

cellular level. For example, a new approach to the study of basic transplantation mechanisms is demonstrated by the application of a cell culture system for the *in vitro* induction of graft reactions; while other investigators have used a bacteriophage as hapten carrier in studies of the influence of valency on the kinetics of hapten-antibody interaction. Another technique of obvious potential offers a new method of attaching antigenic determinants to erythrocytes, which avoids damage to the cells and non-specific reactions. This should prove particularly useful in Jerne plaque studies, in which it has already been used to detect single anti-hapten forming cells.

There are two papers dealing with immunopathological topics, one providing a useful review of the recently described γ A immunoglobulin abnormality commonly known as "α chain" disease and the other reporting the results of a study of the spontaneous *in vitro* degradation of pathological γ M globulins on treatment with urea. Other reports are concerned with the fractionation of mice isoantisera cytotoxic for thymus and lymph node cells; and with the characterization of rabbit antibodies to a limited antigenic stimulus, which it is suggested may be triggering a single clone of cells. There is even a paper, too, on "homocytotropic" antibodies in the primary and secondary responses in rabbits.

The remaining bulk of papers are devoted to the many important aspects of the cellular basis of immunity with which immunologists are at present wrestling; for example, the *in vivo* fate of antigens, the nature of their interaction with lymphoid cells *in vitro*, antigenic competition, stimulation by cross-reacting antigens, modulation of immunological responsiveness by pretreatment with a fraction of tubercle bacilli and the specificity of immunological tolerance. As might be expected, the usefulness of synthetic polypeptide antigens is brought out in many of these studies.

In my opinion, however, some of the most interesting work reported is that concerned with further investigation of the importance of the charge of the antigen on the nature of the antibody elicited. As a result, the hypothesis has been put forward (by Benaceraff and associates) that a charged antigen will preferentially select on the basis of thermodynamic considerations and stimulate cells synthesizing specific antibody of the opposite charge.

These are small crumbs for the immunochemist who delves into what is, after all, a feast of cellular immunological topics. Still, at a time when even the most ardent molecular men are showing more than passive interest in the happenings at cell surfaces, this book affords an ideal opportunity for everybody to get acquainted with many of the challenging problems which lie ahead for those engaged in furthering knowledge of immunological phenomena. DENIS R. STANWORTH

A PERSONAL VIEW

Cell Fusion

By H. Harris. (The Dunham Lectures.) Pp. 108. (Clarendon Press: Oxford; Oxford University Press: London, February 1970.) 40s boards; 20s paper.

PROFESSOR HENRY HARRIS is always an engaging lecturer. He speaks with infectious enthusiasm about his research; one way or another he never fails to be provocative and he has never shunned demolishing current shibboleths. These three Dunham Lectures, which he gave at Harvard last autumn, are entirely in character. They centre around the most impressive series of experiments carried out in his department since he and Watkins discovered, in 1965, that inactivated Sendai virus induces cells of unrelated species to fuse, often to yield viable hybrids. And his review of cell fusion, especially of his own group's contributions, is bound to provoke others in the business to more than scientific discussion.

Professor Harris devotes the first lecture to describing

the general biology of hybrid cells and the history of their discovery and production. Naturally enough, he stresses the way in which the Harris and Watkins experiment of 1965 has transformed the subject. As he says, the most surprising thing about this famous experiment is that it had not been done much earlier. All the evidence suggesting it would work was freely available, but the fact remains that, from 1960 until 1965, nobody then working on hybrid cells realized the potential of Sendai virus as a mediator of cell fusion. It remained for Harris and Watkins to make available "a precise method for producing, at will, interspecific heterokaryons of known composition" which has converted cell hybridization "from an exercise in the exploitation of chance events to a method of general applicability".

A glance at any current journal of genetics, virology, cell biology or molecular biology is enough to prove just how widely the Sendai virus technique is now being used. Needless to say, Harris and his colleagues have not let the grass grow under their feet. Their experiments with hybrids between mammalian cells and hen erythrocytes have led to the important generalization that the signals which pass from the cytoplasm to the nucleus and lead to nuclear activation are not species specific. They have shown that hybrids between malignant and non-malignant cells are not always malignant. Or in Harris's colourful words "that malignancy can be suppressed in this way and that the suppression is amenable to genetic analysis are findings that can hardly be irrelevant to the future progress of cancer research". They have also shown that the movement of mRNA from the nucleus to the cytoplasm seems to be intimately linked with the existence of a functional nucleolus and the movement of ribosomes in the same direction.

These and many of the other important hybrid cell experiments carried out since 1965 are eloquently reviewed and anybody interested in cell biology is well advised to read what Professor Harris has to say. The only blemish is a strange lack of generosity to the other workers who have made outstanding contributions to the study of hybrid cells. The work of Ephrussi and his colleagues, in particular, is singled out for what many must consider gratuitous criticism. JOHN TOOZE

STRAIN IMPROVEMENT

Genetics of Antibiotic-producing Microorganisms

By G. Sermonti. (Techniques in Pure and Applied Microbiology.) Pp. xiii + 389. (Wiley (Interscience): London and New York, November 1969.) 130s.

SERMONTI writes in his preface that this book is intended to provide the industrial microbiologist concerned with antibiotic production with "the techniques and rationale of microbial genetics". The book contains useful chapters on the approaches and techniques involved in the mutation and screening of microorganisms. A very relevant chapter deals with variation within clones and strains, and emphasizes that, in most cases, microorganisms are best considered as populations. Gene recombination and parasexuality in moulds are dealt with in detail and there are also chapters on the genetics of bacteria and streptomycetes. The book is in general well illustrated and contains many useful references.

The need for this book is made clear in the final chapter entitled "Genetics of Antibiotic Production". Sermonti has selected only penicillins and tetracycline for consideration. The underdeveloped state of the field, he suggests, is partly because of inherent difficulties and partly because of commercial pressures which favour the application of empirical and short-cut methods for strain improvement. In addition, advances in microbial genetics on the one hand, and in industrial fermentation on the other, have largely remained distinct. Further progress will occur only when scientists, well trained in microbial