

The main concern of research, since it costs money, is with the investigation of nitrogen-fixing systems that are of economic benefit. Investigations of systems that are out of the way can sometimes produce information of use in understanding those with more economic benefit. Papers on stem nodules and nodules on cycads are there and are to be welcomed. The amount of effort still being expended on the understanding of *Azospirillum* associations is however surprising. This research has been going on some time now, and though it has its interest no one has so far shown it to be of economic importance; yet there are 38 contributions concerned one way or another with this organism. They do not dispel the view that, economically, *Azospirillum* associations are of no account. The group that is of more importance, the woody non-legumes, was much more poorly supported with only 14 contributions. It is however clear from the presentations that now *Frankia* can be cultured, great strides are being made in this part of the subject.

The evidence for lectins being concerned in the primary recognition events in plant microbial interactions has always been best for the legume-*Rhizobium* association, and a section on recognition provides further evidence on this point. There is also evidence in this section and elsewhere in the book that other bacterial polysaccharides than those concerned with lectin binding may have an effect on the initial stages of infection.

In relation to previous symposia there is a larger emphasis on the genetics of nitrogen fixation with sections on the host genetics, the genetics of free-living diazotrophs and of the microsymbionts. The genetics of the free-living diazotrophs is mainly concerned with the interaction of the nitrogen control genes *ntf* and the *nif* genes. This makes an interesting if complex story. The understanding of this is not helped by the fact that agreement does not seem to have been reached with regard to the nomenclature of some of these genes. Thus we have in one paper *glnF(ntfA)* and in the following paper *ntfA(glnF)*. The identification of the nodulation and nitrogen fixation genes on a plasmid in the fast-growing rhizobia, together with the technique of transposon mutagenesis, has led to rapid progress in the understanding of the genetics of rhizobia in relation to nodulation. The fact that there are over 40 contributions on this topic shows that it is a focus of interest to geneticists. There are few offerings dealing with the genetics of the slow-growing rhizobia, where the genes appear to be on the chromosome. But the presence of a very large plasmid cannot be ruled out at the moment, and everyone will be relieved when the whereabouts of the genes in these organisms are finally decided.

All in all there are good things in this book whatever one's speciality within the subject. The very great merit of this book is its immediacy, and for those who wish to get or keep up with the latest research in nitrogen fixation it will be very useful indeed.

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Banbury Report 14. *Recombinant DNA: Applications to Human Disease*. Edited by C. T. CASKEY and R. L. WHITE. Cold Spring Harbor Laboratory. 1983. About 375 pages. \$55 U.S. (\$66 outside U.S.) ISBN 0 87967 214 6.

The Banbury Report series started a few years ago as the records of small and select meetings on biological subjects, with emphasis on the real and potential dangers of environmental agents to human genetic material. The first thirteen Reports followed this general plan quite closely; Number 14 breaks new ground by plunging directly into one of the most exciting and fast-moving areas of modern biology, the application of the new recombinant DNA technology to human genetic and quasi-genetic disease. The meeting was held in October 1982, and the contributed papers together with full transcription

of the exchanges which followed were published within a year. This is fast moving for any type of conference proceedings.

The first task of a reviewer is to ask to what kind of readership the book is directed. The cynic might reply that most copies of Banbury 14 will go to those libraries in the U.S.A. and elsewhere who have standing orders for the series, and who have already built up a fine collection of authoritative accounts of the effects of mutagens and carcinogens on mammalian systems. The same cynic would doubtless point out that the purpose of publishing conference proceedings is primarily to pay for the expense of organizing the meeting and bringing participants together from their distant laboratories. Given a sufficiently distinguished group of contributors, any record of their work and discussion is likely to contain some nuggets which will inform, educate and even entertain the reader. Is it not time for those who have fed on Banbury 3 'A safe cigarette' or Banbury 12 'Nitrosamines and human cancer' to toy with the delicacies of recombinant DNA?

Perhaps it is. But not in the form presented in Banbury 14. This is a book for the expert, for the scientist already deeply involved in and committed to the exploration of the new genetics. For those on the fringe, however ready and willing to be carried along by the high priests of molecular biology, there are no concessions. There is no introduction or overview of what the conference hoped to achieve, and the three-page summary by one of the editors is written as though he had an eye on the time of the next plane back to Houston. If you have trouble with the opaque jargon of recombinant DNA technology, this is no place to start.

Will it then delight the expert? Somehow I doubt it. Molecular biology is moving too fast for any conference proceedings to do more than mark how it was at the time of the meeting. Consider the paper by James Gusella *et al.* on 'Mapping the Huntington's disease locus'. At the time that the paper was written nothing was known about the location of the Huntington's locus, nor were there any informative linkages. Gusella *et al.* planned to tackle the problem by generating random probes, picking out those which recognized endonuclease polymorphisms and then searching for linkages in large Huntington's disease kindreds. A huge task, and the curious absence of any commentary on this paper (did the tape recorder break?) will induce the average reader to move rapidly to more profitable sections of the book. And yet, as all readers of the popular press now know, Gusella struck paydirt on one of his very early probes, established a linkage to Huntington's and localized the gene on chromosome 4. This was reported in *Nature* on 17 November 1983 (paper received 5 October). How quickly irrelevant the Banbury conference of October 1982 had become.

Is this an isolated example? I think not. The phenylalanine hydroxylase gene was cloned in 1983 and prenatal diagnosis and heterozygote detection of phenylketonuria are now practical though limited propositions. This was reported in *Nature* by Savio Woo and his colleagues on 10 November 1983. Woo was at Banbury, where he talked about his work on α_1 -antitrypsin deficiency. A few references to phenylketonuria occur in the discussion of Woo's paper, but he either did not know yet about his imminent cloning of the gene or was not to be drawn on it. Too bad for the non-specialist reader.

Banbury 14 thus falls between two stools. It is too complex and jargon-ridden for the enthusiastic amateur and too dated for the professional. If ever evidence were needed against the automatic publication of conference proceedings, it is to be found here.

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