BOOK REVIEWS

PROBLEMS WITHOUT INSIGHT

Major Problems in Developmental Biology

Edited by Michael Locke. (25th Symposium of the Society for Developmental Biology.) Pp. x+408. (New York: Academic Press, Inc.; London: Academic Press, Inc. (London), Ltd., 1966.) 128*s*.

THIS anniversary volume celebrates publication of the twenty-fifth symposium of the Society for Developmental Biology. What has developed in developmental biology during this period? For one thing, a new view has appeared, and seems now to generally prevail, to wit: "Differentiation (is) the outward sign of selective gene action . . . We shall make use of . . . the increasingly plausible concept . . . that superimposed on the basic template principle . . . there are control mechanisms." (James Ebert and M. E. Kaighn, second chapter of the present volume.) That development constitutes the orderly, sequential turning off and on of the right genes in the right places at the right times is a proposition which few would contest today. The central questions of developmental biology are: How are genes repressed? How are they turned off and on? and How does the programming of gene expression work? These questions, which lie at the heart of developmental biology, are not dealt with in the present volume. *Major Problems in Developmental Biology* deals rather with the new descriptive embryology, the fact that different specialized cells have different enzymes, respond to different media with different growth patterns, are differently affected by hormones, and so on. It may well be that hidden in this wealth of descriptive material of different developmental systems is the system which, by its study in depth, will reveal a new central principle or principles about how development works. One cannot today point to that system.

Major Problems in Developmental Biology is none the less not to be sneezed at as a book to read. The introductory chapter by Jane Oppenheimer, a "what were they doing 25 years ago?" kind of chapter, is wonderful. Holtfreter was studying the development of pieces of gastrulae; Harrison, induction by the neural crest; and Brachet, the number and location of sulphydryl groups in developing amphibian embryos. Change was, however, just around the corner. Beadle, Ephrussi and Tatum were shortly to establish biochemical genetics and hence molecular biology, and then Ebert's and Kaighn's present formulation of developmental biology. The second chapter, by James Ebert and M. E. Kaighn, is a thorough review of modern work of development on the cellular level, and even to some extent on the molecular level. "Are there cases of differential DNA replication other than that which has to do with the nuclear organizer?" they ask—a good question and one that deserves to be settled. Great emphasis is placed, too, on the numerous cases of which we now know in which DNA replication is the prerequisite to change of gene state from repressed to derepressed, or vice versa. A third chapter by E. Hadorn reviews his work on determination as studied with fragments of imaginal disks of insect larvae, which can grow apparently for ever as cell cultures in the abdomen of adults, but which will differentiate briskly if transplanted to a larva. C. H. Waddington discusses fields and gradients, a subject obviously not to his taste, while J. P. Trinkhaus reviews morphogenetic cell movements, coming to the conclusion that there are many unsolved problems in this field. So far as the aggregation of similar cells out of a mixture of dissimilar cells, Trinkhaus votes for selective adhesion of like cells. H. Ursprung in "Patterns of Development" comes to the conclusion that it may well be that all patterns flow from the amino-acid sequences of proteins, and thus directly from nucleotide sequence in the gene. Maybe so—but it's a long step from nucleotide sequence to patterns in butterfly wings or banding of feathers. D. E. Koshland and M. E. Kirtley bring a new discipline, protein chemistry, to the Society for Developmental Biology. They develop a new general theory of control of gene and cell activity. On the level of transcription, Koshland and Kirtley propose that a polynucleotide, presumably the product of the specific DNA sequence involved, is bound covalently to a protein, possibly also the product of that gene, which in addition contains an appropriate binding site for a small effector molecule. They do not specify how such an RNA protein molecule by binding specifically to the gene in question would cause that gene to be repressed so far as transcription is concerned.

Anton Lang next takes up the new knowledge of the action of plant hormones. Thus, it is now clearly established that for gibberellic acid to exert its effect by increasing the rate of plant cell elongation, DNA synthesis must first occur. Induction of cell elongation by indole-acetic acid, a second plant hormone, does not require this intermediate step. Nature is clearly telling us something here, but we do not yet understand her message. Interestingly enough, the influence of gibberellic acid on the aleurone layer of the barley endosperm, an influence which causes the *de novo* production of α-amylase and other hydrolytic enzymes, does not appear to require DNA synthesis. J. W. Saunders and J. F. Fallon review cell death, and point out that many cells die in the course of and as a part of normal development. Cell death in early developmental stages is therefore apparently genetically pre-programmed. H. Rubin discusses in detail contact inhibition and the loss of contact inhibition in the transformation of tissue culture cells to cancer cells *in vitro*. Rubin believes that contact inhibition is caused because the cultured cells prefer to stick to the culture dish rather than to each other, and that loss of contact inhibition is caused by loss on the part of the cells of their affinity for the culture dish. He further suggests that transformation is caused by some sort of self-perpetuating change in the affinity of cell membranes for things like the bottoms of culture dishes. His suggestion should be a testable one.

Finally, and this is a new direction for the Society for Developmental Biology, Marcus Jacobson, in a long and interesting chapter, discusses "starting points for research in the ontogeny of behavior". This important new direction is certainly one still appropriate for study by descriptive methodologies.

Major Problems in Developmental Biology does not therefore really state or encompass the central principal problems of the subject. It does none the less provide an evening of pleasant reading. JAMES BONNER

WHAT SCIENCE EXPLAINS

Completeness in Science

By Richard Schlegel. Pp. xi+280. (New York: Appleton-Century-Crofts, a Division of Meredith Publishing Company, 1967.) $7.50.

As the tide of protest against scientism slowly gathers momentum it is very important that a number of funda-