Evolution of prokaryotes. K. H. Schleifer and E. Stackebrandt (eds). Academic Press, London. 1985. Pp. x+367. Price £49.50; $49.50 US.

This contains 15 articles presented at a symposium in Munich in 1984 in conjunction with the Deutsche Forschungsgemeinschaft and the Federation of European Microbiological Societies. They fall broadly into two categories, (1) the phylogeny of prokaryotes and (2) the evolution of metabolic pathways and genetic mechanisms. They constitute an up-to-date picture of our knowledge in these fields, together with much speculative interpretation.

The main interests for geneticists lie in the second topic. There are reviews on the evolution of genome structure, transcription apparatus, translation apparatus and transposable elements. These are soundly factual, and the authors (particularly W. Zillig and his colleagues) have given thoughtful discussions of alternative explanations for their observations. Metabolic systems that are covered include chemolithoautotrophy, photosynthesis, respiration and energy production. These also contain good compendia of our present knowledge.

The phylogenetic articles are of particular interest to microbiologists. They reflect the important breakthrough some years ago by C. R. Woese and his colleagues, which showed that convincing relationships between the most diverse organisms could be established from ribosomal RNA. This work led to the proposal that certain bacteria (methanogens, extreme halophils, and thermoacidophils) formed a "third kingdom", the "archaebacteria", comparable to the group containing most other bacteria plus the blue-green algae ("eubacteria"), and to the eukaryotes. These articles again consist mainly of reviews, but R. De Wachter and his colleagues present a good deal of new information from 5S ribosomal RNA.

Some of these articles are uncritical and polemical, with numerous unsupported statements and a tendency to ignore evidence that conflicts with the propounded hypotheses. But one may hope they will prove stimulating and will direct new work to the most critical points. The geneticist will find that many terms are misused, *e.g.*, genetic drift (pp. 77, 358), macroevolution (p. 21), and convergent evolution (p. 345) are all used in a sense different from that in the rest of biology. Throughout the volume there is a disposition to use "phylogenetic" for "genomic" or "genotypic" and to equate "phenotypic" with "phenetic": both genotypic and phenotypic relations are of course phenetic. There is little consideration of gene transfer.

The volume is primarily on the "state of the art". As such it is a useful reference work, and may also have a place among books that relate to the interface between biotechnology and genetics.

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Genome multiplication in growth and development. Biology of polytene and polyploid cells. V. Ya. Brodsky and I. V. Uryvaeva. Cambridge University Press. Developmental and Cell Biology Series No. 15. Cambridge. 1985. Pp. viii+305. Price £39.50.

The subject of this book is revealed by its subtitle: "Biology of Polyploid and Polytene Cells". It is the successor of the authors' "Cell Polyploidy" published, in Russian, in 1981. One reason for welcoming "Genome Multiplication" is that it summarises a large Soviet literature on this subject not usually accessible, or, at least, not usually accessed, in the West. Brodsky and Uryvaeva state that their book complements, rather than overlaps, Nagl's "Endopolyploidy and Polyteny in Differentiation" (Amsterdam, 1978), and I tend to agree with them, although their Chapter 4 (on plant cells) is clearly based on Nagl, reflecting the different biases of Brodsky and Uryvaeva (animals) and Nagl (plants).

Brodsky and Uryvaeva divide their monograph into two distinct sections. In Part I they review the occurrence of polyploid nuclei in the eucaryote kingdoms. Their aim is twofold: the first is simply to survey, the second to convince the reader that polyploidy is a "phenomenon of normal development" and, therefore, one worthy of their interest and ours. In both tasks they succeed, though not as well as they might have. Their survey is incomplete—the most surprising omission being the polytene nuclei of the Collembola. In their second aim, success is only achieved by both special pleading and, in my view, a Hegelian contrast between the role, in development, of polyploid and non-polyploid nuclei.

My main point of disagreement with the authors is, however, in their definition of polyteny. Nagl (*loc. cit.*) gives quite a good definition of polyteny: "polyteny is a structural type of endopolyploid interphase nucleus, in which the endochromosomes remain together to form giant [*i.e.*, polytene] chromosomes." This is a workable definition which corresponds to general usage. Brodsky and Uryvaeva's definition is quite different: "Polyteny is the repeated doubling of the number of chromonemata (chromatids) in the diploid, or conjugated diploid, set of chromosomes without subsequent mitosis." This definition leads these authors to interpret, for example, the giant nuclei of the silk-glands of Lepidoptera and Trichoptera as being polytene, rather than simply endopolyploid. It also leads them to make such statements as "polyteny is a *characteristic* of invertebrate cell growth and development" (my emphasis). This extended definition of polyteny obfuscates, rather than clarifies. It overlooks certain characteristic differences between endopolyploid and polytene nuclei, for example their shape, and the possibility that the precise conjugation of chromonemata, seen in a classical polytene nucleus, and most definitely not seen in a classical endopolyploid nucleus, might have important functional consequences.

Why do Brodsky and Uryvaeva adopt this broad definition of polyteny? I think the reason is that they need a stick with which to beat Geitler's concept of