possible in man by the emergence of mind and by the pooling of experience through tradition.

A second point which has struck me as requiring searching analysis, is the extensive and loose use now prevalent of the concept of "self-reproducing" particles or structures. A salutary criticism is to be found at the end of Sonneborn's essay. We all agree that there are cell structures ranging from chromosomes and their parts to viruses and transforming principles, which the cell is usually unable to synthesize *de novo*, and that certain changes in such structures lead to synthesis of replicas of the changed type. If we use the term "self-reproduction" in this descriptive sense, and remember the simple analogy with the "self-reproduction" of glycogen in the presence of a trace of it, of the appropriate enzymes and of glucose-phosphate, there is no harm. However, there is an animistic tendency to attribute to genes, viruses and other such particle properties well beyond these. When we speak of the gene as reproducing *itself*, and *controlling* a metabolic process we tend to think of it as a *wee one*; but immediately afterwards we see it as a macromolecule. This makes for unclear thinking; the sooner we stop using the term "self-reproduction" the better; "genetic continuity" is safer, and the emphasis should shift on to the action of all these structures as specific primers and the part they play in the synthetic processes of the cell.

A third point is the small amount of attention which is at present paid to the problems of spatial organisation of biochemical processes in the cell. Apart from a passing remark by Sturtevant on the promise of position effects, a reference by Darlington to Peters' ideas and one by Sonneborn on the possible assembly-line systems of enzymes on the microsomes there is barely any mention of this problem. Yet, as the reviewer has been stressing for some time, biochemistry is reaching a dead end if it does not find means to bring in the space variable, and genetics has to offer, with crossing-over, a tool of structural analysis the extraordinary resolving power of which is unlikely to be equalled for a long time.

It is impossible to mention here all the twenty-six essays, let alone to do them justice. Some are of historical character and they make most instructive reading. Some are reviews of well delimited fields—such as one on immunological genetics by Irwin, one on chemical genetics by Beadle, one each on cytochemistry by Mirsky and by Caspersson and Schultz, one on bacterial genetics by Lederberg, one on the genetics of cancer by Little, one on population genetics by Dobzhansky, an excellent one on hybrid corn by Mangelsdorf, one on genetics and plant pathology by Walker. Others, like the short stimulating paper by Penrose, deal with a particular approach or piece of research.

The Jubilee Celebrations were intended as a survey of the work done in the fifty years from the "rediscovery" and of the perspectives for the future: this volume certainly achieves this purpose well and pleasantly.

G. Pontecorvo

GENES, PLANTS AND PEOPLE. Essays on Genetics. By C. D. Darlington and K. Mather. xxi+187 pp. London: George Allen & Unwin. 16s.

This book is a symposium of the semi-technical writings of two geneticists at successive stages in the development of their thought. At the very least, it provides in compendious and accessible form a number of published papers which the younger geneticist can now hardly hope to get as reprints; but, fortunately, the writings of both authors show a continuity of scientific purpose that raises this volume above the level of genetical *belles lettres*.

Mather's articles begin with, as they later develop, the theory of mating systems, and the importance of an understanding of mating systems is a theme common to both authors. There is an inclination among teachers of elementary genetics to dismiss the theory of mating systems as a mere enumeration of the bye-laws of heredity. And so, in a sense, it is; for mice mate otherwise than men and obey somewhat different rules of gametic union, though their genes may be supposed to behave in very similar fashion. Elementary expositions of genetics therefore concentrate on those rules (of the behaviour of chromosomes and genes) that are of all but universal competence, and tend to neglect those that govern the coming together of gametes, because they vary from one species to another or even (as with human beings) from one part to another of a single one. The logical wrong-headedness of such a treatment is clear enough: that genes are the coins of lowest denomination in genetical transactions is simply an analytic fiction, for in *fact* gametes are the only legal tender. Chromosomes from two animals will combine only if their gametes do. The theory of the modes of gametic union has therefore some claim to genetical priority.

Mather's later essays deal with " continuous " variation, i.e. with discontinuous variation so fine-grained and so rich in combinational variety as to determine a virtually continuous spectrum of character differences; with genetical buffering systems (cybergenetics?); with the evolutionary import of chromosome behaviour; and with the operational definition of " the gene ". Mather is an analyst by temperament, and it is interesting to see how his essays reveal a chronological increase of analytic power. It is most apparent in his essay on " The Gene ". The " nature of the gene " is a function of the operations used to define it: there is a Mendelian character-difference gene, a gene of mutation, a gene that is the unit of chromosome pairing, and so on. The integrity of genetic theory depends upon their being a high degree of overlap and therefore of interchangeability between these several concepts. Mather's analysis is first-rate, though it would have been enriched by some treatment of cognate problems in other sciences. Physicists have these difficulties too.

Darlington's essays are representative of all his major contributions to genetic and cytological theory: on the relationship between meiosis and mitosis, of which he provided the first complete theory; on chromosome mechanisms considered no less as the products than as the instruments of evolutionary change; and, most far reaching of all, on the relationship between heredity, development, and infection. Darlington's prose style, in his later essays urgent, peremptory and intolerant, seems to reveal an impatience of fine analytic or inferential thought. " If this is not correct," says Churchill of his own exposition of the principles of Radar, " it should be "—and there is something of this intellectual temper here. Darlington's is a mind that moves most easily upstream of the flow of deductive inference —the sort of mind, indeed, that is responsible for formulating the great connecting principles of science. His interpretation of the affinity between the mechanisms of heredity, development and infection reveals an enormous intellectual grasp: no biological synthesis of comparable pretensions has been set before the public in the present century. His hypotheses may be criticised in two sorts of ways. On the one hand, it may be said that the facts he starts with are wrong, or, if right, are made to depend from hypotheses that lead to unfulfilled predictions. It is altogether proper that criticisms of this sort should be made, and so they have been. On the other hand, it may be argued that speculation of this degree of rarefaction is in itself a somewhat disreputable activity, stifled at birth or by early training in those with a better-developed sense of scientific propriety. This is a most mischievous attitude of mind. All sciences, as would-be organised bodies of information, have to counteract the pressure of a sort of intellectual entropy —that is, the dissipation of knowledge into a rabble of particular unrelated facts. Integrative thought of the sort and on the scale indulged in by Darlington is an essential corrective to this tendency; it is all that prevents biology or, indeed, any science, from deteriorating into a mere taxonomy of scientific facts.

P. B. MEDAWAR

SOVIET GENETICS. By Alan G. Morton. London. Lawrence & Wishart. 1951. Pp. 174. 15s.

Dr Morton's book is described on the dust-cover as an unbiased account of the Michurinist theory of heredity with supporting scientific experiments. The author does not claim years of experience in Genetics or plant breeding, and thus he is free from the necessity of believing in any one theory of heredity. He should be able to make a perfectly balanced statement.

Despite the difficulty of penetrating " the curtain of ignorance and misunderstanding with which the Soviet Union is unfortunately so frequently surrounded ", the Michurinist theory is now familiar to most geneticists outside the U.S.S.R. But it will be new to most readers to learn that the regularities of Mendelian ratios—including the precise segregation in the tetrads of pollen grains and reproductive spores of some fungi—are now accepted by Michurinists. Clearly, since 1950, Soviet scientists have benefited from foreign travel. They still deny, however, the existence of determinant particles or genes and explain Mendelian segregation as the " result of the destabilised or shaken heredity caused by hybridisation "

Turning to the facts on which the Michurinist theory is based we find the true and orthodox Mendelian statement on page 96 that the red tomato fruit is dominant to yellow, and yet on page 101 we find: " Of 633 control fruits in F_1 all were yellow." Has F_1 taken on a new meaning in Michurinist experiments or have the controls been shaken by mistake? Unfortunately it is impossible to decide even after repeated reading. But some experiments are described without such ambiguities. For example there is Khachaturov's (1939) selective fertilisation in Tobacco. First generation hybrids were self-pollinated with amounts of pollen on each pistil varying from five grains to a large mass. The second generation hybrids derived from the large mass of pollen were " rather uniform in height, earliness and appearance ". " The plants from the low pollen fertilisation were much less uniform in character, and half of them were of types not found among the normal F_2." This is a very interesting and important demonstration of the effect of selection on the male gametophyte. Dr Morton concludes: " These data are at variance with accepted Mendelian ideas of the ' purity of the gametes '." If this is the author's personal interpretation he has failed to grasp the elements of Mendelism; if it is an inspired conclusion it has