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Book Reviews

Philosophy of Geohistory

Edited by C. C. Albritton, Jr.
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$24.00

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Claude C. Albritton, Jr. has brought together 15 papers written between the years 1785 and 1970. One thing they have in common is that they all touch on some aspect of philosophy and of Geohistory, but otherwise they should be broken down into three or four groups. The first group of papers was written before, during and immediately after the French Revolution when the philosophical issue of the immanence or lack of immanence of God in the creation of the world was being worked out. There is a selection from Hutton, from Playfair, from Lyell and from Whewell which take us into the thick of the battle using the words of the scientists themselves. Unfortunately the catastrophists are not represented at all, except half-heartedly by Whewell.

Philosophers are notoriously sloppy about dates and geologists talk in terms of the nearest million years, but it bothers an historian of science to see the date of Lyell's contribution given as 1872, when in fact, it was written forty years earlier. Again, Albritton takes his selection from the 1872 edition of Whewell's *History of the Inductive Sciences*, when, in fact, it had been written thirty-five years earlier. In 1872, Whewell was long since dead and Lyell nearly so. These dates are significant if one subscribes to the structure of scientific revolutions as outlined by Kuhn.

Kuhn claims that major philosophical issues are hammered out in the pre-paradigm stage of the science or during a scientific revolution. Once worked out, scientists take these philosophical presuppositions for granted and "articulate the paradigm" by means of sophisticated research techniques. Kuhn reasons that if scientists had continued to argue over fundamentals, science would never have proceeded to its highly sophisticated stage of development; it would have remained, like philosophy, in a state of endless and unfruitful debate. Thus for Albritton to give Lyell's paper the date of 1872 and Whewell's paper the same date, is to suggest that the debate over fundamentals was still going on throughout the 19th Century, which is misleading. The debate over the immanence of God was going on among theologians and among philosphers, but geologists had long since passed on to the normal stage of development, spending their time making geological surveys, mapping the course of railroads, searching for mineral deposits and articulating the uniformitarian paradigm in very specific ways.

When a science passes out of the pre-paradigm, or revolutionary stage into what Kuhn calls "normal" science, the philosophical interest of its practitioners passes from questions about the nature of God, or the nature of nature - that is to say passes from questions about fundamentals to questions of methodology. In the paradigm stage methodology becomes all important because the inculcation of the proper method in younger scientists is the means by which the perpetuation of the paradigm is insured. The introduction of new methods is always greeted with suspicion because new methods stand in danger of turning up not only new data, but new kinds of data. The "correct" method is one that insures the young practitioner that he will uncover the "Truth"; it is the method by which the founders of the science had uncovered the truth, or are alleged to have uncovered the truth, and so it is the method that must at all times be used. Actually, a rigid methodology, like a rigid liturgy, insures conformity, which is taken as the same thing as truth. All normal scientific methodologies stress some form of empiricism because facts have never led to a revolution in science. Facts are safe. Every scientific revolution of any consequence - the Copernican, Newtonian, Lyellian, Darwinian, Lavoisierian, Einsteinian, - have been made by the reinterpretation of old facts, not by the discovery of new ones. The second group of papers in Albritton's volume concern "methodology", and are written in the period 1890 to the present, when the uniformitarian paradigm is well established. Typical of these papers is one by W. M. Davis (1926) on "The Value of Outrageous Geological Hypotheses." What is so amusing about this paper is that the "outrageous" hypothesis he proposes, is fully in conformity with the uniformitarian paradigm, as are those used to illustrate the papers by Chamberlin, Gilbert, Johnson, Mackin and Anderson, all of which professed to encourage free inquiry and wild speculation, while doing, in fact, just the opposite.

The last group of papers have all been written since 1962 and represent modern reflection on the philosophical (as opposed to methodological) implications of geology. This group can be divided again into two parts: those reflecting back on the original uniformitarian debate, and those looking on geological issues from the perspective of logical positivism.

There is a paper by M. K. Hubbert, on the "Critique of the Principle of Uniformity", which is amazing not only for the clarity of its writing, for its grasp of the philosophical issues of modern science, but also for its very competent handling of the history of geology. If you have time to read one article in the book, read this one. It is followed by one written by G. G. Simpson, who is every bit Hubbert's match in intellect, but unlike Hubbert, has completely botched the historical end of things. He argues that Darwin's theory of evolution emerged as much from Cuvier's catastrophism as it did from Lyell's uniformitarianism, on the grounds that Lyell believed in a steady state model of the universe while Cuvier allowed for successive progessive creations. Poor Darwin must be either rolling over in his grave, or having a good laugh - I suspect the latter. Actually if Simpson had read Darwin's notebooks, he would have learned that Darwin was not really interested in evolution as such, nor really in "progress" except as a means to an end. Darwin never uses the word "evolution," he uses the word "descent" or "transmutation". "Evolution" was used by Auguste Comte, and was later superimposed on Darwin's theory. Darwin was interested primarily in establishing that God did not interfere arbitrarily in the universe, but ruled by means of law: "Astronomers might formerly have said that God ordered each planet to move in its particular destiny. In same manner God orders each animal created with certain form in certain country, but how much more simple and sublime", Darwin writes on page 101 of his first Transmutation Notebook, "let attraction act according to certain law, such are inevitable consequences - let animal be created, then by the fixed laws of generation, such will be their successors. Let the powers of transportal be such, and so will be the forms of one country to another. - Let geological changes go at such a rate, so will be the number and distribution of the species!!" (Darwin's exclamation). Note the biblical phraseology. Darwin was trained as an Anglican priest. His "tree of life" metaphor was taken from the Book of Revelation, and the above passage was Darwin's rephrasing of the first 13 chapters of the Book of Genesis; in writing this, as in all his writings, he was directly attacking the position of Georges Cuvier, which brings us back to the 14th article in Albritton's volume, written by the Dutch Reform geologist, historian and philosopher, Reijer Hooykaas, "Catastrophism in Geology, Its Scientific Character in Relation to Actualism and Uniformitarianism". Coming from a religious background himself, Hooykaas understands better the theological issues surrounding the early history of geology and hence is a good deal more historically accurate than is Simpson, although from a modern geological standpoint, not so perceptive as Hubbert.

If we stand back for a moment from this battle that has been raging now for more than twenty years between Hooykaas and Simpson and view it with some perspective, we find that the issue involves more than an academic argument, but two completely divergent interpretations of the nature of science itself: does science develop purely by internal methodological procedures, or is the development of a science caught up in the wider intellectual history of the era?

If you believe that science is autonomous, then it is necessary to argue that Cuvier, a scientist, and Lyell, a scientist, and Darwin, a scientist, are all working towards a common end: the accumulation of knowledge. They might be rivals, but they cannot be enemies. It is natural then for Simpson to suppose that Darwin drew from Cuvier as he had drawn from Lyell in the foundations of his thought, and that the theory of evolution was a purely internal scientific matter completely independent of the issues being fought over during the French Revolution.

The opposite position to that of Simpson, Eiseley, and the "cumulative truth" school, is that of the Frankfurt School, which argues that all "truth" including scientific "truth", is mere social projection of the class structure in society used to oppress the masses. Herbert Marcuse is the most influential member - influential particularly among radical graduate students of Sociology, as is Max Horkheimer and Theodor U. Adorno (see Martin Jay, *The Dialectical Imagination).*

As usual the true truth lies half way in between: There are some scientists who operate entirely within the given paradigm and who never question fundamentals, just gather data: but we do not hear much of them; they tend to be second rate; then there are those who are only interested in articulating their own religious or philosophical point of view, and little is heard of them except on the street corner or in bars; if we examine closely the notebooks of the really great scientists - the Lyells, the Darwins, the Newtons, the Einsteins, we discover that they all had very strong religious, political and philosophical points of view, but that they also went through agonies to reconcile their philosophy with current scientific data. The great scientists are not great discoverers, but great reconcilers of man, a social animal, and the natural environment in which he lives.

There are two more articles in Albritton's book that I would like to mention before closing; one, by Toulmin, opens the volume; the other, by Kitts, closes it. Both are on Geological "Time".

When the Uniformitarian debate was closed in the 1840s by assimilation of Louis Agassiz's ice ages, and geologists took to the fields to guide railroads across Europe, Asia and America to carry back minerals for the great industrial revolution, the issues which uniformitarianism raised, did not die, but passed first to Natural History with the appearance of Charles Darwin's theory about the *Origin of Species*, and then passed to physics with the publication of Einstein's theory of relativity in 1905. 17th Century Protestants had argued that God had created each species and then had interrupted the normal laws of nature at the time of the deluge to punish man for his sins. Lyell had placed Geology beyond the realm of the deluge, and had eased God out of the creation of species slightly. He granted that God created the basic species, but that these species radiated out into new varieties. Lyell believed that there was, however, a limit to the variation possible, and if the species was forced to try to exceed the limit by changes in the earth, the species became extinct.

Darwin took the process of easing God out of the universe one step further. In a book with a purposely ambiguous title: *On the Origin of Species*, he argues that the limitation placed on the variation of species by Lyell is not warranted, and that species can vary indefinitely away from the original type. In his notebooks, Darwin warns himself not to attempt too much, not to fall into the trap Lamarck fell into by proposing to explain how a snail could transform itself into a porcupine. In the *Origin of Species* and in *Variations of Animals and Plants under*

Domestication, he talks about the origin of species from a closely related species. He leaves the origin of classes, orders, and genera to the imagination of the reader. If the reader was religiously inclined, as was Asa Gray, he could leave the origin of higher taxa to God, or if he were atheistically inclined, as was Haeckel, he could eliminate God altogether. There was, at any rate, room enough for philosophical and theological discussion, and, indeed, the later part of the 19th Century was filled with it.

Positivists, however, were clear that God had no place in science, and Ernst Mach, the Austrian physicist, who had been flunked out of school by the Jesuits and told to become a carpenter, set himself the task of rigorously stripping science of all concepts which had a theological or even philosophical foundation. The concept of "law", which Newton had so successfully used, and which Darwin had so ardently cultivated, Ernst Mach on the other hand mightily despised. Newton, a fundamentalist, had believed that God had directly promulgated the three laws of motion, but Mach argued that a so called "law" in science was nothing more, and nothing less, than an "economical description of observational data". Einstein, following Mach, pointedly avoided using the term "law" altogether and hence we have the "principle" of relativity instead of the "law" of relativity.

In Vienna, there arose a school of logical positivism, who were direct followers of Mach, and who attempted to systematically examine terms used in science to see what they actually meant, terms like "time", "space", "hypothesis" and "law". These were terms which scientists tended to use without ever really defining them, and it was in these terms that "extrascientific" concepts, like "god", crept in. However, when the logical positivists attempted to define rigorously these scientific concepts, they ran into problems. To define one word, you must use another word, which, in turn, needs to be defined, and so on indefinitely. Finally Gödel showed that sooner or later you must begin with an undefined term, or, more accurately, you must begin with a term which is not defined by science or philosophy, but whose meaning is assumed through the historical experience of using it in the community. Hence Toulmin has argued that to understand the philosophy of science, you must understand the history of science. If you want to know what the word "time" means, you have to go back through history and understand the historical context in which the accepted usage of the word developed.

We have now come full circle. The original concepts of Geology emerged not only from an empirical study of nature, but also from the historical experience of Western Europe in the 17th, 18th and 19th Centuries. In using these terms to describe Nature, the original geologists were not able to escape their involvement in the social, political and theological events around them - nor are we, or if we do escape, it is at the peril both of science and of society.

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Crystalline Solids

By Duncan McKie and Christine McKie
Thomas Nelson and Sons Ltd., London, 628 p., 1974.
£6.50 (about $14)

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The declared object of this book is to provide an introduction to the study of crystalline solids for students in a first year course; a number of more advanced sections have been included to appeal to students in a second year of crystallographic study. The authors also propose that it may appeal to graduate students who have come to crystallography from other disciplines. We consider these objectives too extensive for a treatment satisfactory to any of the classes of subjects it is intended for.

In fact, we consider the book unique in its breadth: crystal morphology, atomic structure, X-ray crystallography, crystal chemistry, crystal physics, optics, thermodynamics, analytical chemistry are all included and treated in a reasonably comprehensive manner.

The illustrations are generally clear. While the development of specific topics is admittedly not rigorous, it does nevertheless convey a physical understanding of the subject. The inclusion of thermodynamics (Part II) is certainly unusual in an introductory text to crystallography; much of this part of the text is concerned with classical thermodynamics and the tie with crystallography via statistical thermodynamics is indeed very tenuous.

As a tool of learning in the hands of first or second year students, we feel that the inclusion of exercises on the various topics would have been very helpful: we did not see any. The book may be more successful as a reference manual.

The number of introductory texts in crystallography published in English is considerable. This one is unique for its breadth: at the current price, its acquisition as a reference manual would appear interesting to us.

MS received October 6, 1975