BOOK REVIEWS

Joel S. Levine (ed), *The Photochemistry of Atmospheres: Earth, the Other Planets, and Comets*, Academic Press, Inc., Orlando, Florida, 1985, $ 79.50.

Atmospheric chemistry is a young discipline. It began with the description by Chapman in 1930 of the chemical reactions that control the concentration of ozone in the stratosphere. Further significant progress in the field did not occur until the late 1940s when Bates and Massey identified the most important chemical reactions in the ionosphere and Bates and Nicolet explored the dominant photochemical processes of the megnetosphere. In 1952 Haagen-Smit reported the dominant role of photochemical processes in the formation of the Los Angeles type of smog, but the recognition of pervasive photochemical activity throughout the troposphere dit not occur until Levy described a tropospheric source for the highly reactive OH radical in 1971. Additional significant advances in the study of atmospheric chemistry have been stimulated by spacecraft missions to the planets and rocket and satellite observations of the upper atmosphere of the earth. All of this history and much more is ably reviewed in this book.

The book contains nine outstanding chapters by exceptionally well qualified authors, who deal with the subject in considerable depth by regions, beginning with the troposphere, stratosphere, and upper atmosphere, continuing with Venus, Mars, and the outer planets, and concluding with comets. There is also an important chapter on the climatic implications of atmospheric chemistry and, of particular interest to students of the origin of life, a chapter on the photochemistry of the early atmosphere, including possible mechanisms of chemical evolution.

This is a good, advanced text, tightly edited, and packed with information. Quantitative information on rate coefficients, cross-sections, and compositions is presented in tables and in extensive appendixes. There is even a carefully prepared and helpful index. Although a number of books on atmospheric chemistry have appeared in the last few years there are no others, in my opinion, that succeed as well as this one in presenting the frontiers of this rapidly developing discipline across the full range of environments that are currently under investigation.

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Steven Rose and Lisa Appignanesi (Eds.), *Science and Beyond*, Basil Blackwell, Inc., New York 1986, 204 pp., $24.95.

The intellectual legacy of J. D. Bernal has continued to manifest itself since his death fifteen years ago. His contributions to science extended across many academic boundaries: he was among the first of an influential generation of physicists who turned to the structural study of biological molecules, his early discussions on the origins of life stressed the need for an interdisciplinary approach, and his postulate regarding the possible role of clay minerals in chemical evolution has in recent years been articulated and expanded by a host of researchers in this field. However, Bernal was also concerned with science as a cultural entity, as reflected in the title of his seminal book of 1939, *The Social Function of Science*, in which he analyzed the complex relationships among science, government and industry in the modern technological society. Bernal's critique borrowed heavily from the fields of history, sociology and economics to develop a utopian vision in which research would be most efficiently applied toward the solution of pressing social problems. *Science and Beyond* offers a pluralistic view of the implications of more recent scientific developments for our understanding of science and its impact on society.

This ensemble of essays is based on a symposium held in London to mark the twentieth anniversary of the Science Policy Foundation, a British organization that dates its conception to 1964 when a commemorative volume entitled *The Science of Science* was published in honor of Bernal. Appropriately, most of the fourteen papers deal with biological issues, although the major reason given by editor Steven Rose for this bias is that the tremendous growth of the life sciences has made biology the central proving ground of philosophical and ideological debate over the nature of scientific inquiry. This book is therefore about scientific controversies: for example, Richard Dawkins defends the emergence of sociobiology as a research specialty, while Patrick Bateson endeavors to correct the misconceptions that such popular works as Dawkins' *Selfish Gene* have engendered. Other essays encompass such diverse topics as the prospects for Artificial Intelligence and the effects of the increasing role of women in science. While the treatment of specific concerns such as scientific funding and health policy emphasizes the situation in Britain, the style and content of this book nevertheless is directed toward a wide audience.

One refreshing feature of this collection is that each writer had access in advance to preliminary drafts of the other contributions to this symposium, with the result that the essays often refer to one another and thus convey the feeling of a live conference. The opening of the book immediately sets this tone by presenting the opposing views of James Watson and Steven Rose on the subject of the limits to science. In his contribution, Watson eschews philosophical speculation and interprets the notion of limits not in terms of the structure of knowledge but rather as artificial constraints that society imposes on scientists; he concludes that science itself is 'limitless' but that boundaries may often be erected for ethical or safety reasons. Steven Rose (whose Marxist interpretation is very much within the Bernal tradition) takes issue with Watson by emphasizing that science is intrinsically limited by the historical and social context in which scientists operate. In a sweeping critique, he concludes 'So long as science – in the questions it asks, and the answers it accepts – is couched in reductionist and deterministic terms, understanding of complex phenomena is frustrated.' Such dissatisfaction with mechanomorphic models is not new, as illustrated for example by the writings of Erwin Chargaff on the origins of life. Unfortunately while it has become fashionable, particularly in the literature of popular science, to advocate a more 'holistic' approach (the term used both by Steven Rose and by Hillary Rose in a later essay) toward the study of nature, the utility of these recommandations is diminished by the lack of a well-articulated paradigm (that is, one that could 'come up with interesting predictions,' according to Watson).

There is much within *Science and Beyond* to provoke and even outrage its audience. Of particular interest to readers of this journal is the thoughtful analysis by John Maynard Smith of the failure of traditional Darwinian theory to account for the emergence of novel structures, which Brian Goodwin in a subsequent essay places in a broader context by stressing the limited heuristic value of a historical approach to biological form. A common theme recalls Bernal's own discussions on the extent to which our interpretation of nature is influenced by the disciplinary organization of science. While nearly all the ideas described in this small volume have been expressed elsewhere, the juxtaposition of divergent viewpoints makes for lively reading. Despite occasional typographical errors, *Science and Beyond* provides an entertaining and informative cross section of ideological issues in contemporary biology.

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Lynn Margulis and Dorion Sagan, *Origins of Sex*, Yale University Press, 1986, $ 35.00.

Why sex? Most introductory courses in biology as well as those treating advanced topics in evolution pose this question, no less for its real heuristic value than to titillate students who have just slogged through chromosomes and chromatids, then centrioles, kinetosomes, and kinetochores. One routinely recites the horrors of sex, metabolic, venereal, and behavioral with the inevitable punch line, "and for all your troubles your offspring carry only half your genes." By now, if one has any theatrical talents, the students are awake. The tension mounts as one cites the seeming success of various asexual protists, plants, and even parthenogenic animals, then the question, "Why sex?" A few dutiful students will have read the assignment and noted that haploid gametes might have 2^n different chromosome combinations given a parent of haploid number, n, and further that eggs and sperm might combine to form $(2^n)^2$ different zygotes, for $n = 23$, $(2^n)^2 = 8388608^2 = 7 \times 10^{13}$. One proudly concludes by emphasizing that crossing-over during prophase I of meiosis provides additional decades of diversity and then compares the result with the number of atoms in the universe. Most texts conclude, as does for instance Keeton, "On balance, given the continual fluctuations that characterize most environments both over the short term and over the long term, sexual reproduction, with its potential for genetic change and hence for evolutionary adaption, has evidently been the more advantageous." Although this explanation is surely not incorrect many of us respond to this first encounter with sex wondering "Is that all there is to it?"

Margulis and Sagan present, for a general audience, a theory of the evolution of sex. Although several aspects of their thesis are quite speculative, it is not unreasonably so. It hardly "represents a radically different 'thought style' from that of the scientific literature from which it has emerged." They "anticipate ... heavy attack of our book by the old guard" by which they mean "members of the thought collectives of sociobiology, population genetics, or population ecology." As noted, most contemporary texts of zoology, botany, or cell and molecular biology answer the rhetorical question "Why sex?" with the almost identical words of "increasing genetic variation" in the offspring. I am confident that most of these authors would welcome a more refined explanation since they recognize that generating genetic variation, per se, is easily accomplished without meiotic sex.

In their Introduction Margulis and Sagan claim that their *Origins of Sex* unravels the "history of the origin of sex." They note that "sex in bacteria crosses species boundaries" and subsequently define sex as the "formation of a genetically new individual." Many organisms "exchanged genes sexually without the sex ever leading to the cell or organism copying known as reproduction." Reproduction is an increase in the number of individuals. This distinction between sex and reproduction is a fundamental theme. Unfortunately the definition of sex is so broad as to reduce its value. Has a bacterium infected with a lysogenic phage or a stable episome engaged in sex? Subsequently (p. 30) they state that there are only two major kinds of sex, "prokaryotic recombination and eukaryotic meiotic."

They emphasize the distinction between the "maintenance of sex" and the "origins of sex." Further they argue without strong evidence that sex "has developed several times." Much of this difficulty arises because we do not know how closely contemporary protists resemble the early precursors of protoctists, fungi, plants, and animals. Hence the assertion that "meiosis... first occurred in protists" really begs understanding the nature of this putative precursor(s).

What is the theory of Margulis and Sagan? How well does it stand up?

(1) They note that the pre-phanerozoic earth had little oxygen and hence ozone in the atmospehre and that for about three billion years, 3.6 to 0.6×10^9 before the present, our prokaryotic ancestors were subject to intense ultraviolet radiation that must have produced various changes, such as thymine dimers, in DNA. Enzymatic identification of and repair of these changes would have been essential to viability. They make the reasonable, and hardly controversial assertion, that "ultraviolet repair preadapted bacteria to sexuality."

Sex, as broadly defined, is usually accompanied by some recombination or exchange in DNA. This may or may not result in genetic novelty. The authors quite rightly emphasize that such sex is not required to generate mutations, and subsequent differential survival. Further, sex usually produces rearrangements of genes as opposed to changes of base sequence within a single gene. However, it is fallacious to argue that: "Because variety produced by sex is often nullified by further sex, such rearrangements are not permanent." Nonsense; differential survival, or natural selection, will operate on these changes just as it does on point mutations, which are also subject to reverse mutation.

(2) "A central thesis of this book is that the eukaryotic cell is homologous to a community of microorganisms." Endosymbiosis is an important event in the evolution of the eukaryotes. It quite certainly accounts for the origin of mitochondria and plasts. These organelles have many similarities to their eu- or archaebacterial precursors, most important of which are similarities in DNA sequence. Further, these organelles are enclosed in a double membrane, the inner of which share numerous properties with those of prokaryotes, while the outer of which has evolved from the host membrane, cytosolic surface facing the cytosol and glycosylated surface opposed to the inner membrane of the organelle.

(3) Undulipodia (eukaryotic flagella and cilia) evolved by endosymbiosis of some sort of spirochete. This long held idea of Margulis is important to the argument and unfortunately not strongly supported. Although the eukaryotic cell membrane does enclose the standard 9 + 2 arrangement of microtubles in the cilium, neither the entire organelle nor the kinetosome (basal body) is enclosed in a descendent of a prokaryotic membrane. So far as is known, the kinetosome contains no DNA, either of episomal or of bacterial origin. Associated RNA, if it is present, would appear to play a structural rather than a genetic role. The assertion (p. 142) "that microtubule organizing centers of eukaryotes are the remnants of the spirochete genome" is unsubstantiated. Neither encoding DNA nor protein homologous to tubulin have yet been found in prokaryotes and definitely not in bacterial flagella. The authors acknowledge (p. 141) "The least sturdy hypothesis of the serial endosymbiotic theory of the origin of eukaryotic cells is that of the origin of undulipodia from free-living bacteria, most likely from spirochetes." (p. 217) "The fact that cortical information is replicated and inherited directly implicates either RNA or DNA; the absence of DNA associated with MTOCs [microtubule organizing centers] implicates replicating, cortical RNA."

(4) Kinetochores and centrioles are important for the separation of the chromosomes, which are immense, relative to the genetic apparatus of prokaryotes. The centrioles are very similar, if not identical, to kinetosomes. Although tubulin-containing spindles are surely essential to mitosis and meiosis, Margulis and Sagan concede that many plants accomplish both without centrioles. A kinetochore forms part of the centromere of most chromosomes. Although it too may function as a microtubule organizing center in vivo, as it can in vitro, its possible homology to the centriole and kinetosome is not well established. The authors maintain (p. 183) that:

"The protoctist ancestors of the animals and plants never solved the problem, on the single-cell level, of how to retain both their motility and their ability to divide by mitosis." This correlation, which is certainly not absolute witness the sperm development in the butterfly *Pygaerd*, is presented as evidence of homology and functional interaction.

(5) On an evolutionary time scale cells are prone to endosymbiosis of both conspecific and other cells. If the digestion is not complete, the greedy host is left with extra, or for sibling cells double, doses of DNA. "The key factor in the origin of meiotic sexuality in haploid protists was the relief of diploidy." Although one can hardly disprove such an argument, one should note that both eu- and prokaryotes have evolved elaborate and relatively successful mechanisms for recognizing and destroying foreign nucleic acids, excepting those of a few troublesome viruses. Further, various plants thrive with hyperploidies. Would the postulated cellular incest really have been so deleterious as to drive one of the major events of eukaryotic evolution?

(6) The authors suggest that differentiation involves rearrangement of genes, citing the switching in immunoglobulins as an example. They (p. 192) "hypothesize that the check-up system of meiosis must from time to time clean out all these excesses." They note a correlation (p. 178) "It is certain that those eukaryotes that demonstrate the phenomenon of differentiation also undergo meiosis." The kernel of their argument (p. 185) then is: "The protoctists that had combined mitosis and undulipodia through the ruse of multicellularity also evolved meiosis. From the beginning meiosis was associated with sex. For over 600 million years organisms in the animal and plant lineages have been paying a dear price for the privilege of differentiation. This price is meiosis. Sex, in the form of biparental mating and associated with meiosis since it arose, has been taken along for the ride." Given our still primitive understanding of differentiation one can hardly disprove any such hypothesis; however, such correlations hardly prove a causal relationship.

Finally on p. 205 they pose the question: "Since sex, in the form of mixis, is not selected for directly the question, 'Why, if asexual beings can have far more offspring than sexual ones, are there so many more sexual animals?' is not a valid scientific problem." The world would welcome the definition of such validity.

They argue that "Meiotic sexuality was never selected for because it generated more variation than asexuality did. Meiosis evolved as a cyclical relief of diploidy," and continue within this sentence to contradict the assertion of the previous sentence, "and was maintained in many species first because of seasonal or other alternating environmental conditions and later due to its obligate association with the development of differentiation."

The arguments addressing each of the six topics – UV repair, endosymbiosis, undulipodia, kinetochores, ploidy, development – grow progressively more speculative. Although each has interest and may have contributed to the origins of sex, Margulis and Sagan have not made their case that meiotic sex, per se, offers no selective advantage.

Although this 258 page book is hardly a coffee table production with lavish color figures, it is reasonably well illustrated and edited. Some digressions and simplifications have been made for the sake of a general scientific audience. A few errors or misleading statements are noted: On p. 12 they assert that during cell division always "new copies, replicas of a cell's DNA, must be made every time that cell grows to double its size just before dividing to form two cells." As they explain later, this is exactly what does not occur during the second meiotic division, a bit confusing for the general reader. The assertion (p. 18) that "about 200000 nucleotide pairs are required to make each protein" is on average true for eukaryotes but it does not explain or distinguish between triplet coding, introns, and intervening sequences. In Figure 2 (p. 20) guanine is mistakenly labelled a pyrimidine and cytosine a purine. On p. 22 one would prefer x-rays to be diffracted by crystals, not "deflected." The nucleus is bounded by a double not a "single-layered membrane" as stated on p. 107. It is incorrect to describe (p. 117) the 3-prime end of the DNA as the "hydrogen-oxygen" end and the 5-prime as the "hydrogen" end.

There are certainly flaws in the formulation and presentation of this theory by Margulis and Sagan. Even so, their enthusiasm merits reading, their postulates experimental confirmation or refutation.

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Norman H. Horowitz, *To Utopia and Back: the Search for Life in the Solar System*, W. H. Freeman and Co., New York, 1986, $ 17.95 hardbound; $ 11.95 paperback.

This book is well written for the nonscientist. At the same time it is of interest to the scientist who may not be familiar with the history of thinking about the origin and distribution of life in the solar system. The book has eight chapters, a glossary, and a bibliography. It begins with a discussion of the definition of life, which Horowitz sees as synonymous with the possession of genetic properties – a generally accepted concept stating that 'life' implies the ability to mutate freely and to reproduce the mutation. Whether or not the first information-containing molecules were as complex as or identical to contemporary RNA-DNA is not discussed. Did life begin with the RNA-DNA system or something simpler?

The next two chapters deal with discussions of spontaneous generation, panspermia, prebiotic systems, and chemical evolution. These are well written and informative chapters. Chapter 4 is a discussion of what makes planets habitable and a consideration of other planets and moons in our solar system of potential biological interest.

The remainder of the book is devoted to Mars and its history as an abode for life in the eyes of many. The author provides a fairly detailed description of Mars as seen by Percival Lowell, and the profound influence of Lowell's conclusions concerning

Mars as an abode for life on subsequent Mars scholars and laymen. He then draws interesting comparisons between those early telescopic observations and the steadily improving data from the space age Mariner flights, leading up to the Viking landers. This all makes for good reading. Horowitz draws a good lesson for us in this chapter: each time we get more detailed and accurate data about Mars, we find that we have drawn the wrong conclusions about the planet from earlier data. He goes into considerable detail about the changes in our knowledge about Mars, based on Mariner data, setting the scene for Viking.

Chapters 6 and 7 deal with the Viking mission, which sent two landers and two orbiters to Mars in the 1975–1976 time frame. These extraordinary spacecraft all functioned almost perfectly and provided us with a wealth of new data, again changing many of our previously held conceptions about Mars. It is these two chapters that I have the most difficulty with in that I believe Horowitz has somewhat overstated his case. He states on page 118 that microbial life in the dry valleys of the antarctic has failed to cope with the pervasive dryness of that region. In fact, microbial life forms (endolithotrophic lichens) have coped very well in these valleys and are flourishing inside certain rocks – a fact noted by Horowitz elsewhere in the book. His principal point, that liquid water available at least transiently or high concentrations of water vapor are necessary for biological activity, is of course valid. He argues that such conditions do not exist and are impossible on Mars. I would think that based on his own observation that most conclusions about Mars drawn from previous data have proven erroneous, one could argue that entirely dismissing the possibility of microbial Martian life, even on the basis of Viking data, might also be premature. I would not close the doors on the possibility of the endolithotropic habitat seen in the antarctic and other deserts of the Earth, may also exist on Mars.

The author also concludes that Mars is uncontaminatable and self-sterilizing. He feels that terrestrial organisms would not survive, that they would be killed by the presumed oxidizing nature of the Mars surface material. I know of no data to support this conclusion, and in any case, the need for sterilization of spacecraft searching for life is not only to preserve the local environment from alien life forms, but also to preserve the integrity of the life detection experiments on board – a considerable task.

Chapter 7 I find to be a reasonable review of the results of the three-part Viking Biology experiment. It is a fair description of how Viking did its work, and quite readable for the layman. However, I am unsure that other biology investigators would agree with Horowitz's conclusions about what the results from the individual experiments mean.

In the final chapter, we find the statement that 'Viking found no life on Mars, and, just as important, it found why there can be no life'. I tend to agree with the first part of this statement, but not the second. I feel the possibility for a highly specialized remnant of a former biota (perhaps in the form of a Martian endolithotroph) is still open, however remote. Our only data to date is based on two tiny landing sites in locations selected for safety, not science. That is inadequate. In addition, Horowitz shows little interest in returning to Mars, looking at other sites (in the bottom of canyons, inside of rocks, in polar regions) about which we know very little, or in looking for evidence of fossil biota, left from a time when Mars was a planet with water and a more hospitable environment. Horotwitz may well prove to be right when he says. "... it is now virtually certain that the Earth is the only life-bearing planet in our region of the galaxy". However, adhering dogmatically to this conclusion, without communicating the scientific community's enthusiasm and interest in continued Martian exploration, is something of a disservice to those chemists, geologists, and biologists who still feel that life, or at least evidence of past life, may exist on Mars.

In conclusion, the book is well done, generally accurate, and fine reading for the layman and novice scientist. Some conclusions may be overstated in the minds of some readers, but this highly respected scientist has convinced himself along with some others of the total absence of life on Mars, and gives his reasons why.

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