions each following from the other and ultimately leading to a great truth, in deduction an idea or hypothesis or explanation is first erected, and facts then sought to support the original notion. Instead of working from the part to the whole, deduction leads from the general to the particular. Deductive hypotheses have been strongly criticized because not only are there the same defects as apply to the inductive process (for apparently logical thought sequences do not necessarily lead to a valid, much less the correct, conclusion), but the acceptance of a premise leads to even more unobjective observation. As Dostoevsky has said, "Men love abstract reasoning and neat systematization so much that they think nothing of distorting the truth, closing their ears and eyes to contrary evidence to preserve their logical constructions." We see what we want to see, we observe data that support our theories and ignore those which do not.

Chamberlin makes the same sort of comments when he describes the evolution of what he calls a Ruling Theory which is really a statement of a general law and thus deductive in type. Chamberlin states:

> A premature explanation passes into a tentative theory, then into an adopted theory, and then into a ruling theory.

He goes on to point out that during these events the originator acquires a personal attachment to the final result, so clouding his judgment and destroying his impartiality. "There is an unconscious selection and magnifying of phenomena that fall into harmony with the theory and support it, and unconscious neglect of those that fail of coincidence."

Thus, many writers have found evidence of high Pleistocene sea levels and shorelines in many parts of the world, because such flights of forms had been described from the Mediterranean area, and since sea level is a worldwide feature, they ought to be developed, and in some instances preserved, at certain altitudes. And, of course, as they ought to be there, they have been found. Even when the original Mediterranean sequence was shown to be affected by tectonism (which had been known but ignored for many years even before the original scheme was formulated), high strand lines were still being found and recognized in distant places!

A prime example of deductive theory based on a wholly untenable assumption is the Diffusion Theory in archeology. Impressed by the splendor of ancient Egypt and Mesopotamia, many archeologists considered the Fertile Crescent—the eastern Mediterranean and the adjacent parts of southwestern Asia and the Nile Valley—to be the cradle of Western Civilization. Some were specific—Egypt, Mesopotamia—as to the principal source of all good, but even the moderates like Gordon Childe believed in a kind of diffusion, permeation, or percolation of skills and arts from this general area to the rest of the western world, and especially to Europe. As this was the source area, monuments like the pyramids (and all that they implied with regard to engineering and logistics), skills like the ability to smelt and work copper, must necessarily predate any similar megaliths or copper tools or artifacts found in western Europe. Thus partly on this basis, and partly because "savages never invent or discover anything," Stonehenge, for instance, was assumed to postdate the pyramids: the barbarians of the Britain of that time could not conceivably have had the know-how to erect such a gigantic edifice. There was no solid physical basis for this and like assumptions, yet the diffusion theory dominated archeological thinking for the best part of a century.

Carbon 14 dates showed, however, that Stonehenge was older than anticipated on the basis of typological and diffusionist theories. This was not accepted by many archeologists, one of whom referred to the dates as "archeologically unacceptable." But recalibration of 14C dates tied to Californian redwood tree rings suggest that they are much older even than first seemed to be the case. Stonehenge predated the pyramids by about 1,000 years and some megaliths in Ireland, Spain, and France are some 1,500 years older. Moreover, Stonehenge, it is now believed, was in reality a complex astronomical clock. Copper was being worked in Bulgaria long before it was used in ancient Greece and the Fertile Crescent. If anything, diffusion was to the Fertile Crescent, not from it, but more likely there were convergent or independent developments.

Again, when the longitudinal (downstream) profiles of rivers and streams are plotted, most have a concave upward shape, i.e. gradients are steeper in upper sectors than near the coast. With this in mind it seemed obvious that stream velocity must be greater in the steeper upland sectors than on the plains, and this conclusion received support from the rushing, churning action apparent in mountain streams and from the tranquil appearance of plains rivers. Many features typical of river courses—flood plains, etc.—were explained in these terms. Yet, when soon after World War II, systematic and long-term measurements of stream velocities were taken, the picture that emerged was the reverse of that anticipated: ignoring local and well understood variations, streams in general increase in velocity from source to sea. The reason probably is that a large proportion of stream energy is dissipated in friction with the channel bed and banks, and that with rough channels prevalent in uplands and smooth ones in the plains country due to large volumes of water and a tendency to deposition during lateral movements of the river, energy losses are lower and velocities higher in the latter than in the former. And it goes without saying that the forms fully explained in terms of the old assumption are just as readily accommodated in terms of the new data and hypothesis!

V

How then, do scientists work? In reality the method which, either consciously or not, is used in scientific investigations at a high level is that which has been used in geology and geomorphology for many years—almost a century now (see for instance papers by T. C. Chamberlin, and by G. K. Gilbert, late in the last century). It is the method of multiple working hypotheses. Observations are made (empiricisms are *not* wholly rejected!), explanations suggest themselves, problems are connected, and overall, general or widely applicable explanations are devised. These explanations do not result from any consciously logical process (see below) but rather result from reflection, or are derived from comparisons. Then, however, each possible explanation is subjected to rigorous testing using the deductive method but in each case consciously attempting to destroy—not sustain—the argument: the deducible consequences of each tentative hypothesis are matched against the field or laboratory evidence, either that to hand or that collected for the express purpose in mind. The significant, the crucial difference here, is that there is no suggestion of a single logical explanation but of many possibilities each of which is attacked and all of which may be eliminated. There are multiple tracks or routes. Thus there is in scientific research a sequence of imagination and critical evaluation: or what Karl Popper has called *Conjectures and Refutations.*

Popper has said that no scientific theory may ever be regarded as definitely established or proved. Few hypotheses long survive critical examination; some are at least modified and refined, most are shown to be valueless. In consequence, it is both dangerous and unwise to base explanation on a concept whose foundations have not been tested but have been unquestioningly accepted. Testing is a crucial procedure.

For instance, Francis Galton who lived in England late last century endeavored scientifically to examine the power of prayer. He subjected the idea to a statistical test, which is important as a breakthrough in the notion of testing an idea with facts rather than accepting an assertion. Is there any basis for the widespread belief in the efficacy of prayer? If prayers are answered then those prayed for, namely the kings and queens and the royal families of England, ought surely to live longer than others? So should the newborn children of the devout clerics as opposed to the less devout other professional classes. In fact, the available data indicated that members of the royal classes, if anything, lived less long than other landed classes of the day (average 64.04 cf to e.g. 69.49 for clergymen, 70.22 for "gentry," and 67.07 for army officers). The stillborn children of clergymen displayed exactly the same frequency as other classes. Of course, Galton's data was limited. And the method was shaky: he excluded accidents and violent deaths (which is relevant to army officer statistics for instance, as well as for Royals!) but surely the power of prayer should apply equally to violent as to 'natural' deaths? Even more basic is the assumption that prayers, goodness, and long life march hand in hand: it can surely be argued that since believers wish to go to heaven they should be granted their wish sooner than the irreligious (and this point is taken by, for instance, the Plymouth Brethren). Nevertheless, Galton's exercise marked a profound advance because he did not simply accept an assumption but tried to test the hypothesis.

But great care has to be taken to obtain complete data, to avoid going beyond the evidence, and to attempt to disprove an hypothesis, for even seemingly well-based explanations or correlations can, on examination, be found to be faulty. For instance, 8 out of every 9 men who suffer from lung cancer are cigarette smokers, and for this reason, because of this strong positive correlation, it has been widely accepted that smoking causes such carcinomas. And certainly it would be most imprudent, even stupid, to smoke cigarettes if one wishes to maximize one's health and longevity. But the simple direct case is by no means proven, for the correlation cited is not applicable to the whole world. Even women smokers are not as susceptible to lung cancer as are men (cancer of the breast is more common). In the tropics, cancer of the skin is a far greater killer. Cancer is a disease of the aged. But because of the statistical correlation, and because it seems plausible that smoking should induce cancer of the lungs etc., it is widely accepted. And it may be so; but it is not yet fully proven and understood.

There are other strong positive statistical correlations that we dismiss (and rightly so) as absurd and unconnected. For instance, the divorce rate in England early this century rose with the import of Tasmanian apples, and the mortality rate correlates positively with the proportion of marriages solemnized in the Church of England. Southeastern England is subsiding, and the same area receives large number of immigrants both from other parts of Britain and from overseas. Statistically there is a correlation but is there a causal connection?

In rejecting an explanation, one has to be careful not to throw the baby out with the bathwater. An idea may contain the germ or even a large part of an idea, yet be shown to be wrong in detail. Thus Wegener's concept of continental drift translated readily into plate tectonics, and there have been and are still major lateral movements of the continents; yet much of the evidence cited by Wegener did not withstand close examination. Moreover, he himself imposed unnecessary difficulties on the idea by stipulating a limited time scale for the process. Yet the concept of plate tectonics today forms the framework within which other geological data are fitted. Again there is a strong temptation to accept as correct any theory which cannot at a particular time be disproved; but such residual theories are not necessarily right.

Nevertheless, with all the pitfalls, the method of multiple hypotheses in which a conscious and rigorous attempt is made to disprove explanations formulated on the basis of the available data provides the best method yet devised of trying to ensure that the explanation that emerges is the best at that time. Thus flared slopes, elegant basal steepenings found around many inselbergs in southern Australia, could conceivably be explained in terms of wave action, stream flow, wind abrasion, and so on; but the only hypothesis that explains all known data—the exceptional cases as well as the characteristic forms—is that they are formed in two stages, the first involving concentrated weathering of the lower slopes and piedmont, and second, the exposure of the weathering front so formed. The origin of bornhardts, spectacular domical hills, is susceptible to analyses in similar fashion, as are the ice-cored hills of the Arctic, known as pingos and almost certainly due to frost action taking place in a particular context—and so on; there are many examples of reasonable, though almost certainly neither final nor complete solutions, but the best *pro tempore* being arrived at using the method of multiple working hypotheses.

VI

Clearly good science calls for imagination; and an essential feature of good science is an abundance of possible explanations to be tested. What is the imaginative process? Some call it intuition, that is, comprehension or apprehension without reasoning. But it is more likely the subconscious working of the mind supplied by data, stimulated by the search for explanation, and backed by experience which can lead to analogies being drawn. Experience (which is the name everyone gives to their mistakes) is all; Gilbert has gone so far as to state that hypotheses are always suggested through analogy. Hence the need for reflection. Analogy and extension are certainly important.

But many insist on inspiration which is variously defined as "spontaneous conjectures of instinctive reasoning"; "happy guesses"; or "felicitous strokes of inventive talent." Julian Huxley says that scientific research is a "mixture of intuition, pertinacity, and occasional good luck." On the other hand Pasteur observed that chance favors the prepared mind. Basically it is the ability to link what is now seen, the present problem, with past experience either direct or indirect; to get outside the problem and to see it as a whole and to look at it critically through new eyes; to have inventiveness and imagination and to test each possible explanation vigorously and rigorously.

VII

One important benefit of the Popperian view of scientific research is that no theory can be taken as the last word on a problem. Thus, no stigma attaches to be honorably wrong. It is not important to be right or wrong, but rather to stimulate thought and observation. Great freedom results if one is not afraid of being shown to be incorrect. There is a gradual refinement which goes on continuously, getting closer and closer to reality. As Einstein once remarked:

> There could be no fairer destiny for any...theory than that it should point the way to a more comprehensive theory in which it lives on, as a limiting case.

In the earth sciences, research takes on another dimension and acquires another hazard, because many of the forms and features we see are of great antiquity. Some features, like some landslides, sinkholes, and gullies, are of recent date, but other forms and surfaces are of great antiquity. Thus many of the lateritic high plains of the Gulfs region of South Australia are perhaps 200 million years old. We have to attempt to deduce their origin by analogy with modern forms, by comparison. But there are problems in the case cited, because at the time of formation the area was in high latitudes, whereas modern laterite is forming only in certain low latitude regions. Again, then, there are grounds for controversy as to whether it is legitimate and prudent to compare paleoforms with modern analogues.

VIII

Finally it is necessary to mention another difficulty that applies in some disciplines. Mention has been made of originality, but not many have either the energy or the facilities really to ransack the literature. In consequence, incorrect attributions are not uncommon. According to Eiseley, even Darwin was not without his faults in this direction for there is some evidence that his great idea was anticipated by Edward Blyth. Reference is here not made to deliberate failure to cite academic or scientific foes, or to push the works of pals and old school friends; nor are casual, incidental statements that can with hindsight be construed as anticipating such and such a concept (e.g. Aristotle on evolution) here in mind. Undoubtedly some works are effectively suppressed. Thus it has taken 50 years for Crickmay's seminal ideas on the nature of stream erosion, and its implications, fully to be considered. Workers with ideas contrary to one's own may not be invited to present a paper at a conference designed to discuss a particular topic.

Boorstin, paraphrasing Shakespeare, has said that some are born great, some achieve greatness, and some have greatness thrust upon them, while yet others employ a good PR man and press secretary to make them appear great. These things undoubtedly happen, but will eventually be lost in the sands of time, and are for that reason not really important. What is more interesting is that original attributions are commonly quite unwittingly omitted. Thus scarp retreat usually associated with Penck and King, was clearly appreciated by Fisher 50 years earlier. The idea of an etch surface or plain is usually considered to be due to Wayland (1933), but both Logan (1849, 1851) and Jutson (1912) were equally aware of the possibilities of a surface produced by the stripping of a mantle of weathered material. In the late 'sixties Campbell and Twidale suggested that wave transport played a part in the development of lunettes, only to find, after publication, fleeting but nevertheless clear reference to this in a book published almost a century earlier.

There is great debate over the origin of inselbergs, and Linton (1955) and Budel (1957) are usually credited with what is called the two-stage hypothesis, but Falconer (1911) succinctly announced such an origin 50 years earlier. Similarly the two-stage development of granite boulders is commonly credited to Linton (1955) but Hassenfratz was fully aware of the mechanism in 1791. And so one could go on—not only can the validity of ideas be seen in perspective only in the context of time, of history, but the determination of those responsible for the development of an idea also rests with the same process.

But with all these weaknesses and frailties, one cannot but be astounded at the imagination and flair and elegance of some minds. Just as there are people like Kathleen Ferrier who still stand head and shoulders above the mass of great singers, though there are only recordings of her work made with technically inferior instrumentation, just as there are supreme writers like Shakespeare, and composers like Bach, Mozart, Beethoven, and Brahms, so there have been some remarkable scientific minds. Newton's style was so distinctive that, having anonymously solved a mathematical problem, Bernoulli recognized the author, saying that "the lion is recognized by his print."

IX

Scientific research then is basically controversial. Good science generates discussion, thought, and refinement of existing views. But whether changes are advances is difficult to ascertain with certainty: the advance may be along a blind alley or dead end. Science is nihilistic and open-ended. Whether an explanation is right is impossible to say; it is only possible to prove that an explanation is incorrect. The best possible verdict to hope for is 'not proven wrong.' No stigma attaches to being shown to be wrong.

The only true test of the worth of a concept is time. For even with all of man's seeking after kudos, limelight, and fame, only the soundest ideas stand the test of time. Also, the real origins of ideas eventually emerge, in time. In the words of the old proverb: Truth is the daughter of time.'

References and Further Reading

Boorstin, D. J., 1973, *The Americans: the Democratic Experience.* Random House, New York.

Budel, J., 1957, Die 'Doppelten Einebnungsflächen' in den feuchten Tropen. Z. *Geomorph.,* 1: 201-228.

Campbell, E. M, 1968, Lunettes in southern South Australia. *Trans. Royal Soc. S. Aust., 92:* 85-109.

Castany, G. and F. Ottman, 1957, Le Quaternaire marin de la Méditerranée occidentale. *Rev. G'eogr. Phys. G'eol. Dynam.,* 2: 46-55.

Chamberlin, T. C, 1890, The method of multiple working hypotheses. *Science,* 15: 92-96.

Childe, V. G., 1927, *The Dawn of European Civilisation.* Paul, Trench Trubner, London.

Childe, V. G., 1942, *What Happened in History,* Pelican, Harmondsworth, Middlesex.

Darwin, C, 1859, *The Origin of Species,* Murray, London.

Darwin, C, 1958, *The Autobiography of Charles Darwin, 1809-1882.* Collins, London.

Darwin, F. (Ed.), 1887, *The Life and Letters of Charles Darwin.* Murray, London.

Deperet, C, 1918, Essai de co-ordination chronologique général des temps Quaternaires. C. *R. Acad. Sci. Paris,* 167: 418-422.

Eiseley, L., 1958, *Darwin's Century.* Doubleday, New York.

Eiseley, L., 1979, *Darwin and the Mysterious Mr.* X. Harvest/HBJ, New York.

Falconer, J. D., 1911, *The Geology and Geography of northern Nigeria.* Macmiîlan, London.

Feyerabend, P., 1975, *Against Method.* NLB, London.

Fisher, O., 1868, On the disintegration of a chalk cliff. *Geol. Mag.,* 3: 354-356.

Fisher, O., 1872, On cirques and taluses. *Geol. Mag.,* 8: 10-12.

Gilbert, G. K. 1886, The inculcation of scientific method. *Amer.]. Sci.,* 31: 248-299.

Hassenfratz, J-H, 1791, Sur l'arrangement de plusieurs gros blocs de différentes pierres que l'on observe dans les montagnes. *Ann. Chimie,* 11: 95-107.

Hitch, C. J., 1982, Dendrochronology and serendipity. *Amer. Sci.,* 70: 300-305.

Jutson, J. T., 1914, An outline of the physiographical geology (physiography) of Western Australia. *Geol. Surv. W. Aust. Bull,* 61.

Kant, E., 1958, *Critique of Pure Reason* (trans. N. K. Smith). Modern Library, New York (1st edition 1781).

King, L. C, 1953, Canons of landscape evolution. *Geol. Soc. Amer. Bull,* 64: 721-752.

King, L. C, 1957, The uniformitarian nature of hillslopes. *Trans. Geol. Soc, Edin.,* 17: 81-102.

Leopold, L. B., 1953, Downstream change in velocity in rivers. *Amer.]. Sci.,* 251: 606-624.

Linton, D. L., 1955, The problem of tors. *Geogr. J.,* 121: 470-487.

Logan, J. R., 1849, The rocks of Palo Ubin. *Genoots. Kunsten Wetenchappen (Batavia),* 22: 3-43.

Logan, J. R. 1851, Notices of the geology of the Straits of Singapore. *Quart. J. geol. Soc. London,* 7: 310-344.

Mackey, J. R., 1962, Pingos of the Pleistocene Mackenzie Delta area, *Geogr. Bull,* 4:21-63.

Mackey, J. R. and J. K. Stager, 1966, The structure of some pingos in the Mackenzie Delta area, N.W.T. *Geogr. Bull,* 8: 360-368.

Medawar, P. B., 1969, *Induction and Intuition in Scientific Thought.* Amer. Phil. Soc, Philadelphia.

Penck, W., 1924, *Die Morphologische Analyse.* Engelhorns, Stuttgart.

Penck, W., 1953, *Morphological Analysis of Landforms* (trans. H. Czeck & K. C. Boswell). Macmillan, London.

Popper, K., 1963, *Conjectures and Refutations.* Routlege, Keagan Paul, London.

Popper, K., 1972, *Objective Knowledge.* Clarendon Press, Oxford.

Read, H. H., 1957, *The Granite Controversy.* Murby, London.

Renfrew, C, 1973, *Before Civilization: the Radiocarbon Revolution and Prehistoric Europe..* Knopf, New York.

Rubey, W. W., 1952, Geology and mineral resources of the Hardin and Brussels quadrangles, Illinois. *U. S. Geol Surv. Prof. Paper,* 218.

Tarling, D. H. and M. P. Tarling, 1971, *Continental Drift.* Bell, London.

Twidale, C. R., 1962, Steepened margins of inselbergs from north-western Eyre Peninsula, South Australia. *Z. Geomorph.,* 6: 51-69.

Twidale, C. R., 1968, *Geomorphology, with special reference to Australia.* Nelson, Melbourne.

Twidale, C. R., 1978, Early explanations of granite boulders. *Rev. Geomorph. Dynam.,* 27: 133-142.

Twidale, C. R., 1982, The evolution of bornhardts. *Amer. Sci.,* 70: 268-276.

Twidale, C. R., 1982. *Granite Landforms.* Elsevier, Amsterdam.

Wayland, E. J., 1933, Peneplains and some other erosional platforms. *Geol. Surv. Uganda Ann. Rept. Bull,* 1: 77-79.

Watson, J. D. 1968, *The Double Helix.* Weidenfeld & Nicolson, London.

Wegener, A., 1924. *The Origin of Continents and Oceans.* Methuen, London.

Woods,]. E. T., 1862, *Geological Observations in South Australia: principally in the district southeast of Adelaide.* Longmans Green, London.