biology is necessary to understand the presentations. The book is therefore useful mainly to final-year undergraduates, postgraduates, molecular biologists and virologists. Each chapter contains references which will allow the reader to carry out background reading, but the main emphasis of the text is on research work carried out by the different groups involved in the meeting. Is it good value? Probably it is a good and reasonably up-to-date reference for any library attached to an appropriate research laboratory.

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Genetic Takeover and the Mineral Origins of Life. By A. G. CAIRNS-SMITH (first published 1982, first paperback edition 1987). Cambridge University Press. ISBN 0 521 34682 7.

Since the early writings of A. I. Oparin and of J. B. S. Haldane in the 1920s, virtually all scientists have accepted that the molecules of life arose by spontaneous synthesis on Earth, in strongly reducing conditions. There has been a prevailing view that small organic molecules forming from primordial gases became concentrated in various ways, that polymerization eventually led to long-chain molecules, that reactions between different kinds of such molecules resulted in self-replication, and finally that simple systems emerged, able to reproduce and evolve by natural selection. This conception of chemical evolution has been supported by the many experimental syntheses, done since Miller's classic work of 1953, of organic molecules in conditions simulating those of the early Earth. Impressively, many of the nitrogen-containing compounds vital to the biochemistry of living organisms can be made abiotically. But this is only the first step. For despite experimental demonstrations of polymerization, the rest of the story is far less clear. And to Andrew Cairns-Smith parts of it, at least, are despairingly implausible.

The author begins his text by setting out, very fairly in my view, the standard view on chemical evolution. Then he proceeds to explain, with singular clarity, why he does not believe it. There are too many difficulties. Consider, for example, prevital nucleic acid. Even allowing for the abiotic formation of purines and pyrimidines, nucleotides cannot be assembled from these components by any kind of easy synthesis. Cairns-Smith gives no less than 19 reasons why not. The next problem is that of multiple interdependence. We seem to be dealing with endless interrelated chicken-and-egg questions. How, in fact, are all the various bits of biochemical mechanism set up when the significance of each component depends upon the existence of all the others? We should consider, too, the origins of chirality. Why are only L-amino acids found in living bodies? These are only a few of the difficulties of the conventional view. Going further, there lies the ultimate spectre – the origin of the machinery of heredity.

The problems being thus defined, Cairns-Smith develops in great detail the argument he has already championed in his earlier books – that of the clay–crystalline origins of life. 'For a picture of first life do not think about cells, think instead about a kind of mud, an assemblage of clays actively crystallising from solution. . .' Yet this goes far beyond the ideas of Bernal and others, of clays involved in early evolution simply because they concentrated and catalysed organic molecules. To Cairns-Smith clay minerals were the actual materials, and perhaps the only ones, out of which the earliest organisms were constructed. Genes, controlling the chemistry of the immediate environment, came early, but they were mineral, and not of nucleic acid. They existed in immense numbers. These colloidal inorganic minerals were able accurately to replicate themselves as they grew; any defects would likewise be replicated. This clay-based life could fix carbon and nitrogen, could harness sunlight, and could consistently synthesize particular molecules. It came to use the readily available free organic molecules in the environment as phenotypic components. They were to begin with no more than 'optional extras'. But they built up by steps into organic genes. The organic genes were secondary, but since they were so much more effective than the 'clay genes' they became dominant. This is the 'genetic takeover' referred to in the title. And as a result life, with its now effective hereditary mechanism based upon nucleic acids, escaped from its clay base.

Each chapter argues different elements of the case at length. Primary genes, takeover, first biochemicals, first life, and the entry of carbon into the evolving system are considered in turn. In the process the author covers a very broad field, but with singular erudition. Cairns-Smith writes extremely well. His prose is lucid and superbly constructed, and he has been able to make even the difficult bits more or less comprehensible. The text is illustrated by many attractive diagrams of chemical and crystal structure, and there are a fair number of fine electron micrographs. At one point the author introduces an imaginary dialogue between Dr Advo, a champion of genetic takeover, and the sceptical Dr Kritic. It is really very well done. The concepts developed here may be heterodox, but they are based upon a vast knowledge of crystal structure and biochemistry, so clearly illustrated throughout the whole book. Cairns-Smith has defined the impasse which current studies in the origin of life have reached, and he has set forward his own case clearly, and in the final chapter points to ways in which future research might go. Now there may well be weaknesses in the argument, but whether any such are fundamental was not immediately evident to this reviewer. I found it intensely thought-

provoking – more so than any other work upon life's origins that I have read in recent years.

This book is replete with information of interest to anyone studying the origins of life, whether biochemist, crystallographer, biologist or earth scientist. It is also a key text in the continuing debate on life's origins. Whether one is sceptical or accepting, this is a book which cannot be ignored.

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Phage Mu. Edited by N. SYMONDS, A. TOUSSAINT, P. VAN DE PUTTE and M. M. HOWE. New York: Cold Spring Harbor Laboratory 1987. 368 pages. Cloth, $75.00. ISBN 0 87969 301 1.

When this book was advertised I said to myself 'At last – and high time too!' and I think this will be the reaction of many geneticists and microbiologists who have been intrigued by the mysteries of this extraordinary bacteriophage and have had difficulties with the paucity of literature on it. The new book looks rather slim when compared with the second large volume on bacteriophage Lambda, *Lambda II*, already five years old, a contrast which reflects the very different levels of popularity of Lambda and Mu among phage geneticists (though it has to be borne in mind that Lambda was discovered 12 years before Mu). The Mu genome is also a little shorter than that of Lambda: 37 kbp compared with 48 kbp; and one might add another rather irritating difference, that lysates of Mu are very unstable, which means that the phage has to be kept in the lysogenic state.

Phage Mu (for mutagen) was first described by Austin Taylor in 1963; and was immediately remarkable for its ability to insert anywhere in the bacterial chromosome (and stay there), and so produce a wealth of stable mutations in the *E. coli* genome. But interest in Mu was slow in spreading: 3 papers over the next seven years, 4 in 1971, 8 in 1972 and 12–15 per year in 1973–5. The (presumably complete) list of references at the end of the book contains about 450 papers specifically on Mu and 300 on related topics, a surprisingly modest total. The reason for this slow growth seems to have been that the few people who started working on Mu in the late 1960's kept strangers away by exchanging information in 'Mu workshops', secret or unpublicized meetings with no published proceedings, and there was no pressure to publish the experimental results since all those in the Charmed Circle were in close touch. It was not until 1981 that, at a meeting on temperate phages, the speakers at the session on Mu were actually congratulated by the Lambda experts on making their talks comprehensible!

Mu had one disadvantage: unlike lambda it is not inducible by UV or other agents; but this problem was soon solved by making thermo-inducible mutants which can lysogenize at 30 °C, but become lytic at 42 °C. But it has many unexpected characteristics. Whether grown lytically by infection or induced by heat, Mu reproduces by a system of replication which requires multiple transpositions of its DNA into many sites in the bacterial chromosome, followed by 'headfull' packaging of the DNA into preformed phage heads. There is room in these heads for a little extra DNA, so the virion DNA has 50–150 base pairs (bp) of host DNA at the *c*-terminal end and 1500–3000 bp of host DNA at the other end, which is believed to be the end packed last into the head. These host DNA fragments are picked up at random from the host chromosome and differ between individual phage progeny even when these come from a single burst. Transposition is essential for phage replication, and, together with the extra bits of chromosomal DNA taken up, makes up a quite novel form of DNA reproduction, giving Mu many of the properties of a transposon.

A further novelty is the method of changing host range, which depends on inversion of 3 kbp of phage DNA, the G segment, through the action of a specific DNA invertase coded by the *gin* gene. The G segment controls the nature of the phage tail fibres; and one orientation, G(+), produces fibres which bind to *E. coli* K12 and B, while the other orientation, G(−), binds to *E. coli* C, *Citrobacter freundii* and *Shigella sonnei*. It should be borne in mind that one can get strains labelled *Citrobacter freundii* from many different sources which look rather different (I have done so), and I would be surprised if they all showed sensitivity to G(−) phage.

The book starts with some variable-quality photographs of members of the Mu tribe, for those who want to recognize them when they meet one. This is a requirement for Cold Spring Harbor books, but I wish they could afford a better photographer – or a better camera. A dedication to Ahmad Bukhari, who was a major inspiration to Mu workers, is followed by 16 well-organized chapters entitled: A history of Mu; Phage Mu – an overview; Regulation of transcription; The SE region; Late genes, particle morphogenesis and DNA packaging; The invertible G segment; Regulation and expression of the *mom* gene; Integration of the infecting Mu DNA; Transposition of Mu DNA *in vivo*; Transposition-replication of Mu *in vitro*; Replication of Mu DNA *in vivo*; Transposable elements – an overview; Transposable Mu-like phages; Mu as a genetic tool; Some lessons of Mu; and The evolution of Mu. After this we have appendixes on Genetic and physical maps; Mu DNA sequences, from the left and the right end; Useful Mu and Mini-Mu derivatives. Finally the bibliography and subject-index.

To add a few remarks on some of these chapters, let us start with 'The SE Region'. This consists of 5 kb between genes B and C in the early transcription region. Originally labelled non-essential because no