ARTICLES

A PHILOSOPHIC RATIONALE FOR A CREATION-FLOOD MODEL

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Patterns in the progress of science provide a sound basis for the need to develop new ideas. The application of this principe to the development of a creation-flood model is elucidated.

All too often science is viewed as a collection of objective scientists: cold, calculating appraisers of data like so many animated computers moving steadily upward to higher and higher levels of truth. However, reality is somewhat different from that, for computers and scientists do not analyze data from neutral positions. Both computers and scientists have to be programmed, and this programming process introduces limits to the objectivity of both.

As a scientist progresses through his formal education, he is taught about the data in his field, but it cannot be presented as a mass of unrelated facts. Rather, the data must be organized into a meaningful picture, or theory, that the new scientist can readily understand. Having grasped the meaning of what is already known, he can then move on and challenge the unknown. The problem is that as the scientist learns the accepted theories in his field, he frequently develops a sort of tunnel vision, for he is taught these accepted theories before he is prepared to critically evaluate them. Consequently by the time he is ready to make original contributions of his own, his thinking has already been channeled along certain lines. He has learned what type of questions are considered legitimate scientific questions to ask in his research, and which questions are not.

An astronomy student 2000 years ago would have learned that the problems yet to be solved were the development of improved mathematical models of the movements of the heavenly bodies around the earth. It was not legitimate to ask if the earth really was the center of the universe, as it was already known, with good supporting evidence, that the earth was

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indeed stationary. This “fact of nature” was questioned by very few scientists.

The narrowing of acceptable thought patterns is, of course, useful to science. When a field of science is operating within a “paradigm” — a universally accepted scientific principle or theory — the research of all workers in that field will be unified and directed toward common goals. Much more is accomplished than if each worker were left to flounder on his own. Astronomers working under the paradigm of the geocentric universe could agree on the position of the earth and work together plotting the paths of stars and planets within that world view.

This process of solving the relevant problem within a given paradigm is what T. S. Kuhn (1970, p 24) refers to as normal science:

> *Closely examined, whether historically or in the contemporary laboratory, that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena: indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others. Instead, normal-scientific research is directed to the articulation of the phenomena and theories that the paradigm already supplies.*

A paradigm is thus a useful part of science, but it is at the same time a hazard. Most scientists never escape the restrictions of a well-accepted paradigm, even if it is wrong. A scientist’s paradigm determines what observations he will make, what experiments he will do, and how he will interpret the results of his experiments (Kuhn 1970, p 116, 126). Under the geocentric-universe paradigm, if it was difficult to fit the movements of planets into the system, it was just a challenging problem and was not generally considered as evidence against the theory. For a scientist working under the evolution paradigm, the absence of fossil intermediates between the phyla of plants and animals is not evidence against his theory; it is just a problem to be solved by future research.

A paradigm can be a very pervasive concept, because once it is accepted, prevailing opinion encourages continued acceptance and development. As an example, the San Juan River has generally been interpreted as the result (see Figure 1) of slowly flowing water on a gentle slope. Such a slowly flowing river would take a very long time to carve a trench that deep. Even some conservative Christians who originally believed in a short-earth chronology have considered this erosion pattern as evidence for long ages for life on the earth — far beyond the amount of time allowed by standard Biblical interpretations. This seems to illustrate how the

FIGURE 1. The meanders of the San Juan River in southern Utah. Note that the river has cut straight down through the sediments rather than undercutting the bank at bends in the river bed.

popularity of a paradigm can cause it to pervade beyond the limits of rigorous objectivity, since experimental results (Shepherd 1972) propose that a meandering river like the San Juan, which cuts straight down into the sediments, does not form by slowly flowing water, but by flood conditions which keep all the sediments in suspension. His conclusion is that such river courses are cut by periodic flash floods.

Dr. J. W. Provonsha (1973) has a useful concept which he uses in discussing the nature of revelation. I believe it applies to a scientist in the same way. In the study of science, as in the study of religion, we receive new information only through our senses, and the scientist, as well as the religionist, has a “filter” in his mind, with a feed-back mechanism. The concepts developed in his mind determine what observations the filter will allow, and what observations will be filtered out because they are not relevant.

A scientist working within a given paradigm will evaluate all new ideas under the rules of his paradigm — it is a more-or-less closed system. Only the imaginative and daring few ever break out of widely accepted paradigms. The geocentric theory held sway in astronomy for at least 1800 years, and even when it became evident that the theory wasn’t working, it was not generally abandoned until the creative ideas of Copernicus opened the way for a new interpretation.

Science, like other areas of intellectual endeavor, is slow to accept new ideas. The list of great scientists whose work was not accepted by their contemporaries is frightfully long. Newton, Gauss, Copernicus, Vesalius, Lister, Mendel, and Avogadro are a few examples (Clark 1972, p 99-101; Kuhn 1970, p 150). An idea that doesn't fit accepted concepts will generally look rather foolish. A. N. Whitehead (1950, p 70) observes that:

> *If you have had your attention directed to the novelties in thought in your own lifetime, you will have observed that almost all really now ideas have a certain aspect of foolishness when they are first produced.*

I would suggest that almost any idea which jogs you out of your current abstractions may be better than no exposure to new ideas.

Karl Popper (1963) makes the following comments on the scientific process:

> *What is called scientific objectivity consists solely in the critical approach — in the fact that if you are biased in favor of your pet theory, some of your friends and colleagues (or failing these, some workers of the next generation) will be eager to criticize you; that is to say, to refute your pet theories if they can.... it would be a mistake to think that scientists are more 'objective' than other people. It is not the objectivity or detachment of the individual scientist but science itself — or what may be called 'the friendly-hostile cooperation of scientists', that is, their readiness for mutual criticism — which makes for objectivity.*
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> *There is even something like a methodological justification for individual scientists to be dogmatic and biased. Since the method of science is that of critical discussion, it is of great importance that the theories criticized should be tenaciously defended. For only in this way can we learn their real power; and only if criticism meets resistance can we learn the full force of a critical argument.*

Thus even though it may seem incongruous, the scientist who gets the most work done may be the one who is personally committed to his paradigm and is determined to prove it correct. It seems to me that this is the kind of commitment necessary to develop a new paradigm to the point where it is acceptable.

If Kuhn (1970) is correct, a new paradigm is likely to arise when an accepted paradigm has reached a crisis — it persistently fails to solve important problems. This failure may lead one or a few independent-minded scientists to look for a new paradigm which may turn out to be completely incompatible with the old — a different world view, like putting the sun, rather than the earth, in the middle of the solar system. Kuhn points out that new paradigms often attract their first followers for reasons that are not entirely scientific — it may be more aesthetic, "neater," or "simpler" than the old one. And I would add another reason — it may be supported by revealed information.

A newly proposed paradigm will at first be able to solve only a few of the problems that confront it and will attract only a few followers. The success of the new paradigm may depend on the work of these few, and "ordinarily it is only much later, after the new paradigm has been developed, accepted, and exploited, that apparently decisive arguments ... are developed (Kuhn 1970, p 156). The decision to accept a new paradigm may be primarily based on future promise rather than past achievement (Kuhn 1970, p 158):

> *The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith.*

This does not mean that a scientist can ignore evidence; it just means that even a good paradigm will not succeed unless its followers believe in it enough to carry it through its difficult early stage of development.

This analysis of changing paradigms may help us in our consideration of the relation between science and religion. When a prevailing paradigm, such as the geologic paradigm requiring a long history for life on earth, contradicts sacred history, the problem will not be solved by making a few adjustments in current geologic theory. Also a paradigm suggesting an old age for the earth is not necessarily true just because there are many lines of evidence that fit the theory. In the study of astronomy, the inadequacies of the geocentric-universe paradigm could not be remedied by making more adjustments — an entirely different view was required. When a new paradigm was suggested, it was not immediately apparent that it was better than the old one. The data fit the geocentric theory amazingly well — so well that it is still used today in fields such as surveying and navigation (Kuhn 1957, p 373).

I believe that before we can make a fair comparison between the long-ages paradigm of the geologists and the Biblical idea of a short chronology, we will have to build an entirely new paradigm. This will only happen when enough scientists in various disciplines have sufficient faith in revelation to stake their careers on the effort. In developing a new paradigm, much data will have to be placed on a shelf temporarily; not to be forgotten, however, but to be taken down periodically and reexamined to see if its true explanation can yet be found. Much information cannot be expected to fit until a new theory has been worked out in some detail. I personally do not feel that we are justified in questioning the many direct and pointed statements in the Bible and writings of E. G. White indicating a 6-day creation a short time ago, followed by a worldwide flood, when current geologic theories have never been confronted with a diligent, well-staffed effort to develop an alternative paradigm.

Judging from the history of other scientific paradigms, it would seem that an incorrect geology paradigm will be eventually rejected for a better one. That could take hundreds of years. It took 1800 years for the problems in the geocentric theory to be adequately appreciated. Science moves faster now, but even so, geology, especially radioactive dating, is a young science, and a complex one, and it will take a long time for such theories to be developed to the point where they can be properly analyzed. The process could be greatly speeded up if enough people were working on a competing paradigm now.

When a scientist, who is a Christian, decides after a few years of study in a field of science dominated by a non-Christian paradigm, that he must abandon his religious views, I respect his right to make his own choice, but I can still wish that he had been more cautious in making such a decision, since it will take a huge amount of effort by scientists committed to the Biblical view before that view will have a fair trial. Perhaps a comment by R. E. D. Clark (1972, p 117) is pertinent here:

> *... a man who abandons his faith in God may do so in the conscious belief that he is humbly bowing before the facts, while unconsciously he proudly asserts that his intellectual grasp of the universe is such that no complicating factors have escaped his notice.*

The geological phenomenon known as turbidity currents is an interesting illustration of the danger of believing currently accepted ideas without question. Tremendous volumes of sedimentary rock with graded bedding were formerly interpreted as gradual accumulations of shallow-water sediments. They were interpreted that way until the concept of turbidity currents was introduced in 1950 (Kuenen & Migliorini 1950). Turbidity currents are rapid underwater mud-or-sand-flows on a large scale. Since 1950 the supposed “shallow-water sediments” mentioned above have been reinterpreted as the result of turbidity currents. Walker (1973, p 3) states that:

> *The revolution in thought has affected our ideas of erosion, transport, dispersal, and deposition in modern ocean basins. It has also completely changed our interpretations of sandstones and conglomerates in ancient basins....*

The turbidite concept is based on extensive observations and experimental work, and Walker (1973, p 3) maintains that “no other development in clastic sedimentology in this century has caused a complete change in thinking of comparable magnitude.” Graded bedding was routinely observed in these deposits before, but “all authors, however, followed the old geological paradigm which stated that sandstones and conglomerates were shallow-water deposits” (Walker 1973, p 16). The old explanation of gradual accumulation in shallow water was accepted until the phenomenon of turbidity currents was discovered, causing a complete reinterpretation. In how many other fields might there be comparable new discoveries waiting to be found, that will precipitate reinterpretations of comparable scope?

In the study of radioactive dating, certain things are known, at least in an elementary way: the patterns of distribution of the relevant chemicals in certain rocks; general trends through the geologic column; the presence of anomalous dates, including systematic anomalies through geologic “time.” Other factors may affect radio decay products. Some very important factors are not known: were clocks really set to zero in molten rocks; what were conditions in the magma chambers; are those conditions reflected in present distribution of radioactive elements and products; what other factors could have affected it, including possible divine energy influencing the earth at the time of the flood; what causes a given atom to decay (perhaps the most important fact to know); to what extent do scientists’ filters influence them in selection of which data are relevant?

I propose that the pillars of scientific truth are not always as solid at they appear. The solidity may be an illusion, produced by the tendency of a scientist to interpret all data in harmony with currently accepted paradigms. Scientific explanations which we do not agree with should not be ignored, but if we are too quick to embrace them, we may be on as weak a foundation as the astronomers in the days when it was a “known fact” that the earth was the center of the universe. In the words of Karl Popper (1959, p 111):

> *The empirical basis of objective science has thus nothing ‘absolute’ about it. Science does not rest upon rock-bottom. The bold structure of its theories rises, as it were, above a swamp. It Is like*

a building erected on piles. The piles are driven down from above into the 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.

In evaluating various scientific theories, we must weigh the reliability of the evidence available for each theory — how much is known, how much is not known, how long the theory has been under study, how good the evidence is. There will be a wide spectrum, from reliable to very shaky speculation.

The theory of gravitation may be close to the upper end of the spectrum of reliability, but I suspect that most theories of interest in a study of science and religion are fairly close to the bottom end of the spectrum, floating on the surface of the "swamp."

Another factor to consider is the fact that modern science is moving very fast. This leads to a rapid increase of knowledge, but it also may mean that much of our most advanced thought is still on the fringes of reliability.

There was a time when the Bible was used to support the geocentric theory. However, I have not found any statements that "in the beginning God created the earth in the center of the universe, and rested on the seventh day; therefore God blessed the Sabbath day...." However I do find that for certain other ideas related to earth history (length of creation week, world-wide nature of the flood, e.g.), the revealed information is much more explicit, and the scientific information is much less explicit.

We must, however, be careful that we do not read our pet theories into the Bible. Just to mention one example: we sometimes talk as if the flood was a simple event, lasting one year, but all we know from the Bible is that after one year there was at least enough dry land on Ararat for the occupants of the ark to survive.

In summary, I am convinced that the widely accepted concept of the complete objectivity of the individual scientist is naive — an unfortunate twentieth-century myth. A new theory triumphs, not because of the greater objectivity of its adherents, nor because an "objective" person can tell, initially, that it fits the data better, or even as well, as other theories. Eventually it will succeed only if it stands the test of time and criticism. In other words a theory will succeed if the practical world of research shows that it *works*. No matter how right a new and different idea may be, it is destined to failure unless a group of scientists have the devotion, determination, and "guts" to put their full energy into developing their hypothesis in spite of criticism.

Those who in the past have worked hard, and often alone, in their efforts to develop concepts of flood geology have provided a valuable launching point for the team effort that has finally become possible in recent years. In many areas of geology and paleontology we have only been able to answer science-based criticisms of Genesis by giving the reasons why the accepted scientific interpretations are not compelling or proven. These reasons are good to know, and are a profitable start, but they are convincing only to a few people. Conventional uniformitarian theories in geology and paleontology are satisfying to many because of the abundance of research data (inconclusive though it may be) that backs it up. In our efforts to aid honest people in gaining confidence in revelation, the one thing that will make the difference is a demonstration that in the practical world of research, flood geology *works*!

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