

The early Earth

Euan Nisbet

Proterozoic Crustal Evolution. Edited by K. C. Condie. Elsevier: 1993. Pp. 537. DFL280, \$175.

TOWARDS the end of the summer, not long before Labor Day, a curious ritual is to be seen in many geology libraries: the spectacle of flustered Phanerozoic stratigraphers, their suntans melanomically glowing after a summer's dinosaur digging, being overwhelmed by toppling piles of Precambrian journals. It is time to write the early lectures for Earth History 301, those quickly passed-over synopses of the first 4 billion years before the really interesting stuff begins in the Burgess shale. In particular, the Proterozoic eon (from roughly 2.5×10^9 to 0.5×10^9 years ago) is the dark age of stratigraphy, a nightmare for many of those lecturing in Earth evolution. For those poor teachers, Kent Condie has edited a welcome time-saver — a volume that provides a good general review of the problems of the Proterozoic. For the brave souls who actually specialize in Proterozoic evolution, the volume provides some excellent summaries of active areas of research, and should provoke both thought and interest.

Proterozoic Crustal Evolution contains 13 chapters, each tackling a major aspect of the problem. Most of the chapters are written by well-known workers in the field. The volume is the outgrowth of the International Geological Correlation Program Project 217 on Proterozoic geochemistry, so its bias is towards crustal geochemistry, but the spread is wide. There are chapters on the ophiolite problem by H. H. Helmstaedt and D. J. Scott, mafic dykes by J. Tarney, the Bushveld complex and other layered bodies by G. von Grunewaldt and R. E. Harmer, granulites by S. L. Harley, iron formations by C. Klein and N. J. Beukes, and isotopes by P. J. Patchett. Each of these topics has its controversy, and the issues are important not simply for the narrow sphere of Proterozoic geochemistry, but more broadly in the longer sweep of Earth evolution. Did early Proterozoic tectonics emplace ophiolites? Perhaps, but the answer depends in part on how far the definition is stretched. How did the gigantic Bushveld intrusion occur? Is it an ambitious boninite or a perverted komatiite, or some other beast? Are the Proterozoic granulites the bones of ancient 'Tibets'? What moved the iron in ironstones? Why were the anorthosites and anorogenic granites emplaced?

These are only a few of the topics discussed. Other chapters include work on collisional and accretionary orogens, volcanics, xenoliths, rifts and accretion. We

still know far too little about the great length of Proterozoic time. In a semi-familiar world, it is not clear whether strict actualism applies — were Proterozoic orogens strictly comparable to modern parallels? — or whether uniformitarianism applies in the modified sense of a planet evolving within its own internal constraints. Was the temperature of the asthenosphere different, and if so, by how much, and how much thicker did this make oceanic crust? How large and how thick were the rafts of continental lithosphere? To what extent did life control the chemistry of the seas?

Recently, another large volume on the Proterozoic has been published (*The Proterozoic Biosphere* edited by J. W. Schopf and C. Klein, Cambridge University Press, 1992; for a review see *Nature* 361,

601; 1993). The two books will make excellent companions, one concentrating on the biological side of the history of the eon, the other mainly on abiotic processes. The Proterozoic evolution of North America has also been well chronicled by Paul Hoffman in a set of essays for the Decade of North American Geology series and elsewhere. As a general introduction to the Proterozoic, this outpouring should keep any worried lecturer supplied with enough material, and may save her from perishing under a toppling pile of journals. This may even be the year to expand the Proterozoic section of the course. □

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The constraints of history

Hugh Freeman

The Most Solitary of Afflictions: Madness and Society in Britain, 1700–1900. By Andrew Scull. Yale University Press: 1993. Pp. 448. \$45, £29.95.

"MADNESS", writes Andrew Scull, "seems to attract more than its share of myths"; how unfortunate, then, that his latest work should most egregiously add to them. An Englishman who is now professor of sociology at San Diego University, Scull has devoted virtually his whole career to studying the management of mental illness — in particular, English

nineteenth-century asylums. He burst on the scene in 1977 with *Decarceration*, a short but vitriolic work in which he interpreted the historical rise and fall of the mental hospital primarily in terms of social control. Its ideological underpinning, reflecting the sociology of the time, was strongly Marxist, an aspect of the discipline which has since quietly faded. Two years later, Scull published *Museums of Madness*, a history of the Victorian asylum, of which the present work is an updated edition.

Since then, the author has remained highly productive, contributing to and editing books on this theme; his scholastic energy is prodigious and every piece of his writing is profusely referenced from an immense range of sources. In, for instance, an earlier biographical chapter on John Conolly, the reforming asylum superintendent of the 1840s, Scull greatly advanced knowledge of an important figure who until then had been poorly researched. Yet, regrettably, this work has often seemed to be less of a genuine search for greater understanding of the historical processes of psychiatry than an attempt to gain support for an ideological position taken up *a priori*.

Like German historians embroiled in their *historischstreit* over the interpretation of Nazism, Scull has pursued a vendetta against those — notably Kathleen Jones and Gerald Grob — who did the fundamental research on British and American asylums, respectively. Their scholastic crimes, in his eyes, are first to regard the motives of lunacy reformers as at least significantly humanitarian, and second to portray things as becoming generally much better for the mentally ill in the 250 years from the early eighteenth century. This is dismissed here as "naive

IMAGE
UNAVAILABLE
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REASONS

Insane treatment — early nineteenth century engraving of Bedlam.

Mary Evans/Society for Psychological Research

Whiggism", yet Scull himself depends repeatedly on Jones as a source, and indeed before her pioneering work of 1972 (*History of the Mental Health Services*, Routledge) — soon to appear in a new edition — the whole subject was largely *terra incognita*. Whether he recognizes it or not, he stands on the scholarly shoulders of those who came before him.

The other main strand in his argument is that from its earliest beginnings, psychiatry has been little more than a gigantic confidence trick, "persuading a wider public to take seriously its claims to possess an expertise resting on a scientific basis". Part of this process, Scull believes, was the establishment of a monopoly of the superintendent posts of asylums in the early 1800s, although his account does not reach the point where doctors were themselves replaced by lay administrators. Even the psychiatry of today is described as "seeking to assure us of the benevolence and rationality of its interventions even as it attempts to lay claim to an ever-widening sphere of social life". The extreme bias of this interpretation, with its unrelieved sarcasm and invective, is reminiscent of *Pravda* in its worst days.

Behind the brilliant verbal pyrotechnics and exhaustive citing of archival sources is a deeply flawed concept. Scull's view is essentially ahistorical. He condemns Grob for pointing out that asylums were little different from most human institutions in their imperfect functioning, on the grounds that these were "the product of historically specific and closely inter-related changes in that society's political, economic and social structure", as well as of intellectual and cultural shifts.

But that is precisely the point. To understand this story, we need to ask: what was the 'success' of any kind of hospital at that time, and were asylums any different? How did the proportions of patients in asylums who became better or, worse, remained unchanged or died, compare with the outcome of those sufferers from mental illness who remained at home, in workhouses or wandering abroad? To what extent were those admitted to asylums as physically ill and neglected as they were mentally disturbed? Were psychiatrists any different from other contemporary doctors in their clinical approach or scientific knowledge, and were they any less effective? Most importantly, how does the evolution of the asylum, with the increasing chronicity of its patients in the nineteenth century, relate to the natural histories of the disorders it treated? These questions are never asked, let alone answered. □

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Prescription or poison?

Beverly E. Griffin

A Dancing Matrix: Voyages Along the Viral Frontier. By Robin Marantz Henig. Knopf: 1993. Pp. 269. \$23.

THIS book is about viruses that may have existed centuries ago and are now gone, some that emerged more recently and are still with us, and some that will probably yet emerge and need to be faced. In the field of virology, there are few miracle cures and fewer still short, sharp shocks that can be administered to counter an agent that cannot be seen (except with the aid of the electron microscope) or even propagated outside a living cell. So it is sometimes difficult to generate optimism about viruses and public health. Yet Robin Marantz Henig ends her book with a section that not only reads splendidly but also illustrates how a community's insight and creativity can be effective in "fighting back", that is, in predicting a viral outbreak before it occurs or, as she calls it, "anticipating the next AIDS".

She discusses, for example, how chickens are being used in Florida as sentinels to anticipate outbreaks of human viral encephalitis that could result from infection with some mosquito-borne viruses. Because the chickens can also be infected by these encephalitic viruses, they can act as forecasters, alerting physicians to the possibility of the disease appearing in patients and public health authorities of the need to institute an anti-mosquito spraying campaign. Similar viral "listening posts", in an age of rapid transportation of goods and people, established on an international basis, could also be used to anticipate problems that might arise in the transportation of viruses from an "immunologically seasoned" population in one part of the world to a virologically naive population in another. To a limited extent, this approach is now being adapted for the influenza virus. The time may yet come when broader global epidemiologic surveillance is given due consideration.

The harnessing of viruses for delivering 'good' genes to replace 'bad' or faulty ones by gene therapy is also cause for optimism. Henig calls it "viral domestication". Why not? Successful virus domestication probably requires, however, a depth of scientific knowledge that is at present available for only few, if any, viruses. For sure, viruses may be harnessed with only limited knowledge for the advantage of the host, but this would seem to require a kind of quirky luck that has not been a hallmark of the field. So Henig concludes her book by arguing for the creation of a

"new biology" to deal with viruses — one where individual and now more strictly defined disciplines, such as molecular genetics, classical microbiology, immunology and population biology, are amalgamated in efforts to outface this wily microorganism, loved by none (except perhaps genuine card-carrying virologists) but crying out for understanding. To create an institute for such a new biology would, however, require an element of imagination few governments seem to possess.

The title of Henig's book is attributed to Lewis Thomas's *The Lives of a Cell: Notes of a Biology Watcher* (1974). The dancing matrix comes from Thomas's idea that viruses "dart, rather like bees, from organism to organism, from plant to insect to mammal to me and back again" and as such, may be nature's efficient mechanism for keeping a gene pool varied and fluid, stirring up the genetic arrangements of species that might otherwise be staid, inflexible and unable to adapt to changing environments. Evolutionarily, viruses may act for the good of the species. This is not our short-term experience, however; even the word for this microorganism is derived from the Latin for 'poison'.

A virus is the ultimate parasite, totally dependent on its host for existence, and therefore having a vested interest in the survival of that host. Alternatively, it must be sufficiently infectious that it can find a new host before the old one dies. Most of the book therefore concerns the fragile balance between this microbe and man. Henig's excellent chapter entitled "A Virus Primer" introduces this subject, and even the (apparently) complex concept of DNA, RNA and the genetic code, in such a lucid manner that it should be intelligible to all but the most recalcitrant reader. Although trained as a journalist, Henig reveals herself as one who has become deeply and intensely involved in her subject, even to the extent of (almost) becoming anthropomorphic about it: "Viruses are so weird and wily that they fairly cry out for metaphors when you write about them". (This eventually happens to all of us in this field!) There are many examples here of the occasions when the fragile balance between the host and the organism has been upset. They range from the stories of mad cows (maybe a virus) to dead dolphins (definitely a virus), from AIDS to Ebola, and make excellent reading, for this is *not* science fiction. The recent outbreak of an Ebola-like virus among research monkeys in an animal colony in Virginia with its frightening overtones is also discussed here; this outbreak was contained, but only just.

If the book has a central overriding theme, it is about new viruses that may be on the horizon, about 'why' and 'how' they could emerge, and the effect they might have on our own society if we are not prepared to deal with them. Viruses