VLADIMIR VLADIMIROVICH TIKHOMIROV (1915–1994)

Vladimir Vladimirovich Tikhomirov was born on October 25, 1915, in St. Petersburg. In 1938 he graduated from the geological prospecting faculty of Azerbaijan Industrial Institute in Baku, and until 1942 he worked as an engineer-geologist in the Azerbaijan Geological Department. In 1942 he went to the front as a volunteer. He fought near Leningrad, had a military rank of senior lieutenant, was a navigator of an air-section, and served as assistant commander of a detached air squadron of the 13th Air Army. On April 18, 1944, he was seriously wounded and lost his sight completely. In January 1945, not long before the end of the 2nd World War, he was demobilized as an invalid of the 1st group. On leaving the hospital he found the will-power to continue his scientific work. In 1949 he finished a graduate course in the Moscow Geological Prospecting Institute where he obtained two degrees at the same time: Candidate of Geological-Mineralogical Sciences, and Doctor of Sciences for his dissertation on the geology of the Caucasus Minor.

From 1951 V. V. Tikhomirov at first headed the cabinet, then the section (from 1956), and from 1961 the laboratory of the history of geology in the Geological Institute of the Academy of Sciences of the USSR. In 1955 he was appointed a professor of the history of geological sciences. In 1956 V. V. Tikhomirov, together with V. E. Khain, published the book *Short Essay on the History of Geology,* which was reprinted in Peking in 1959 in Chinese. In 1960 and in 1963 two parts of his monograph *Geology in Russia in the First half of the XIX Century* appeared. Tikhomirov was the founder, beginning in 1953, of the well known series *Essays in the History of Geological Knowledge.* Volume 20 out of the 28 volumes in this series is comprised of his work *Geology in the Academy of Sciences (from Lomonosov to Karpinsky).* From 1955 to 1991 V. V. Tikhomirov headed the work of the Commission on the History of Geological Knowledge and Geological Study of the USSR (COGHI), and he served as the main editor of the 52 volumes of the unique reference-information book *Geological Study of the USSR.*

V. V. Tikhomirov demonstrated his strong organizational skills as a founder and long-term officer of INHIGEO. The National Committee of Geologists of the USSR proposed the formation of this body and plans for it were initiated during the XXIInd International Geological Congress in New Delhi in 1964. In June, 1967, the Constituent Assembly of INHIGEO (which originally was called the International Committee on the History of Geological Sciences) met in Erevan, Armenia, and adopted statutes and bylaws for the new organization. Arriving in Armenia, 150 historians of geology from 15 countries elected a directing Bureau with I. I. Gorsky, of the National Committee of Geologists of the USSR, presiding. On August 23, 1968, at the XXIIIrd IGC in Prague, INHIGEO was formally admitted as a Committee of the International Union of Geological Sciences (IUGS). The Bureau unanimously elected Vladimir Tikhomirov as president of INHIGEO, and the IUGS Council endorsed this action. Later in the same month, at a meeting in Paris on August 29, INHIGEO was enrolled as an Affiliate of the International Union of the History and Philosophy of Science (IUHPS). Interest in this field of knowledge spread rapidly over five continents due, in large part, to Tikhomirov's efforts. He was elected to a second term as president of INHIGEO at the XXIVth IGC in Montreal in 1972, and, as past-president, he continued to serve on the INHIGEO Bureau during the IGC sessions in Sydney in 1976 and in Paris in 1980. It was at the 1980 IGC in Paris that the IUGS Council adopted new statutes and bylaws that transformed INHIGEO from a Committee to a Commission. In 1984, at the IGC in Moscow, Tikhomirov began two terms as a vice-president of INHIGEO.

In his works V. V. Tikhomirov performed historical analyses of the development of the ideas and methods of geology. After considering many principles of dividing the history of geology into periods, he concluded that division into periods is not an artificial method of investigation but one that reflects objective reality, given the irregular course of development of the sciences as a whole. He was convinced that not knowing the history of any field of knowledge and its methodological problems, makes it impossible to value its present state, to predict future development, and to carry on successful research. This was clearly shown by the collective authors headed by Tikhomirov in the 1980 monograph *History of the Geological Institute of the Academy of Sciences of the USSR: Development of the Institute, its Scientific Schools and Bibliography of the Works.*

V. V. Tikhomirov received many honors from his government and from the international community. The USSR awarded twenty medals and orders to him for services in battle and in labor. In 1963 the International Academy of History of Sciences in Paris elected him a corresponding member, and in 1966 promoted him to a full member. In 1976 the Society of Geological Sciences in Berlin elected him an honorary member, and in 1981 V. V. Tikhomirov became a corresponding member of the Academy of Sciences of the USSR in the speciality "geology." In 1992 he was awarded the degree of Academician by the Academy of Natural Sciences of the Russian Federation. From 1989, in the Geological Institute of the Academy of Sciences of the USSR, and from 1991 in the Vernadsky State Geological Museum, Tikhomirov, using his profound knowledge and wide-ranging experience, continued to work as a councillor of the Academy of Sciences.

Vladimir V. Tikhomirov died on January 13, 1994, and was buried in Troekurovskoe Cemetery in Moscow. This eminent scientist in the field of the history of sciences of the Earth will forever remain in the memory of all who knew him.

Yu. Ya. Soloviev

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BOOK REVIEWS

Gretchen Luepke, Book Review Editor

CRATERS, COSMOS, AND CHRONICLES: A NEW THEORY OF EARTH. *Herbert R. Shaw. 1994. Stanford University Press, Stanford, California. 688 p. Hardcover, $79.50.*

It is just two hundred years since the publication of James Hutton's *Theory of the Earth* (1795) introduced the basic principles of geology. Ernst F. F. Chladni's 1794 analysis of historical accounts of meteorite falls provides almost the same date for the beginnings of scientific study of meteorites as extraterrestrial objects (some prefer the later date of Biot's study of the 1803 meteorite shower at l'Aigle). The two branches of natural science have since developed with remarkably little influence on each other. Meteoritics has been pursued by a dedicated few attracted to the exotic composition of meteorites or their use as probes of the Solar System. The geological sciences, absorbed in deciphering Earth's history by reading the rocks, have generally given little thought to possible extraterrestrial influences.

There is now a convergence as a deeper understanding of the Earth and its dynamics has come together with results from space exploration to provide a more comprehensive view of the external as well as internal forces working on Earth. Included, after decades of vigourous debate, is a general consensus on the reality of impacts of asteroids and comets with Earth. Presumably, lingering doubts were put to rest by the richly documented impact of the fragments of comet Shoemaker-Levy 9 with Jupiter. Collisions are now well established as a fundamental process in the formation and history of all members of the Solar System. On the rocky and icy planets, impactors leave their calling cards in the form of distinctive scars and, on Earth, in addition, a record of direct influence on the evolution of life through global mass extinctions. The stage is thus well set for texts which seek to synthesize the flood of new knowledge and provide a fresh base for a comprehensive theory of the earth within the Solar System. This is the promise of Herbert Shaw's *Craters, Cosmos and Chronicles: A New Theory of Earth,* a promise, however, that is largely unfulfilled. His book will doubtless win applause for the prominence it gives to ideas on impact cratering, mass extinctions, nonlinear dynamics, seismic tomography and other topics which have tended to lie at the margins rather than the centre of earth science. Yet, despite its breadth and the author's diligence in incorporating recent results, few will find it either a satisfactory summary of insights gained or a useful point of departure for future studies.

Just as, in Einstein's view, God had no use for dice, Shaw's view of the universe has no place for random events. Although he accepts without apparent reservation the reality of impacts, he argues strenuously against bombardment without pattern. The patterns that he discerns of craters in clusters and tracing arcuate lineaments are curiously reminiscent of similar patterns drawn in pre-Apollo mission debates by some of those urging an internal origin for craters on the Earth and Moon. Underlying many of their anti-impact arguments was an abiding faith in the power of processes within the Earth to account for all geological structures and events, allied with a deep dislike for the idea that terrestrial structures could be formed randomly and catastrophically by forces outside Earth's influence. Shaw has now reversed these arguments by his proposition that feed-back mechanisms allow the Earth to exert a measure of control over incoming objects which, in turn, influence magmatic episodes and other events. To do this he postulates the existence of several interrelated features which he considers to have been invariant over much of the geological record. The resultant theory of the Earth, although dressed in the modern cloth of non-linear dynamics, incorporates elements of a decidedly static nature.

He begins with the claim that the distribution of impacts on Earth is not random but is clustered in central North America, Eurasia and Australia. This is scarcely a revelation to those who, over the last half century, have based searches for ancient impact craters on likelihood of preservation over geological time. What is novel is his proposal that crater concentrations are not residual but are the direct result of forces exerting control at the time of their formation. The idea is not supported by any effort at statistical analysis or close consideration of the geologic circumstances at impact sites. Rather it appears to rest simply on personal conviction that potential impactors are stored for a time as near-Earth satellites and that cratering events occur in "bursts" along preferred trajectories whenever there is a "non-linear crisis" in their dynamics.

Shaw dismisses the lack of present evidence for bodies in orbit around the Earth-Moon system as simply an observational problem. He pays little attention to data on observed falls except the Tunguska event of 1908 and the Great Fireball Procession of 1913 which he considers bolster his ideas on preferred trajectories. Similarly, he ignores calculations of the terrestrial impact flux, based on craters being the preserved sample of a statistically unbiased distribution, and the agreement within a factor of two thus found between the flux on Earth, the Moon and the inner planets and estimates of the current population of Apollo and Amor asteroids. Indeed the Magellan mission to Venus has now provided just such an example of a uniform, planet-wide distribution of impact craters, data that Shaw does not address.

In extending his ideas on how the Earth acts, Shaw is even more selective in his choice of evidence. Although it would seem that gravity is the force controlling crater concentrations, the detailed gravity field of the Earth (or other planets) is not analyzed at any point. As proxy we are offered a partial interpretation of the Earth's interior as revealed by one of the several seismic tomography representations now available. From it he selects velocity anomalies at a depth of 2300 km in the mantle as the dominant factor in terrestrial dynamics. There is no calculation of their possible contribution to variations in the Earth's gravity field or consideration of similar or larger velocity anomalies at other depths. His choice of depth derives from a complicated scenario for the early stages of Earth history from which he postulates that the Earth was left with "a stabilizing keel-like structure" deep in the mantle. Despite the motions of mantle dynamics, he suggests that the structure has persisted for as much as 4 billion years and has influenced many geological events, including magmatic episodes, geomagnetic variations and the location of impact craters. He links it to the distribution of impact craters and centres of volcanic activity through ". . . the persistence over time of at least three spatially invariant (crater) clusters . . ." defining three poles which have remained fixed relative to the Earth's spin axis as a "celestial reference frame" (CRF). The frame extends far into space so the Earth in turn is tied to the Solar System through "the dynamics of processes common to both (the orbital evolution of, plus the impact-dynamic effects of, the CRF system of impactors)."

In elaborating on these relationships he embraces the growing field of non-linear dynamics with its extensions into "fuzzy" logic, self-organization, attractors and other themes derived from modern theories of chaos. There is no doubt that non-linear theory and fractal geometry are becoming widely employed in the analysis of natural systems. Shaw makes liberal use of the work of those who have applied them to phenomena as diverse as the behaviour of magnetoelastic materials and the motions of planets, asteroids and artificial satellites while asserting his own notions of terrestrial and cosmic non-linear dynamics. These he expresses with less than crystal clarity in terms such as "chaotic crises, self-organized criticality, and critical golden-mean transitional quasiperiodic/chaotic states" (p. 161).

This is a necessarily small sample of the scope and some of the difficulties of Shaw's "New Theory." Technically the author has been well served by Stanford University Press which has produced clean, fully indexed copy in a handsome volume. His style, however, has resulted in a text which is often an ironic mirror of its author's approach to science. The title of the Prologue is as apt an encapsulation as any: "Reverie, Obsession, and Algorithmic Natural Selection in Stalking the Nonlinear Paradigm in Science." The book comprises several complementary though often repetitive versions of the same themes. The reader is sent back and forth between these various presentations in the Introduction, two sets of overburdened figures, the body of the book, the Epilogue, and 181 pages of notes, while hurdling thickets of references. The result is a tangled labyrinth of thoughts and subtexts in often indulgent "new-speak" by which lack of rigour and clarity are cloaked by numerous reiterations of the non-linear mantra.

William Glen, in a friendly yet cautious Foreword, quotes a review from Archie Roy that the book "will arouse strong feelings, either enthusiastically for or scathingly against, but that is the fate of all new work that threatens traditional views." It is more likely that traditionalists, who may be uneasy with the new concepts emerging from the geology-planetary science convergence, will gain unwarranted solace by equating it with extreme examples of holistic excess. Those more at home with the rich new insights now accessible are more likely to feel disappointment that so much of the broader visions of the Earth in the Solar System and the extraordinary achievements of the last 30 years in planetary science have been distorted in the telling.

Michael R. Dence, 824 Nesbit Place, Ottawa, Ontario K2C 0K1 Canada.

RESTRUCTURING SCIENTIFIC REVOLUTIONS: THOMAS S. KUHN'S PHILOSOPHY OF SCIENCE. *Paul Hoynigen-Huene (translated by Alexander Levine). 1993. University of Chicago Press. 310 p. Softcover, $16.95.*

PARADIGMS AND BARRIERS: HOW HABITS OF MIND GOVERN SCIENTIFIC BELIEFS. *Howard Margolis. 1993. University of Chicago Press. 267 p. Softcover, $15.95.*

If you were wondering what made Thomas Kuhn's *The Structure of Scientific Revolutions* such a threatening and liberating analysis of the history of science, reading these two books can give you some substantial hints. This is not because there is a convergence of belief in these two analyses of Kuhn's seminal text. There isn't even a shared definition of what constitutes a scientific revolution. What is fascinating and telling in both of the books are what they are trying to avoid or circumvent: the social vision of science in *The Structure of Scientific Revolutions.* For these two authors, Kuhn only becomes credible and useful if the significance of the social in his analysis is contained, if not eliminated. For a sociologist like myself drawn to Kuhn for precisely the social theory in his philosophy, this "reading" of Kuhn is provocative and quite interesting. It is provocative because the works seem intent on putting me and my colleagues out of business. It is interesting because developing explanation of historical change (even in the history of science) without a strong sense of social context is actually quite an effort and raises urgent questions about why it all seems necessary. Simple and elegant explanations of scientific work so apparent in *The Structure of Scientific Revolutions* are replaced in these texts with explanative contortions that are sometimes quite breathtakingly difficult to explain and make plausible.

Paul Hoynigen-Huene's (PHH) reconstruction of Kuhn is the more straight-forward of the two books, but it should be called a massive elaboration of Kuhnian notions of the history of science rather than an explanation of his philosophy. PHH begins his text by suggesting that *The Structure of Scientific Revolutions* (SSR) suffered from a lack of philosophical rigor precisely because of the simplicity of the argument. This made Kuhn's work vulnerable to attack from philosophers and open to misinterpretation from non-philosophers who did not understand the traditions from which Kuhn was working. PHH wants to save Kuhn for philosophy by adding parts of the argument that he thinks Kuhn originally left out in SSR, some of which Kuhn has added since and some of which PHH finds implicit in SSR.

Hoynigen-Huene begins by arguing that what was taken as Kuhn's attempt to reduce science to a social system of cognitive control is rather a very serious attempt to inform philosophy of science with the once-new history of science. The point of the new history was to show how past science made sense in its own context, not to show how much better science had gotten through time. By taking the past rather than present-day science as the subject of his philosophy, Kuhn became drawn to long-term stabilities in the history of science, not just those changes deemed as leading toward the present. Revolutions might be in the title of the book, but the most important object of explanation were the periods of normal science. Assuming that past scientists were not knaves or fools, Kuhn, like historians, showed how scientists could have held certain premises and worked in theoretical traditions that we now consider discredited.

This project seemed to many readers, when SSR was published, to be a kind of social reduction. Scientists were pawns inside social forces that made them follow paradigms at one moment and reject them the next. PHH wants to show that this is not what Kuhn meant (or came to mean) at all. Scientific revolutions were not socially stimulated and regulated gestalt switches, but changes in scientific practice around a pattern of ostention. Scientists, rather than following theoretical traditions, work in relation to representative problem solutions which are held as ideals inside their communities. Scientists are trained to organize their thinking and work around these exemplars, which provide them with models for both theorizing and research practice. The community of scientists in a discipline would not so much constrain the thought of individual scientists as organize it around these paradigmatic exemplars.

Individual scientists, in PHH's version of Kuhn, are not controlled by paradigms or their communities as much as given a common focus through their training in the lexicon of their disciplines and similarity relations developed around the paradigmatic exemplars. In periods of normal science, scientists live in phenomenal worlds organized with these cultural tools that they take as the natural world, but they do not develop their phenomenal worlds to suit a system of social control that governs their cognition and behavior.

During scientific revolutions, PHH argues, new phenomenal worlds begin to take over the imagination of some scientists. Old exemplars lose their cultural significance and cannot any longer organize the work of scientific disciplines. The assumed seamlessness of the phenomenal world of scientists and the object world is made problematic, and a proliferation of phenomenal worlds results. The revolution is over when a new set of exemplars and a new shared phenomenal world (or limited number of worlds) emerges from the community of scientists. For PHH, revolutionary science is less problematic than normal science. Kuhn's revolutions are interesting to the extent that they provide the opportunity for reconstituting similarity relations taken-for-granted within scientific disciplines in periods of normal science. Revolution disrupts culture, and the fabric of language-based possibilities for thought and action is destroyed until a new one can be constituted. Like a period of molting, this destructive shedding allows a kind of rapid but less contained growth, contrasted with the more sustained and more directed growth periods of normal science.

Leaving aside for the moment what constitutes scientific growth, and how the social is both important to and irrelevant to it, let us turn to the other book on Kuhn at issue here, and see how it takes SSR as a starting point for arguing about scientific development.

Harold Margolis' (HM) *Paradigms and Barriers* has actually very little use for Kuhn's philosophy of science. It begins from the premise that scientific revolutions are at heart cognitive shifts requiring some more careful elaboration than Kuhn gives them in SSR. Notions of gestalt switch need to be replaced by more recent and credited notions from cognitive science. HM explains periods of normal science as ones in which the habits of mind of scientists are deemed useful enough to merit continuation. Revolution occurs when the habits can no longer be sustained in the face of evidence contradicting them. They feel revolutionary because habits of mind, like any other personal habits, are very difficult to change. Paradigms, by this account, are aids to learning that become barriers to innovation. Seeing how the barriers have been constructed and then overcome in historical examples are meant to tell us how paradigms work in science.

All the rest of SSR, the attempt to write a philosophy of science based on scientific disciplines and their activities rather than individual minds and their cognitive styles, is simply ignored. Even though Margolis uses historical examples to document his theory of cognitive resistance and change in science, his is a much more conservative piece of historical explanation than Kuhn's. Periods of normal science are reduced to periods in which habits of mind govern research until they are so poor in explaining what scientists are seeing that they are finally dropped. Margolis contends that the evidence for a new scientific paradigm is more often than not available long before the paradigm is accepted. The reason scientists are unable to see this, he argues, is that scientists develop habits of mind that inhibit their ability to absorb other ways of thinking. All scientists are vulnerable to these habits. HM suggests that if you find it hard to give up smoking, you should try giving up habits of mind.

HM's work on paradigms, then, resists Kuhn's social explanation of the history of science. HM is vociferously against sociology of science, and against attempts to explain scientific change as a fundamentally social activity. Science may be an institution and require group participation, but what drives science to HM are individual minds comparing models of the world with the world itself. He has no room for PHH's ideas about how scientific communities train their members by ostention. There are no exemplars from science in HM's model around which group life in scientific disciplines is organized. Habits of mind may be learned but the social conditions of their learning are not part of the theory and the social reinforcements for them that help sustain them in the face of contrary evidence are also not part of this picture.

The mentalism of the theory is quite at odds with Kuhnian explanations of science, but the notion of habits of mind could indeed improve the cognitive side of Kuhn's analysis. Like the notion of paradigm, habits of mind could help make sense of collective scientific practices. In a period of normal science, habits of mind would clearly facilitate coordination among scientists trained into similar ways of thinking. But this kind of social explanation is not what interests HM. In fact, he wants to avoid any hint of the social because he thinks social studies of science are inherently relativistic. He believes in scientific realism, but he wants to explain how it can be faulty. He uses the notion of habits of mind to point to a strain of irrationality in the rational pursuit of science. Scientists temporarily avoid data not comfortably rolled into their existing theories not because they think the data is suspect, but to learn from it would require a mental restructuring that would be uncomfortable. Habits of mind, in other words, keep scientists from simply taking the measure of the world without bias.

The functioning of habits of mind in scientific thinking could indeed be interesting, but where do the habits of mind come from? Why are they there? HM argues that they come mostly from earlier perceptions. Science to HM is not so much a linguistically based or structured activity but rather an organized and directed form of perception. Scientists observe the world or organize experiments in which they can observe the results. Observation, as always, is shaped by expectations that affect what is seen. Habits of mind can thus quite easily keep scientists from literally seeing what is in front of their eyes. The shift between normal and revolutionary science in the history of science, then, is simply a matter of habitualized perceptions alternating with ones radically breaking with expectations.

As an explanation of what can happen to the beliefs of scientists during their careers, this might be a very useful model, but as a theory of the history of science, it is pretty weak. Ptolemaic images of the universe, according to HM, were sustained so long not because of historical social and cultural forces like Christianity in Europe, but because of habits of mind. People used to thinking about glass spheres could not easily drop that conception. The problem for HM is simply one of individualized pattern recognition, so it is never muddied by such collective forces in history as cultural commitments or social desires. HM thinks that social explanations of science are bad because he thinks sociologists refuse to acknowledge that sometimes science is right, and new ideas are accepted because they are better than old ones. This is certainly true for most of the negative exemplars he chooses to discuss, but not for the field as a whole. It is also not quite right to argue that if sociologists are going to be agnostic about scientific truth, HM needs no social factors to explain the history of science. This logic is suspect to say the least.

Both PHH and HM are intent in their own ways on using Kuhn to restore a kind of seriousness to the history and philosophy of science by saving realism and keeping sociologists at bay. Kuhn's book let the social cat out of the bag of the history of science, and the effects can only be fixed by limiting the readings of and damage resulting from SSR. Kuhn made disciplines rather than individuals authors of history of science. This is fine according to PHH, but some readers have taken this social turn as a retreat from realism. Kuhn's version of scientific knowledge is both subject-sided and object-sided; science is always a response to characteristics of both the natural world and social worlds. So science can be social in PHH's Kuhn as long as it is based on reality. The social is also limited in PHH's approach to linguistic features of the social worlds of science, in other words, the culture of science more than its social forms. Communities may sustain normal science and stimulate revolutions in science, but they do so as sites of meanings and practices, not of groups vying for power or reputation. HM depicts scientists as more human, in a way, because they are driven by irrational as well as rational forces, but he will not give their relations to one another (the social) an inch in his view of the history of science. Mentalism is, I suppose, a habit of mind difficult to give up.

In these troubling times of budget cuts for science, there is good reason to fight to keep the reputation of the sciences in the best possible shape. There is good reason for the proponents of realism to say that science is to be valued because it really is about something out there—the world. But this is no reason to deny the fact that science is the product of group efforts. Science is made by unknown lab technicians as well as great thinkers. Scientific learning may be an historical enterprise in which large movements shake the foundations of thought and action, but it is still a routine activity in most periods only sustained because so many people participate. Accumulation of data is often a tedious, heroic, collective endeavor and to deny this by saying that social explanations of science are wrong or misleading seems to me to denigrate much of the work that goes into science and toward which scientific funding is and should be earmarked.

Chandra Mukerji, Department of Sociology, University of California–San Diego, La Jolla, California 92093.

HENRY DARWIN ROGERS, 1808–1866, AMERICAN GEOLOGIST. *Patsy Gerstner. 1994. University of Alabama Press, Tuscaloosa. 311 p. Hardcover, $49.95.*

Prior to this book by Gerstner, the biography of Henry Darwin Rogers had to be inferred from separate sources: the letters of his brother, William Barton Rogers, edited by William's widow; a 1916 address by J. W. Gregory to the Glasgow University Geological Society; and the publications of the Pennsylvania state geological survey, including comments by Rogers' successor at the survey, J. P. Lesley. The first of these is a family document, presumed accurate as far as it goes. The second is a considered review of Rogers' life, but clearly is intended to add luster to the department which Gregory chaired. The third is tough sledding and, where Lesley is concerned, requires discrimination. Gerstner draws on these and a wide range of other sources to put in one book a coherent view of the life of Henry Darwin Rogers.

The strengths of this book are in its detailed reporting from primary sources on specific phases of Rogers' life: for examples, Rogers' dealings with the Pennsylvania legislature while trying to publish the *Geology of Pennsylvania,* or the actions of his opponents in the campaign against his candidacy for the Rumford Professorship at Harvard. In these strengths are also its weaknesses: there is less space for larger questions in geology and the wider world, and the prose often creaks along under its load of facts.

This is one of several recent biographies by the University of Alabama Press that bring to our attention 19th-century American scientists active in social causes who have contributed to geology. Henry Darwin Rogers held well-articulated liberal views, particularly on the education of the working class and the abolition of slavery, which affected his career favorably by bringing him in contact with the science elite in both Great Britain and the United States, but which worked against him when the power was held by more conservative authorities. Gerstner provides evidence that, throughout his life, Rogers did not trim his views to fit his opportunities. Because of this, he was fired from his first real academic appointment (Dickinson College), and his opponents could more easily pass over him for the Rumford Professorship. Later, his abolitionist, pro-Northern activities in Great Britain probably dissipated his scientific opportunities there. On the other hand, his catastrophic theories in geology were throw-backs to an earlier time, and got him at least initial support from Murchison, Sedgwick, and the conservative rear guard of his day.

As geologist, Henry Darwin Rogers directed the first Pennsylvania state geological survey, made the first accurate regional description of folded Appalachian rocks, and attempted to classify the Paleozoic rocks of these mountains and to explain their folding. His principal legacy to American geology are the maps, sections, and descriptions of folding which accompany the final report of the first Pennsylvania survey. Although he accurately described the structure of the folded rocks in Pennsylvania, his theoretical explanation of these folds came from the outer fringes of catastrophism, whether judged in his or our time. He held the chair of Natural History at the University of Glasgow for the last eight years of his life.

His life and work were intimately tied to his family. Henry was the third of four scientifically-inclined brothers. Among them, his scientific contributions probably rank second to those of William, who did similar geologic work in Virginia and later founded the Massachusetts Institute of Technology. The brothers' correspondence indicate close family ties, a reliance on William for leadership, and ambitions toward academic posts, fostered by personal inclinations and family environment.

Henry Darwin Rogers' particular middle name came because Henry's father admired Charles' grandfather, Erasmus. The flowery style of the brothers' correspondence does suggest the elder Darwin's poetry, and this formal approach to words carried over to their work in geology. Henry and William developed a classification of Paleozoic rocks that was radically different from the classification used by the neighboring New York state survey. The New York survey gave rock units place names from the sites where they were first well described, and relative ages from their fossil content and positions in the inferred section. This the Rogers brothers thought premature: "none of the existing systems of nomenclature—neither the imported British one, nor the narrowly local geographical one of New York—were applicable to our strata.'' As an example of their solution to this problem, they would have called the Trenton limestone of the New York survey by the provisional phrase ''Matinal newer limestone'' (with relative age further indicated by a Roman numeral), until such time as the exact relations between rock units could be worked out (Gerstner, 1979). Rogers did not foresee the empirical function of a stratigraphic place name like 'Trenton,' which came to imply a certain slabby limestone with particular time relations derived from fossils. The geographic source of the name (Trenton Falls, north of Utica, New York) was divorced from its geologic meaning, although, as Henry and William foresaw, there have been adjustments to the position of the Trenton within the original Paleozoic section of the New York survey. Faced with a choice between a single place name and a phrase from a classical, abstract time classification, the geologists of Henry's time ignored his scheme.

When Rogers took up his professorship at the University of Glasgow, his reputation was declining in America. His appointment at Glasgow may have been made over the heads of his scientific peers, because of his friendship with the Duke of Argyll. He began lecturing six months after the founding of the Glasgow Geological Society, but in the fifty-year Jubilee History of that organization (by then, the Geological Society of Glasgow) his name is absent, even though he held the principal geologic post in the city. The revitalization of the Hunterian Museum and the hiring of John Young, attributed by Gerstner to Rogers based on Museum minutes, is described in the Jubilee History without mentioning Rogers. Gregory wrote part of this Jubilee History, and it was published while Gregory was president of the Society.

At the University, Rogers and Kelvin occupied neighboring houses when Kelvin's interests in geology were intensifying, but the only mention of Rogers in Thompson's two-volume biography of Kelvin is of Rogers acting as a stand-in after Kelvin broke his leg. Kelvin gave Rogers 100 guineas for taking over Kelvin's lectures (the amount was roughly one third of Rogers' annual salary), and Kelvin implies that Rogers was a less than satisfactory representative to read his (Kelvin's) paper on the age of the sun's heat. From the record, Rogers does not appear to have made a notable contribution to geology while at Glasgow; he was an 'American Geologist' as the subtitle states.

A close reading of Gerstner's book suggests hypotheses on the origin of Rogers' geologic opinions that might be worth examining by historically-inclined geologists. Here are three hypotheses: (1) Rogers had a low opinion of fossils for correlating rocks. Did this come, in part, from an early recognition of the effects of facies change on fossil content? (2) Rogers supposed the folded Appalachians were frozen waves of monster earthquakes traversing a molten crust. Were these ideas prompted by observations he made at the iron furnace he operated near Pittsburgh? (3) Rogers mentions 'bedded granite' with an implication of metamorphic origin, although he clearly demonstrates intrusive igneous relations elsewhere. Did Rogers have a sedimentary perspective on the origin of granite?

Cyril Galvin, Coastal Engineer, Box 623, Springfield, Virginia 22150, USA.

ROGUE ASTEROIDS AND DOOMSDAY COMETS. *Duncan Steel. 1995. John Wiley and Sons, Inc., New York. 308 p. Hardcover, $24.95.*

This book begins with a foreward by the eminent Arthur C. Clarke, who, contrasting the book with his own work of fiction, *The Hammer of God,* writes that ''the strands of fact and fiction are becoming inextricably entwined.'' This is not only true when comparing the two volumes, but, sadly, also an accurate characterization of *Rogue Asteroids and Doomsday Comets.* As the author points out in his preface and introductory chapter, this book also has an agenda that is as much political as it is educational. Its purpose is to argue that ''searching out all large Earth-crossing asteroids is an important task that we *should* tackle, and that we *can* tackle.'' Such a political agenda is not necessarily bad, but it needs to be remembered when reading the book. The book is not simply a treatise of scientific work being made accessible for the non-specialist.

In Chapter 1, the author outlines the threat of impacting asteroids and comets, discusses the once-predicted (now-dismissed) collision of comet Swift-Tuttle with Earth in the year 2126, and then introduces the concept of Spaceguard, which is a system designed to detect and possibly deflect threatening bodies. In Chapter 2, the discovery of asteroids by astronomers is reviewed, which leads to a discussion of their population and the probability that asteroids (and comets) could collide with Earth. Chapter 3 follows with descriptions of some of the effects produced by an impact event and, to put them into a perspective that is easier to understand, compares the number of predicted human fatalities with the number of fatalities associated with more familiar hazards (e.g., automobile accidents and floods). With repeated reference to the K/T boundary impact event(s), the author then explores several other impact effects in Chapter 4, particularly those that occur in the atmosphere. He also argues that a comet does not have to hit the Earth to be hazardous; rather, if the Earth were to pass through the coma or tail of a large comet, the dust in it could alter Earth's climate by shielding the surface from sunlight.

At this point in the book, it became increasingly difficult to overlook its flaws. As the author admits, the effects of comet comae and tails may sound like a ''wild story'' to some people, but he then argues that the K/T boundary impact event(s) provides ''hard evidence'' supporting it. Unfortunately, the ''evidence'' is a gross misunderstanding of the stratigraphic record at the K/T boundary. In a leap of logic that is hard to grasp, he then goes on to suggest that there may be many other Chicxulub-size craters waiting to be discovered at the K/T boundary. This too is naive. He apparently does not appreciate that the stratigraphy of the boundary sediments and the volume of the boundary sediments indicate that the Chicxulub impact event provided the bulk (if not all) of the impact ejecta at the K/T boundary. There is no evidence to suggest there is a second or third 180-km diameter or larger crater.

Chapter 5 reviews the record of impact craters on Earth and includes a historical account of G. K. Gilbert's study of the origin of Barringer (or Meteor) Crater in Arizona. This chapter also reviews the ages of impact craters and explains how older craters are more likely to have succumbed to the geologic processes that constantly modify the surface of the Earth. Chapter 6 extends the discussion of crater ages in the context of periodic bombardment, relating a perceived 26-million-year cycle to patterns in the fossil record of extinctions. He also tells the reader that the Deccan Traps erupted when seismic energy produced by the Chicxulub impact event was focussed on the far side of the world.

I paused again at this point in the book because of the baldness with which these conclusions were presented. For example, how can the Deccan Traps have been produced by the Chicxulub impact, when the best dates available indicate the trap eruptions began before the Chicxulub impact event? Also, I am not yet convinced that the cratering record contains a periodic component. I note that the criticisms of periodicity made by geologists who study and date impact craters were not addressed. Instead, only the alternative perspectives (mostly by those in non-geological fields) were presented. In any case, if there is a periodic component as the author conveys, then it is supposed to be among the population of long period comets, not asteroids. For that reason, the issues of periodicity and hazardous comet comae seem to depart from the author's theme about the hazards of near-Earth asteroids and the need to detect them.

An additional departure follows in Chapters 7 and 8, where the author argues that there is a core of material in the Taurid stream of cometary debris, that this debris is the dominant hazard to humankind, and that it is the previous interaction of this complex with Earth 5000 years ago that led our ancestors to build Stonehenge and the Egyptian pyramids. It is hard to evaluate the hazard of the Taurid complex from what is written. The conclusion is based partly on orbital calculations, for which the author is considered an expert. On the other hand, he supports the idea of previous Taurid impact events with dubious data. For example, he says the Farmington meteorite was part of the Taurid comet stream. However, the Farmington meteorite is a thermally-metamorphosed and shock-metamorphosed ordinary chondrite, which is a type of object that decades of geologic and astronomical research indicate came from the asteroid belt. If the author wants to sweep this interpretation aside, it is incumbent on him to provide evidence in support of his alternative hypothesis.

The physical evidence at the two sites is pretty thin. For example, the author argues that a gap in the wall at Stonehenge faces the rising sun because the detonations of Taurid objects occurred at night and the occupants would anxiously be waiting for dawn. It seems simpler to me to say that the gap was there because the sun rose in that direction, not because of something that occurred beforehand. With regard to the pyramids, he suggests their shape was chosen because it resembles the wedge of zodiacal light in the night sky which would have been particularly bright if his Taurid stream calculations are correct. The author, in the end, admits that his modeled origin of Stonehenge and the pyramids is only supposition. It is thus unclear to me why, if the author realized that the work was so speculative, he would include a 30-page-long description of it in what is supposed to be a professional trade book in which he is trying to build a credible argument in support of Spaceguard.

Having argued that cometary fragments caused spectacular effects 5000 years ago, the author then reviews the consequences of the Tunguska event in 1908, when a fragile body exploded a few kilometers above the Siberian taiga. Chapter 10 explains how our detection of asteroids and comets has evolved in modern observatories and then introduces the very successful Spacewatch program at the University of Arizona, which is currently detecting ~25 new near-Earth asteroids and ~20,000 additional asteroids each year. Chapter 11 has a description of the Spaceguard concept and the work of the Detection Committee, which was assembled to review the potential effects of an impacting object and assess the continuously expanding catalogues of asteroids in near-Earth space. The possibility of intercepting threatening asteroids (and comets) is discussed in Chapter 12. The discussion of Spaceguard is concluded in Chapter 13, and it is there that the reason for the departures from the author's theme in Chapters 6, 7, and 8 becomes clear. The author says that he supports the Spaceguard project, not because of the perceived threat of near-Earth asteroids, but because he believes that it will prove his theory that streams of small comet fragments rather than larger isolated asteroids are the worst hazard in our future.

Appended to the book is a section of notes and a bibliography, both of which are keyed to specific chapters. The bibliography is very shallow, however, and does not contain the depth that would be useful for a historian. I note, for example, that while the Chicxulub crater was mentioned repeatedly and the environmental effects of that impact discussed, not a single reference was made to those involved in its discovery, the dating of the structure, the production of SO_2, or any other area of subsequent geological analysis. In those cases where an attempt was made to cite investigators, it was usually not the primary source. On the other hand, there is a nice glossary and index in the book.

The book closes with an Epilogue that briefly summarizes the collision of comet Shoemaker-Levy 9 with Jupiter in the summer of 1994. In this Epilogue, the author makes it clear that he believes it is important to keep the public informed about impact hazards. I agree. However, it is also important not to mislead the public. Many of the arguments being made in the book are built on supposition, as-yet controversial theories, and, sometimes, erroneous "facts." The author is taking advantage of an unapprized public, presenting cases that have yet to withstand the review of knowledgeable peers. These cases often appear stronger than they really are because of the way in which facts (as best science can define facts) are weaved with speculative ideas. The hazards of impacting asteroids and comets are frightening by themselves and do not need to be exaggerated. Personally, I too think it is important for us to be aware of these hazards and to identify near-Earth or Earth-approaching objects (the Spaceguard concept). Unfortunately, I think books like this one may jeopardize science's credibility. Certainly, many of its concepts should be avoided by educators whose students may assimilate the anecdotes as facts.

David A. Kring, Lunar and Planetary Laboratory, University of Arizona, Tucson, AZ 85721 USA.

IN MARBLE HALLS. GEOLOGY IN TRINITY COLLEGE, DUBLIN. *Patrick W. Jackson, ed. 1994. Department of Geology, Trinity College, Dublin, Ireland. 135 p. Softcover, IR £7.00 (incl. postage and handling; make checks payable to Patrick W. Jackson).*

Publication of this book marks the 150th anniversary of the establishment of the Chair of Geology at Trinity College, Dublin, Ireland, in 1843. This commemorative volume has been organized into two parts: (1) the history of the geology program, authored by Patrick W. Jackson; and (2) recollections by former staff, students, and faculty.

The Preface to the book, under the authorship of Wallace S. Pitcher, who shared his undergraduate experience with me at the University of London, describes the history of Trinity College as an observer from a distance, rather than as a former student or faculty member. He points out that the mid-19th century witnessed a remarkable blossoming of scientific effort in Ireland, marking a golden age for Irish geology. The initial appointment went to William Smith's nephew John Phillips, who was one of the most brilliant contributors to geology in the 19th century. Unfortunately, his stay at Trinity College lasted only one year. Thomas Oldham replaced Phillips; Oldham's reputation has survived to this day through his discovery of one of the earliest trace fossils then known and named *Oldhamia* after him. Oldham moved on to become superintendent of the Geological Survey of India, and Samuel Haughton, who introduced a chemical system for the classification of granites, was next in line. William Sollas, who studied microfossils and chert in chalk, followed Haughton. John Joly brought Trinity College into the 20th century with his contributions concerning the nature and age of the Earth's crust.

This book relates to achievements of the various faculty, staff, and even students of the college. Their achievements are outstanding and literally amazing. Most of the faculty reached the prestigious pinnacle of fellowship in the Royal Society and presidency in Section C (Geology) of the British Association for the Advancement of Science. Fossils or minerals were named after them. The only faculty member whom I personally knew was William D. Gill, who headed the department between 1953 and 1961 and was responsible for the construction and expansion in the 1950's. (My acquaintance with Gill was in his later life, when he held the chair of petroleum geology at Imperial College, University of London.)

At Trinity College, expansion resulted in new facilities, which were officially opened in 1956 by the Earl of Iveagh, Chancellor of the University, after a ceremony during which honorary degrees were conferred on the three eminent geologists H. H. Read, Ph. H. Kuenen, and Sir Harold Jeffreys. My nostalgic recollections include H. H. Read, whose lectures I attended as a student; Ph. H. Kuenen, as whose host I served when he received Honorary Membership in the Society of Economic Paleontologist and Mineralogists; and the Countess of Iveagh (not the Earl himself) was my chemistry partner in my undergraduate days at the University of London.

The second part of the book consists of tall stories, including an undergraduate perspective from 1984 to 1988. These tales were invited and requested to be "easy-going, anecdotal, minimally libelous, and neither stodgy nor boring." For those who related the tales or witnessed the action, the stories are entertaining; others may find them a bit dull.

I noted one serious omission in this book. William Smith received an Honorary Degree from Trinity College. English, Scottish, and Welsh universities failed to honor him, whereas Trinity College honored itself by honoring Smith. This distinction should have been emphasized and some of the background provided, because Smith received this honor as a cartographer, and not as a geologist.

Gerald M. Friedman, Brooklyn College and Graduate School of the City University of New York, Brooklyn, New York 11210 and Northeastern Science Foundation, Rensselaer Center of Applied Geology, P.O. Box 746, 15 Third Street, Troy, New York 12181.

DEVONIAN PALEONTOLOGY OF NEW YORK. CONTAINING THE BRACHIOPODS, BIVALVES, ROSTROCONCHS, GASTROPODS, TERGOMYANS, AMMONOIDS, TRILOBITES, EURYPTERIDS, AND PHYLLOCARIDS; BASED ON THE LITHOGRAPHS OF JAMES HALL AND JOHN CLARKE. *David M. Linsley. 1994. Special Publication 21, Paleontological Research Institution, Ithaca, New York. 472 p. Softcover, U.S. $24.95.*

This book is not *about* history—it *is* history, an updated version of an important document in the history of paleontology. James Hall was New York State Geologist and Paleontologist, primarily the latter, from almost the founding of the State Survey in 1836 to Hall's death in 1898. John M. Clarke succeeded Hall as State Paleontologist, became State Geologist in 1904, and served until *his* death in 1925. The 8 volume (in 13 quarto volumes) *Natural History of New York, Part VI, Palaeontology,* 1847–1894, is a monument to Hall; it was largely his work, and what wasn't his work (opinions differ on this) was inspired, driven, and made possible by him. Volumes VII and VIII (3 quartos) are by "Hall, assisted by Clarke;" Clarke is thought to have been the principal author. Those wanting more information and discussion of Hall, Clarke, and the authorship of the *Palaeontology of New York* should go to Clarke's *James Hall of Albany* (Albany, 1921) and the James Hall issue of *Earth Sciences History* (v. 6.1, 1987, Fakundiny and Yochelson, eds.). Whatever the results of any analysis, the *Palaeontology* is a 19th-Century work that is still of enormous importance.

Linsley's *Paleontology* is a contemporary guide to most of the Devonian part of Hall's *Palaeontology* (vols. III–VIII; 11 quartos). Its most important contribution is 2-fold; updated names of genera and species and the reproduction of many of the original illustrations. Hall's work has held up very well; his illustrations are excellent and most of his species are recognizable and still good, but genus- and higher-level revisions by succeeding generations of paleontologists have made it difficult to use the original volumes as a reference, except by specialists. In addition, the original volumes have become very expensive although available "new" up to 30 or 40 years ago. This one-volume handbook will be useful to professional and amateur paleontologists, whether or not they have access to the originals, and to those interested in geology who simply want to identify a fossil or to see what a particular species or genus looks like.

Linsley considers his book to have four principal parts: introduction, brief descriptions of the kinds of animals represented, an outline of the Devonian stratigraphy of New York, and the plates. The second (8 pp.) and third parts (34 pp. plus a 12-page bibliography) are aimed at the non-specialist although even the most specialized are likely to find them useful reviews and handy references. Each of the 342 plates includes from one to 40 or more figures and illustrates from one to eight species. These are not just reproductions of the Hall plates. Species are arranged in systematic order within the major groups listed in the subtitle. Most of the figures are from the *Palaeontology,* but they are rearranged and supplemented by figures from other publications: a few by Hall, several later works by Clarke, and a few by other authors. Species names are at the bottom of each plate and the plates are preceded by species lists in plate order (24 pp.) and the ranges of the species in New York (10 pp.). The species lists cite the source of each figure.

Linsley's coverage of the brachiopods, mollusks, and arthropods in Hall's *Palaeontology* is extensive, but not quite complete. Omissions are explained in the description of the species lists. Corals, bryozoans, crinoids, nautiloids, and a few smaller groups, although common in the Devonian of New York, are omitted, apparently because they are less readily identified without extensive preparation (p. 3).

Linsley's *Paleontology* was not written for historians and should not be judged by how appropriate it is for readers of this journal. The Introduction (14 pp.) is the only part of the book that recounts earth science history. There are biographical sketches of both Hall and Clarke, a comparison of the early stratigraphic terminology with that of today, and notes on Hall's plates. The latter include a description of the lithographic process and a list of the artists employed in the production of each volume. It is worth noting that S. M. Hall and M. E. Brooks, two of the three artists listed for volumes I and II, were Hall's wife and her sister (Blum, 1987, *Earth Sciences History,* v. 6.1, p. 72–85). Hall seems a little more human when one realizes that the early works were family affairs. Of greater importance is to recognize the number of Hall's "artists" who became important paleontologists in their own right.

Linsley includes, as an appendix and for no evident reason, Clarke's story of the Cardiff Giant, taken from his *James Hall of Albany* (Albany, 1921). This is so amusing and revealing of the state of the science at the time, as to need no other excuse for inclusion.

To end as I began, there is very little in this book on or about earth sciences history, but there is a great deal that points to a particular aspect of history and its continuing interest and importance. The book will be a useful reference for all kinds of geoscientists and will be of inestimable value to those interested in or working on the Devonian geology and paleontology of New York and surrounding areas.

Oh, yes, to answer the inevitable question, tergomyans are monoplacophorans, one-shelled molluscs, thought by some to be "primitive" and ancestral to the other groups.

William A. Oliver, Jr., U.S. Geological Survey and Department of Paleobiology, U.S. National Museum of

Natural History, MRC 137, Smithsonian Institution, Washington, D.C. 20560.

THE HISTORY OF MINERAL COLLECTING, 1530–1799, WITH NOTES ON TWELVE HUNDRED EARLY MINERAL COLLECTORS. *Wendell E. Wilson. 1994. The Mineralogical Record, Tucson, Arizona. 243 p. Softcover, $24.00.*

This volume of the *Mineralogical Record* (V. 25, no. 6) is profusely illustrated by photographs and reproductions of crystals, portraits of noted mineral collectors and dealers, and views of mineral-specimen rooms of noted museums and private collections. Many of the photos and illustrations are in color. There are also numerous reproductions of pages from books and collection catalogs.

This work is an extensive survey of mineral collectors and their collections from the 16th to the end of the 18th century encompassing France, Great Britain, Germany, Austria, Hungary, Bohemia, the Low Countries, Switzerland, Italy, Russia, Scandinavia, Spain, Portugal, Mexico, Brazil, and America.

The volume is divided into four parts: (1) collectors and their collections; (2) census of mineral collectors, 1590–1799; (3) bibliography of collection catalogs; and (4) general bibliography. There is also a brief discussion of public mineral museums, as well as mineralogical societies and magazines.

Part 1 describes each collector and his/her collection in brief, with accompanying illustrations, photos, and portraits wherever possible by country of origin. There is a brief biography of each individual, which can be followed up by checking the references provided in Part 2, the census of mineral collectors. For example, the section dealing with James Sowerby discusses his life, collections, books, mineral-collector friends, and what became of his collection. There is a color reproduction of his portrait and two pages of excellent color reproductions of specimens from his collection. The article on Rene-Just Haüy also includes a color reproduction of his portrait as well as color photographs of a few of his specimens, with his hand-written labels, that still exist.

The census of mineral collectors gives an alphabetical synopsis of each collector's life, along with such information as collection size, who purchased the collection, where it now resides (if possible to trace), and references to books and/or articles listed in Part 4, the general bibliography.

The bibliography of collections catalogs is arranged alphabetically by collector's name and complete catalog title. It also includes numerous reproductions of selected catalog title-pages.

The general bibliography is very thorough and shows that the author spent some time in tracking down references in various libraries around the world.

The History of Mineral Collecting is an excellent compilation of data on mineral collectors and their collections, and also provides a tantalizing glimpse of books on crystal collecting and collecting as well as where these can be found in various museums. This volume of the *Mineralogical Record* complements *Mineral Museums of Europe* by Burchard and Bode, and other selected articles on collectors and collections that have previously appeared in this journal in previous issues. Hopefully we can look forward to an overview of mineral collectors and collections dealing with the 19th and 20th centuries sometime in the near future.

Robert M. Rich, Ph.D., Rich Associates, 695 Sierra Vista Avenue, Suite 3, Mountain View, California 94043.

INTERESTING PUBLICATIONS

Since the start of this journal, Founding Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our members are requested to send them to Gerald M. Friedman, Brooklyn College and Graduate School of the City University of New York, ℅ Northeastern Science Foundation, Inc., Rensselaer Center of Applied Geology, P.O. Box 746, Troy, NY 12181–0746 U.S.A.

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ANNOUNCEMENTS

MEETINGS AND CONFERENCES

The **German Geophysical Society** will sponsor a half day session on the History of Geophysics during its **Spring 1997** meeting in Potsdam. Prospective participants should contact the committee of organization: either Dr. W. Schröder, Hechelstrasse 8, D-28777 Bremen-Rönnebeck, Germany, or Dr. E. Unterreirmeier, GeoForschungszentrum, Telegrafenberg C1, D-14473 Potsdam, Germany. **Contact by March 1996**. Papers 15–20 pp. Papers will be published; page charges DM 15 per page.

To celebrate the 20th anniversary of the foundation, by François Ellenberger of the **Comité français d'Histoire de la Géologie**, the committee will sponsor a joint meeting, with the **Société Géologique de France** on the History of Geology in Paris on **June 12, 1996**. There will be three proposed but not exclusive sessions: discovery and evaluation of source materials, development of geology and paleontology in the 19th and 20th centuries, and grand epistemological themes: earth, space, time structures, life. **Deadline January 31 1996.** Address correspondence to: Jean Gaudant, Secretary of COFRHIGEO, 17 rue du Docteur Magnan, 75013 Paris; or to Eric Buffetaut, Société Géologique de France, 77 rue Claude Bernard, 75005 Paris.

The Drake Well Museum in Titusville Pennsylvania will sponsor, as the 1996 Coming Home to the Valley Symposium, a meeting on The History of **Oil and Gas Exploration in North America**, **July 18–21 1996. Deadline** for one page **Abstracts** is **April 10, 1996**. Additional information from organizer, William R. Brice, Geology Department, University of Pittsburgh at Johnstown, Johnstown, PA 15904 [bbrice@upj.pitt.edu], who should also receive the abstracts. Registration is $125.00 US, by June 1, 1996 to Anne Stewart, The Colonel Inc. RD #3 Titusville, PA 16354. make checks to "The Colonel". Hotel is The Inn at Franklin, 1411 Liberty Avenue, Franklin, PA 16323 1-800-535-4056, rate is $53 US per night. **Co Sponsors** include **HESS** and **GSA History Division** as well as **Northeastern Science Fdn**.

The **American Heritage Center** at the University of Wyoming (Laramie) announces its **1996 Symposium: ''Rocks, Rails and Ranching''**, to be held **September 26–28 1996 at the University of Wyoming** to consider economic and natural uses for western lands. Suggested topics for papers include early geological and paleontological expeditions and surveys, development and establishment of national parks and forests, development of the state survey, mining history, petroleum exploration, cattle drives and emigrant trails, patterns of land settlement and other matters consistent with the symposium's theme. Program proposal form is available from Tom Wilsted, Acting Director of the American Heritage Center. Phone 307-766-6811, Fax 306-766-5511 and e-mail: tomw@uwy.edu. **Proposal deadline February 16, 1996**.

HISTORY OF GEOLOGY GOES ELECTRONIC!

History of geology and geosciences now has its own webserver on the World Wide Web, organized by Dean Dunn with support from NSF, with a server at University of Southern Mississippi. It is named **GeoClio**, and its web address is **http://geoclio.st.usm.edu/** Register as a friend of GeoClio! Earth Science History tables of contents from Vol. 1 No. 1 to Present will go on line sometime this year.

AWARDS

Samuel T. Pees, a contributor to ESH and a historian of the early Pennsylvania oil industry, has been named Historian Emeritus of the Drake Well Museum, Titusville, PA.

François Ellenberger received the 1994 GSA History of Geology Division History of Geology Award (in absentia) at the 1994 GSA Meeting in Seattle.

Gerry Friedman will receive the Distinguished Educator Award of the American Association of Petroleum Geologists in May 1996 at the society's meeting in San Diego.

NOTABLE PUBLISHING EVENTS

The American Meteorological Society will publish in December 1995 **Historical Essays on Meteorology 1919–1995,** edited by James R. Fleming. Order from AMS 45 beacon Street, Boston MA 02138-3693.

In 1996 Cambridge University Press will; publish Steven G. Brush's long awaited **A History of Modern Planetary Physics**, in three volumes. Steve is an editor of ESH and one of the world's most accomplished historians of science. 20% discount on orders before May 31, 1996 -order from Dept. PJL Cambridge U. Press, 40 West 20th St. NY, NY 10011-4211

BILL NEEDS HELP!!

Bill Sarjeant is publishing his three-volume **Supplement 2 (1985–1995)** of the indispensible **Historians and the History of Geology: An International Bibliography from the Origins to 1978.** However, unless there is a good presale, the publication may be canceled. Get your library to order this 2350 page set-US $299.50 before publication, $365 after. For details call 306-966-5722. fax 306-966-8593 or e-mail to linda.dietz.@sask.usask.ca.

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