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Book Reviews

The Early Days of Yeast Genetics. Edited by MICHAEL N. HALL and PATRICK LINDER. Cold Spring Harbor Laboratory Press. 1993. 477 pages. Price $75.00. ISBN 0 87969 378 9.

The leading experimental organism in the early days of biochemical genetics was *Neurospora crassa*, followed closely by *Escherichia coli*. Molecular genetics stemmed in the first instance from the T-even bacteriophages. But *Saccharomyces cerevisiae*, as a good sexual eukaryote with the growth habit of a bacterium, always had great potential advantages. The potential of the organism became reality through the efforts of a few small but talented and enthusiastic research groups. This volume, which consists of 30 essays from a good sample of those most actively involved, provides as good an account as we are likely to get of the relatively slow beginnings and subsequent explosive development of yeast genetics. The interest of the book is further increased, at least for those with some knowledge of the yeast scene, by numerous photographs of the main participants.

The 'early days' of the title is a relative term; the contributors range in time from Ojvind Winge and Carl Lindegren, the pioneers in the 1940s and early 1950s, to John Carbon, Ben Hall and David Botstein, 25 or 30 years later. The editors have allowed each contributor to reminisce in his or her own way; the styles range from the sober and documentary to the racy and anecdotal. The contributions also vary greatly in the relative emphasis given to scientific issues as opposed to personalities. Brian Cox's account of his work on the extranuclear *psi* element comes closest to a scientific review, drawing attention to a fascinating and still largely unsolved problem. The diversity of approach makes the book all the more instructive and enjoyable. The authors are, to a large extent, all commenting on the same scene, but each illuminates it in a different way.

Most of the major leaders and innovators in yeast genetics are represented. Of those deceased, Lindegren and Winge are the subjects of brief critical biographies by Bob Mortimer, and Boris Ephrussi and Herschel Roman are represented by selections from their past writings. The most notable absentee is Piotr Slonimski, who must surely have been invited. In his absence, the book tells us too little about the exciting genetic analysis of the yeast mitochondrial genome, though the article by David Wilkie, who made some of the earliest observations in this area, does something to fill the gap. Slonimski's group also discovered the crucially important two-micron plasmid. This was developed as a transformation and cloning vector by Jean Beggs who is not here either – perhaps 1978 was just too late.

One question raised by Mortimer's account of the very early days is why, following the early breakthroughs by Winge and Lindegren, yeast did not come sooner to the centre of the stage. One reason perhaps was that it posed too many problems too early in the game. Winge obtained some of the first examples of gene–enzyme relationships in micro-organisms, but the sugar fermentations that he studied tended to show multiple genes for the same function. This poses no problem now, but, in the days when the *Neurospora* workers were trying to substantiate the oversimple but very fruitful concept of 'one gene – one enzyme', it seemed something of a distraction. Even less had the ground been prepared for Lindegren's novelties. He and his wife Gertrude discovered gene conversion, a then outrageous phenomenon that everyone now accepts. At the time there was a general wish to explain it away in terms of classical genetic mechanisms – polyploidy, aneuploidy or multifactorial inheritance. It was unfortunate that the Lindegrens placed so much emphasis on a phenotype (α-methylglucoside fermentation) that was susceptible to multigenic interpretation. Again (an example mentioned only marginally in this book), Lindegren, with Sol Spiegelman, investigated a phenomenon of great novelty and interest – the delayed and apparently mutation-like adaptation of a certain galactose-negative mutant to growth on galactose. In the light of what we now know, this can be quite reasonably explained in terms of a positive feedback loop in the control of genes of galactose utilization. But Spiegelman and Lindegren's ingenious hypothesis in terms of extranuclear galactose-dependent self-replicating templates attracted much interest but no support. Spiegelman soon turned, with great success, to RNA 'phages and animal viruses, but Lindegren persisted with yeast and the utilization of sugars, especially the little-known sugar melezitose. The melezitose system, in Lindegren's hands, was so

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complex, and so eccentrically interpreted, that nobody has ever tried to sort it out.

The Lindegrens did provide some of the first breeding stocks of yeast, started the linkage mapping and, crucially, defined the mating types. The story of the post-Lindegren build-up of yeast formal genetics is well told in articles by Robert Mortimer and Don Hawthorne. Mortimer was responsible for two key advances. In the mid-1950s he made the first extensive collection of auxotrophic mutants, as much an essential basis for yeast genetics as it had been for *Neurospora*. During the same period he, together with his then student Fred Sherman, introduced snail stomach enzyme for the routine softening of yeast asci for much easier dissection of tetrads. Subsequently, Mortimer was involved in two extremely productive partnerships: with Hawthorne, in the 1960s, he greatly extended the yeast genetic maps, and later (in the years around 1970) he joined Seymour Fogel (who contributes his own account) in a massive tetrad analysis of intragenic recombination. The Fogel/Mortimer data still provide the most substantial demonstration of the formal rules, if not the molecular mechanism, of meiotic gene conversion.

The later sections of the book, which take us up to about 1980, deal with the origins of work on mating and mating type, the cell cycle, gene structure and expression, and molecular biology. The 'other yeast', *Schizosaccharomyces pombe*, gets only a minor role, corresponding, I suppose, to its lower profile in the world literature. Urs Leupold contributes a perhaps unduly short article describing how he started *S. pombe* genetics at the suggestion of Winge. Murdoch Mitchison tells us how *S. pombe* came to be adopted as a model organism for the study of the cell cycle.

The final 40 pages deal with departments and institutions. The field was seeded from a rather small number of places: the Carlsberg Laboratory in Copenhagen (Winge), Carbondale in Southern Illinois (Lindegren) and Paris (Ephrussi). The American effort, after Lindegren, came predominantly from Brooklyn College (Hurst and Fogel), Berkeley (Mortimer, later joined by Fogel) and Seattle (Roman and Hawthorne). Rochelle Esposito's lively, even joyful, account of the unique working environments in Brooklyn and Seattle is one of the best things in the book. Here, as well as in several of the other articles, we get a sense of Herschel Roman's beneficial and sustained influence. Another kind of institution, of great importance in the long run, was the Yeast Course, organized for many years at Cold Spring Harbor by Gerry Fink and Fred Sherman. From Fink's account, the course appears to have been part slave camp and part general riot. He credits Sherman with most of the riotous aspect, but we don't get the reciprocal view – Sherman's own contribution is all about cytochrome c. At all events, the course, over the years, had the effect of attracting a good number of extremely accomplished molecular biologists to yeast.

The last essay, 'The International Yeast Community' by R. C. 'Jack' von Borstel (the second of his two contributions), deals with the yeast conferences that have been held, in various countries, every few years since 1961. They seem to have been models of what specialist meetings should be – relaxed, informal and not too large. One hopes that the very evident community spirit has survived the great increase in the numbers of yeast geneticists.

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The Cell Cycle. Cold Spring Harbor Symposia on Quantitative Biology. Volume LVI. Cold Spring Harbor Laboratory Press. 1992. 780 pages. Cloth $210. ISBN 0 87969 061 5. Paperback $95. ISBN 0 87969 062 3.

Following the discovery of cyclins and the universality of the control of entry into mitosis, there was a great flurry of activity into the molecular basis of cell cycle progression in the period 1988–91. Fortunately, the production of this book was timed so as to include the great bulk of this work and for researchers and students of the cell cycle, the vast majority of the important experiments of this time are included here. The year of the 56th Symposium on Quantitative Biology, 1991, probably marked the beginning of the peak of activity of investigations into the occurrence of cyclins and their interaction with the ubiquitous Histone H1 Kinase.

The advantage of a meeting of this nature is the opportunity it affords to meet just about all the people engaged in cell cycle research and a major benefit comes from the informal exchanges of ideas and theories that take place away from the lecture platform. This of course cannot be brought out in any report on the proceedings and consequently one of the most important assets is lost. A flavour of this could be maintained by including a report of the discussions held immediately after the lectures. However, this does not detract from the value of this publication in which the most significant findings of an exciting field of research are described.

The meeting is summed up in the final section of the book where Tim Hunt gives an account of the coming of age of cell cycle research and a synopsis of the current knowledge concerning cdc2 activation and its putative role in the entry into mitosis; the involvement of G-protein in checkpoint controls; the role of START and how long it lasts; and the way that oncogenes fit into the current models of the cell cycle. At the moment most of this involves asking further questions but questions that are likely to be answered in the near future.

The first section of the book deals, logically, with START. Cross describes the *CLN* family of redundant

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