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

endopolyploid mitoses. Brodsky and Uryvaeva make a convincing case that the interpretation of endopolyploid nuclei originating via endomitotic cycles has been overused. One of their useful achievements is to stress the variety of mechanisms that can give rise to polyploid nuclei. What I object to is the conclusion that the suppression of mitosis necessarily gives rise to polytene nuclei *by definition.*

In Part II Brodsky and Uryvaeva are concerned with "modes and reasons for genome multiplication". In general I was rather disappointed with this, the conclusion of a long discussion on the biological significance of polyteny (by their definition) being rather weak: it is "advantageous". True, but not very helpful. They try and argue that there are some fundamental differences between polytene and diploid nuclei in their "protein spectra, [their] RNA fractions and [in] the activity of isozymes". I doubt it. Moreover they claim that the "redundancy" of their genetic information gives polyploid cells an advantage over diploid cells. This overlooks the fact that what (little) polyploid cells may win on this roundabout they lose on the swings, polyploid cells cannot usually be replaced by regenerative mitoses. In fact I consider any attempt at a general hypothesis to "account" for such a diverse phenomenon as the occurrence of polyploid nuclei as being naive. There can be no question but that polyploidy, endopolyploidy and polyteny (in Nagl's sense) have all originated many times, and independently, during the last 500 million years (or so).

One rather disturbing feature of this book is that the authors are so obviously unsure of themselves, and unauthoratitive, when discussing material of which they lack first-hand experience. This is especially true when they review work of a more molecular nature. My hackles rise when, for example, I read such statements as "After fertilisation, this RNA [and here, "this RNA" is the ribosomal RNA of the oocytes], which has accumulated in an inactive form, *determines* the early events of embryogenesis" (my emphasis). I puzzled some time over the statement that "the number of genes should correspond to the degree of perfection of form and function' and can only conclude that this is one of the instances where we suffer from reading a translation. Brodsky and Uryvaeva clearly fail to grasp the generality and significance of somatic chromosome pairing in the Diptera and have not grasped the issues involved in recent controversies concerning the underreplication of chromosome regions in polytene chromosomes. Similar instances of a failure to comprehend the literature in areas peripheral to their own research could be cited. Unfortunately these undermine one's confidence in their discussion of fields of which oneself has no first-hand knowledge.

My criticisms should not overshadow the fact that this *is* a useful book for those interested in nuclear structure and function. I can only repeat that the two main reasons for reading it are (a) that it will open your eyes to a literature which you may have overlooked and (b) that the discussion of the origin and significance of polyploidy, even if somewhat misdirected, at least will make you think.

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Experiments in plant tissue culture (2nd edition). John H. Dodds and Lorin W. Roberts. Cambridge University Press, Cambridge. 1985. Pp. xvii+232. Price £25 (HB), £8.95 (PB).

This text book offers a comprehensive practical introduction to research in plant tissue culture, at a reasonable price. All aspects of the work are covered, from planning a tissue culture laboratory through to protoplast production, somatic hybridisation, and the production of economically important compounds from cell cultures.

The chapter on aseptic technique should be read by all students before attempting tissue culture experiments, and there is a detailed consideration of basic tissue culture media. Many practical hints are included, such as the choice of the correct solvents for dissolving different plant growth regulators, and how to prepare stock solutions.

The main techniques commonly used in plant tissue culture research are covered by individual chapters, which are easy to follow. A brief introduction and explanation of the procedures and their potential uses precedes a detailed experimental procedure, ideal for teaching and/or training purposes. A number of relevant questions for further consideration by the student are also included, and each chapter concludes with a considerable number of useful references for further reading.

There are however some notable omissions, such as the consideration of protoplast electro-fusion techniques which are now becoming more widely used, and the brief page on plant tumors and genetic engineering, which seems rather inadequate considering the possible potential of such techniques, although they may have been considered to be outside the scope of this text. Overall, therefore, a useful laboratory manual for students of plant tissue culture.

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Evolution of fish species flocks. Anthony A. Echelle and Irv Kornfield (eds.). University of Maine at Orono Press, Maine. 1984. Pp. vi+257. Price $20.95US (PB), $28.95US (HB).

This book fills an important gap in the interpretation of the evolution of fish species flocks, it being 25 years