BOOK REVIEWS

A CLASSIC OF GENETICS

INTRODUCTION TO QUANTITATIVE GENETICS

By D. S. FALCONER

(Oliver and Boyd, Edinburgh and London, 1960, pp. ix+365, 35s)

"What the world's million lips are thirsting for
Must be substantial somewhere",

said W. B. Yeats, rather optimistically. Here, at any rate, is a book for which I have long been waiting. Unlike some other books on the same subject, it is based on a mass of experimental data, and if, as is often the case, e.g. pp. 208-212, the facts do not conform too well to any particular theory, so much the worse for theory. However the theory which is currently orthodox in "Western" genetics, and is probably fairly close to the truth, is well presented. The mathematics are set out as simply as is practicable, but still some geneticists will find them difficult. I would ask such readers to bear with the author. I do not think he has put in any mathematics for its own sake.

Every geneticist should read this book. It is so very good that I hope that it will run to many editions. So the criticisms which follow are not hostile. There should be more data on plants and men, and the statements about them should be carefully scrutinized. Thus many authors think that hybrid vigor has been responsible for about 30% improvement in the U.S. Maize crop, and better agronomy for 20%. Falconer (p. 277) seems to attribute the whole increase to genetical causes. Another subject which I should like the author to take up is the responses of genotypes to different environments. Such work as that of Chandraratna on the response of rice genotypes to different day lengths is a fine example of quantitative genetics, as is Bonnier's use of monozygotic cattle twins to test the effects of different dietaries on milk yields. Finally the work of Gustafsson, Sakai, and Roy on the interaction of genotypes has opened up a new field of quantitative genetics which will certainly be important in agriculture and may be so in evolution theory.

The book is largely based on the author's experience in the Agricultural Research Council's Edinburgh Unit of Animal Genetics. It is sufficient by itself to justify the existence of this Unit, and the appointment of Professor Waddington as its head. But if Dr. Falconer could now spend a year (or better thirteen months with two harvests) at a plant breeding institution, I believe that this would lead to a manifestation of hybrid vigor on the intellectual plane, beneficial to both parties.

J. B. S. HALDANE

STATISTICHE METHODEN DER POPULATIONSGENETIK

By HENRI LOUIS LE ROY

1960, pp. 397, (Birkhaüser, Basel and Stuttgart, 67·5 Swiss francs or W. German marks)

I expect this book to be widely read and translated. It is the finest example that I have seen in genetical literature of the point of view which, in physics, led to problems about a small elephant whose weight may be neglected. It is not easy to review, since there is no index in the ordinary sense. But so far as I can see, there is not a single reference to mutation, which some biologists regard as playing an important part in population genetics. Nor could I find any reference to the effects of inbreeding in human populations.

The book seems to be mainly concerned with the problems arising in selective breeding of plants and animals. But so far as I can make out, only 6 pages (335-341) are devoted to heterosis, or hybrid vigor, largely to "Estimation of mean overdominance". There are several references to monozygotic twins, but none to Bonnier's work on cattle twins for testing the effects of environment. In breeding cattle for milk production and poultry for egg production we are concerned with female characters, but can improve them largely by selecting males. One would think that this problem was worthy of mention. But I failed to find such mention, partly perhaps because the substitute for an index contains no such mundane items as "milk", "egg", "cow", or "hen". One might have hoped that a statistician would have been interested by such very ingenious statistical work as that of C. A. B. Smith and N. E. Morton on human population genetics. But it did not fit into the author's rather Procrustean framework.

This book is admirably fitted as a text for a university course leading to a competitive examination. Whether the students securing the highest marks in such an examination would be well qualified for improving animal or plant production is another question.

J. B. S. Haldane

AGING AND MUTATION

THE BIOLOGY OF AGING, edited by Bernard L. Strehler and others, 1960, pp. vii + 364. American Institute of Biological Sciences, Washington, D.C., **n.p.**

This is the record of a conference held at Gatlinbury, Tennessee, in May 1957, but the preface is dated September 1960. Meanwhile C.I.B.A. has published several volumes on the same topic. The first part of the book is a record of discussions; the remainder consists of 50 articles with a mean length of 4·8 pages, not fully covered by print. These are mostly summaries of work published elsewhere. A few of these are quite interesting, notably Failla's discussion of somatic mutation as a possible

cause of aging, and Casarett's evidence that many of the effects of chronic high frequency irradiation are like those of normal aging. Huennekens' biochemical rejuvenation of aged erythrocytes is perhaps the most exciting story in the book.

I doubt if such a book serves a useful purpose. As however the price is not stated, it is unlikely to find wide circulation outside its country of origin.

J. B. S. HALDANE

OFFBUMPING ONGOING

THE PROCESSES OF ONGOING HUMAN EVOLUTION, edited by G. W. Lasker, 1960, pp. 108, Wayne State University Press, $ 3·75.

This book is, in my opinion, a noble, interesting, and highly readable failure. The editor set out to assemble the evidence as to human evolution as an observable process occurring to-day. All authors are Darwinists, and the first five articles are concerned with natural selection. Baker has a lot to say about selection for large surface per unit weight in hot countries and the opposite character in cold. As he writes "it remained for Schreider actually to combine this data into a surface area over weight ratio and correlate it to the climatic environment of different populations". He cites two letters of Schreider to "Nature" in 1950 and 1951. Since then Schreider has written numerous papers on the subject in "Biotypologie", but these are not mentioned.

Livingstone has a rather speculative article on blood group gene frequences in West Africa, and makes a valiant attempt to derive the relative fitnesses of the six genotypes, with the amount of gene drift between different breeding units. If he is right he has made a big methodological advance. But I suspend judgement.

Motulsky, though specially interested in primaquine sensitivity, gives the best review known to me of human biochemical polymorphisms which are thought to give protection against diseases.

Hulse's article on "Adaptation, Selection, and Plasticity" excited me considerably. He seems to have proved that heterosis produces an increase in human stature, both in Switzerland and California. But he is apparently unaware of Russell's results in the British Journal of Psychology which give evidence that intelligence is similarly determined. Similarly Lasker in "Migration, Isolation, and Evolution" ignores the work of Khanolkar and Sanghvi on Indian groups, though this was published in a London journal. The last article is by Reynolds on irradiation and human evolution. I happen to agree with it, but not much of it is new.

The main lesson of the book is how little we know. Many populations are getting taller. This can be due to diet, selection, and heterosis. We have no idea of their relative importance, and only the second falls under the heading of evolution. If genetics had been discovered before bacteriology we should know a lot more about natural selection in man. To-day we must go to under-developed countries to study

natural selection by disease. "Offbumping ongoing" was a correspondent's announcement of a revolution. Offbumping before maturity is now rather slight in economically developed countries; yet as family sizes differ, we have no reason to doubt that natural selection is still at work. But, to quote Hulse on one particular population, in most cases "no evolutionary trend can be either observed or predicted on the basis of the data on hand".

This is an honest book. On looking at the cover, ornamented with a left-handed Diskobolos, or Chakravarti, on the front, and a right-handed one on the back, I feared that it was going to be propagandist, and state that we were rapidly changing into angels or devils. This is not so. But the cost of the Diskoboloi could have been better spent in giving it an index.

J. B. S. HALDANE

GENETICS AND CANCER

University of Texas Press, Austin 1959, pp. 459, $ 8.50

This book is a collection of papers presented at an annual symposium on fundamental cancer research. Cancer research is connected with genetics in three distinct ways. First we may study the genetics of spontaneous cancer, or cancer developed in response to a stimulus, secondly the genetics of resistance to transplantable cancer, and thirdly the cytology of cancer cells, and their possible genetics. Three authors gave new information about spontaneous cancer. Anderson studied cancer of the eye and eyelid in Hereford cattle, which is fairly common over the age of 6 years in Texas. As eyelid cancer only occurs in the white areas, the white face gene of this breed must be regarded as sublethal in strong sunlight. Apart from this the tendency to cancer is quite strongly inherited, and an adequate analysis might disclose its genetic basis. Macklin has collected valuable data on the heredity of human breast and stomach cancer. She confirms the greater frequency of breast cancer in relatives, but finds it increased in paternal as well as maternal grandmothers and aunts. The same is true for stomach cancer. On the other hand she got strong evidence against a general hereditary factor for cancer. Schull and Morton spoke on human genetics in general. Both were rather dogmatic, and, as I believe, wrong on various details. For example I think the argument leading to Morton's Table 5 is incorrect. A woman may bear a son with sex-linked muscular dystrophy for three distinct reasons. She may be heterozygous, a gene in one of her pronuclei may have mutated, or mutation may have occurred much earlier in her life so that perhaps one ovary but not the other is heterozygous. Until we know something about the frequency of sectorial mutations we cannot assume as he does that there is a sharp distinction between segregating families and single sporadic cases.

Several authors studied the changes which occur in the chromosome number and antigenic properties of transplanted human cells. Levan however makes it clear that similar changes occur in cultures of normal tissues. However Tjio and Östergren have found heterochromatism in one chromosome in 18 out of 19 tumours caused in mice by the Bittner milk-borne virus. This, and many other facts, suggest that when Professor C. D. Darlington, to quote his own words in the opening article of the book "felt that we could exclude the nucleus from the problem of cancer causation", he was ignoring so much evidence that his views require no further consideration. Wollman and Jacob suggest that lysogeny and related bacterial phenomena "might serve as useful models in the analysis of cancer genetics". To this reviewer by far the most exciting paper in the volume is that by Latarjet. He produces several pieces of evidence that carcinogenesis is not an event occurring in a single cell. He claims that it requires the removal of growth inhibiting influences of neighbouring cells, and mutation, virus infection, or some such event, in one or more cells ancestral to the tumour. This, he suggests, can be due to substances released by cells killed by radiation.

Haas and Doudney, in an extremely interesting contribution, bring forward strong evidence from the study of bacterial mutation that DNA is not directly copied, but that its pattern is transferred to RNA and possibly protein. This idea, due to Stent and Zubay, is of course an elaboration of the views put forward by Gall and myself in 1953. But Haas and Doudney reach the gloomy conclusion that if most of the carcinogenic changes take place during the synthesis of new genetic DNA, "it is difficult to see just how cancer can be prevented". If so it follows that most cancer research is worthless. If, as Latarjet believes, cancer involves the interaction of cells of different genotypes, it is possible that work on genotype interaction, such as that of Sakai and S. K. Roy, may be more relevant to the prevention of cancer than most of the research here described.

The printing and proof-reading are good. I only detected one misprint (on p. 352). But the editing is not so good. I believe that the "L cells" first mentioned on p. 184 are human cells. But I am not sure. An editor should remember that others may be as ignorant as I. However the book contains a great deal which will interest geneticists, even if they are not concerned with cancer. I at least shall continue to dip into it in search of queer facts of which anyone interested in how cells are reproduced must take cognizance.

J. B. S. HALDANE

MARSHALL'S PHYSIOLOGY OF REPRODUCTION

Third edition, Vol. 1, Part 2, edited by A. S. Parkes. pp. xx+877, £ 12-0-0. Longmans Green, London, 1960.

When Parkes took over the editing of a new edition of Marshall's great book, he may not have realised the vastness of his task. This volume consists of eight chapters, the

first five mainly concerned with spermatozoa, the last three with hormones. The late Arthur Walton is mainly responsible for the first part, Dodd, Parkes, and Amoroso for the second. The subjects are very fully covered, though there is a tendency to neglect French work; for example there are no references to Soulairac or le Magnen.

When a book which was originally a unit is divided up there is a danger that certain facts will be left out. This volume is mainly concerned with vertebrate physiology. But while Austin and Walton consider invertebrates in their chapter on fertilization, there is no mention of the crustacean system in the chapters on hormones. No student of vertebrate sex physiology can neglect it, and I hope it will be or has been described in one of the other volumes.

A geneticist may find the book irritating, as a little more information on genetics (for example of the feather response to steroids) could often have been given. But it is indispensable. Any geneticist working on vertebrates must be able to consult this volume. Its high price is doubtless due to the numerous and excellent illustrations. The gift by the British Government of a few sets of "Marshall" to university libraries in poor countries might do something for the prestige of British science. But for many potential readers the high price will render it an inaccessible treasure.

J. B. S. HALDANE

THE CLASSICAL THEORY OF IRRADIATION

ACTIONS OF RADIATIONS ON LIVING CELLS, BY D. E. LEA pp. xiv+416, Cambridge University Press, second edition 1955, reprinted 1956, £ 1-15-0.

This reprint raises a most interesting question. The first edition, in 1946, was in its way a classic. It gave a coherent account of the actions of ionizing photons and "material" particles on living cells, viruses, and enzymes. This account was shown to be incorrect in a very important respect when Thoday and Read, in 1948, found that chromosome breakage by X-rays is increased about fourfold when air is substituted for nitrogen. Had Lea lived he would of course have modified his book accordingly. His second chapter shows that he was prepared for such a possibility.

This second edition is a reprint of the first, with some corrections made by Lea himself, and the amendment of a mathematical mistake. There is a misprint on p. 369 which Lea might have detected. The fact that a further reprint has been called for shows that there is a demand for the book. We have only to compare it with the diffuse and expensive productions of other authors to see why. Here at least is a clear-cut presentation, and where it is wrong one can see how it is wrong. I think how-

ever that the value of the reprint would have been enhanced had Gray, who saw it through the press, added a few pages stating what sections, in his opinion, needed most revision. I can imagine an innocent Indian finding this book, fascinated by its logical coherence, and believing that it was a more or less correct account of the subject up to 1956. The truth is that Lea's book is one, like "Mendel's Principles of Heredity", which should be in every genetical library, but which must not be taken as an infallible or even nearly up-to-date, source of facts.

J. B. S. HALDANE

GENETIC RESEARCH

BY ARNE MÜNTZING

A SURVEY OF METHODS AND MAIN RESULTS

L T Förlag, Stockholm, 1961, pp. 345, **n.p.**

Müntzing will be remembered as long as any systematic records of science in our age remain, as the first man to produce artificially a pre-existing species, namely *Galeopsis tetrahit*, from two others. Such a man has an original outlook on genetics, and any book written by him on this subject is bound to be worth reading. The book before us is not merely worth reading; it is very good. It is intended to "be read without any special previous knowledge of biology" and though I should hesitate to recommend it to some of my literary friends, they would find it more assimilable than any text known to me. It is also sufficiently different from any such text to make it a useful addition to any university library.

Inevitably the treatment of higher plants is fuller than that of animals, men, or microorganisms, and accounts of animal genetics are sometimes a little misleading. Sex determination by a single pair of genes is not confined to fish, as readers of p. 97 might think, and the first animal species in which Mendelian inheritance was demonstrated (by Bateson) was *Gallus gallus*, not as stated on p. 45. These are minor blemishes. Naturally the author chooses many examples from Scandinavian work. This is not a blemish at all. The world should know more of the work of such men as Åkarrman, Bonnier, Gustafsson, Håkansson, Mohr, Nilsson-Ehle, Rosenberg, Sjögren, and Westergaard.

The book was translated into English by its author, and the translation revised by others whom he cites. They could have done their job better. For example on p. 98 a queen bee is said to lay her eggs in "cells in the wax cake", and on p. 303 I read of "anthropocorous apes". I hope that the book will appear in a second English edition in which these and other errors will be corrected. I have to repeat the mono-

tonous complaint that larger numbers of such books as this would be sold in India, and doubtless in other relatively poor countries, were their price stated.

J. B. S. HALDANE

SOME ADVANCES IN HUMAN GENETICS

British Medical Bulletin, Vol. 17, Number 3, September 1961. Human Genetics. Medical Dept., British Council, 65 Davies St., London, pp. 87, 20s.

This volume contains fifteen articles, all of them good. They may be recommended as summarizing recent advances in a subject which, after some decades of relative stagnation, is now growing very quickly. The volume cannot be regarded as a text-book. It contains, for example, nothing about blood groups, cancer genetics, or the effects of inter-racial-crossing. Five articles deal with chromosomal abnormalities, and overlap to some extent, which the editor might perhaps have avoided. To non-medical geneticists the most interesting may be that by Lennox, which deals especially with colourblindness in XXY individuals. It seems highly probable that Nowakowski, Lenz, and Parada have discovered equational non-disjunction in two women heterozygous for colour blindness. I can see no advantage in ignoring the classical Drosophila work, and calling the process "non-disjunction at second meiosis" even if the mechanism in woman and Drosophila differs. The typography in this article is also inadequate. The 'bold-face' X's should be still darker.

Most of the work described is on more or less undesirable characters. The exceptions are Davidson's article on inherited variations in leucocytes only some of which are harmful, Harris' on plasma proteins, and Holt's on finger prints. The first of these contains a good deal which was novel to this reviewer.

The volume is evidently intended for physicians. It can however be read with advantage by animal, plant, and microbial geneticists. But if it is to profit them as fully as possible the editor should see that authors do not confuse linkage with pleiotropism, as at least one seems to have done. This and several other lapses by some, but not all, contributors, emphasize the danger that genetics may break up into a number of isolated disciplines. Editors and writers of textbooks have a special responsibility to prevent this. It would also be useful to the general reader if the preface explained why certain advances in a rapidly growing subject are not considered (e.g. the recent discoveries as to incompatibility at the ABO locus). Otherwise a somewhat false impression of a subject may well be given. I mention these facts because a number of the same bulletin on microbial genetics is due to appear in 1962, and it should be quite possible to avoid the need for both these criticisms of it. They are not perhaps very serious, and I have no doubt that geneticists of all sorts will find this number a useful summary of current work.

J. B. S. HALDANE

TU PRODUXISTI NOS INTER LUMINIS ORAS

THE AUTOBIOGRAPHY OF CHARLES DARWIN, 1809-1882, with original omissions restored. Edited with appendix and notes by his grand-daughter Nora Barlow, Collins, 1958, pp. 253, 16s.

It is a great pleasure to review the complete *Autobiography of Charles Darwin* edited by his grand-daughter. This publication is well timed to coincide with the centenary celebrations of the *Origin of Species*, which was, according to Darwin "no doubt the chief work of my life". Lady Barlow explains in the Preface the circumstances responsible for the suppression of parts of the *Autobiography* and some of the correspondence in the previous publications. The appendix contains two parts, one on Charles Darwin and his grandfather Dr. Erasmus Darwin, and the other on the Darwin-Butler controversy. The correspondence between Darwin and Samuel Butler is also now fully published. The appendix is followed by notes on various topics such as Mrs. Darwin's papers on religion, Charles Darwin's ill-health, and page and line references to previously omitted passages. The family tree of Charles Darwin is given on page 10, and it would have been better if Lady Barlow's relationship were also shown here. The foot-notes added on many pages by Lady Barlow are also very helpful.

Darwin's autobiography is of special interest from an Indian point of view. Charles Darwin was not a brilliant student at the University, but his latter work showed his outstanding scientific curiosity, and his power of reasoning based on carefully collected facts. He was very zealous in collecting insects and minerals, and at bird-watching since his boyhood. Darwin's researches are mainly non-violent, and a good deal of work can easily be done on Indian plants and animals along his lines. His work on the heterostyly of Primula and Lythrum, and on insect pollination, are examples of this kind. We see his generous and fair-natured mind in his attitude to Wallace's essay which contained the same theory as his own on the origin of species.

The most important reasons for his success, according to Darwin, are "—the love of science—unbounded patience in long reflecting over any subject—industry in observing and collecting facts—and a fair share of invention as well as of common-sense", and these are needed in Indian science more than anything else.

Nora Barlow has done a splendid job in editing this complete autobiography.

K. R. DRONAMRAJU

A TALE OF TWO PUMPS

I hope that most readers of this Journal are already familiar with the expurgated editions of Darwin's autobiography which have so far been published. They will want to know what more they get for their sixteen shillings. In the first place they get approximately 6000 words (over 30 for a penny) expunged from the autobiography in 1887. Some of these are irreligious. Their omission may well have led

readers to underestimate the logical and ethical coherence of Darwin's views on the existence of god and similar questions. Others are stories about Darwin's contemporaries, and his own judgements on them. My favourite is the tale of Charles Babbage and the pump. He had seen a pump by an Italian road-side, with an inscription that the owner had erected it for the love of God and his country, that the tired wayfarer might drink. Babbage found that most of the water pumped went into the owner's house, and added:

"There is one thing that I hate more than piety, and that is patriotism".

In 1845 Galton (Memories of my life, p. 105) found a similar pump near Jaffa. Most of the water pumped went to the owner's garden. Galton's comment is as follows. "It was an excellent example of the higher forms of commercial enterprise. They enrich all round, but the merchant to whose initiative they are due gets by far the biggest share." Darwin's comment on Babbage was "I believe his bark was much worse than his bite". But he preserved Babbage's value judgement for the benefit of posterity. It is noteworthy that both patriotism and piety have an important function in supporting "the higher forms of commercial enterprise". I think the difference between the two cousins in their reactions to the pumps throws a good deal of light on their other differences.

Among the notes is one on Darwin's ill-health. Alvarez puts it down "to an inherited peculiarity of the nervous system". Good, Hubble and Kemp agree that it was due to a repressed emotion of fear and hatred of his father. However, just too late for inclusion in this book, Adler was able not only to show that Darwin's symptoms agree with those of Chagas' disease, which is endemic in South America, but to identify the date on which he acquired the disease from the bite of a bug. However, I understand that Lady Barlow is not convinced by some of Adler's evidence. I trust that a new edition of the Autobiography will soon be called for, and the matter can there be discussed.

I regard the full edition of the autobiography as an essential in any biological library.

J. B. S. HALDANE

A PARADE OF IGNORANCE

DARWINISM AND THE STUDY OF SOCIETY

A centenary symposium edited by Michael Banton with an introduction by J. Bronowski, London, Tavistock Publications, Chicago, Quadrangle Books 1961, pp. xi+191, 21s.

This book is based on papers read at a conference at Edinburgh in 1959. It consists of twelve chapters, mostly, in my opinion, very bad, by four biologists, a statistician turned engineer, and seven sociologists or humanists. Presumably for these writers "Darwinism" means the Darwinian theory of evolution. Personally I think Darwin's books on plant physiology, and the attitude revealed by them, may be more original

than his theory of evolution, much of which had been developed independently by Wallace. But let us suppose that Darwinism means evolution by natural selection.

A good deal of work has been done on natural selection in man. Classical examples are that of Pearson and Beeton on the inheritance of longevity, of Fisher on the inheritance of fertility, of Karn and Penrose on selection for birth weight, and of Robson on its genetical determination. None of this work is noticed by any author. It is a sad fact that the nonsense written about the study of society in the name of Darwin has been of much more historical importance than the sense. None of the writers even mentioned the fact that natural selection, when observed, has generally (though not always) been found to be centripetal, that is to say individuals near to the median value for any character left more offspring, or at least survived longer, than those which diverged from it. This is not a new discovery. It was made on animals by Bumpus in 1898 and Weldon in 1901. Penrose showed that it also held for human intelligence as well as for birth weight.

The concentration of false statements is probably maximal in Bronowski's introduction, with a mode on page xiv. "What an animal species survives by in the end is not adaptation but adaptability". Presumably no species survives "in the end". But the vast majority of those alive today are insect species, highly adapted to some narrow environment, and incapable of surviving outside it. "The mammals are less spendthrift in this fertility than, say, the fishes or insects." The most successful mammals, both in numbers of individuals and of species, are the small rodents, which produce many young at a birth, whereas a tsetse fly only produces one. "Alexander the Great spread Greek culture to the ends of the known earth." He didn't. When his army got to the Beas they *knew* that King Xandrames (presumably Chandra Mesh, or Shining Moon) had a larger army in the Ganga basin, and refused to go on. "Alexander, Solomon, and Newton turned the evolution of man in new directions in a way in which no animal can change its evolution." The Galapagos Islands were very possibly colonized by the offspring of a single pair of finches whose descendants evolved into a group of new species. No doubt the founders did not intend this. Nor did Newton intend to produce an intellectual basis for atheism. "We have not begun to look for units of social change." Nirmal Kumar Bose (1929) has begun to look for them, and has found some. This is a high concentration of errors on one page. I only mention two others. On page xvi he writes "Yet the solution was not in sight when the *Origin of species* was published, even though Gregor Johann Mendel was about to begin, in an obscure corner of Europe remote from the flow of scientific debate, the experiments whose rediscovery after 1900 gave the key to the primeval problem." Brno, where Mendel worked, was within earshot of the guns fired in the obscure skirmish at Austerlitz, of which Bronowski may have read, and is not very far from Praha, where Purkinje founded the world's first physiological laboratory and Landsteiner later discovered some of the first members of group AB, including Hans Kalmus. More remarkably the solution was "in sight" when the sixth edition of the Origin was published. In Darwin's paper on heterostylism in the Proceedings of the Linnean Society for 1868, on work some of which had been done as

early as 1862, gives conclusive proof that short styles are dominant over long in *Primula sinensis* and *P. acaulis*, and purple over yellow petals in the latter. The results are more readily available in his book on "The different forms of flowers in the same species." This is not the place to discuss why Darwin did not draw the correct conclusions from his work. But the solution was quite literally in sight. On page xx we read that "For dead nature, time is a running-down movement towards more probable, that is, more unordered states". Look at a block of granite, Dr. Bronowski, in which a number of different compounds have been arranged in large, orderly, crystals. They may be more probable, but are also more ordered, than in a block of gneiss. And granite seems to be an end product.

Professor Willey is equally parochial. For him "the History of Thought" means the history of thought among well-to-do persons in Europe, North America, and the shores of the Mediterranean. He concludes that Darwin would turn in his grave if he knew that he would be saluted as an unwitting champion of Christianity. This may be so. But Willey is evidently unaware that Darwin did convert millions of Europeans to some of the essential tenets of Hinduism.

Professor Hogben's contribution is of course brilliant. "Given grain storage, then mice and rats, then the cat". he writes. But is it true ? Many ancient Greek cities lived largely on imported grain, but they used ferrets (or some other mustelids) when the Egyptians were using cats. And if his statement on p. 45 that "the mutation of chromosomes or of single genes is admittedly the pace-maker of evolution" is true, then I have wasted my life rather completely.

Werner Stark, who claims to write on Natural and Social Selection, in fact writes on Ammon, Lapouge, and Gumplowicz, all of whom produced egregious nonsense on human biology of which Stark writes that "This is good Darwinism'. This is in my opinion, not only a lie, but a damned lie. Given that centripetal selection is the usual pattern, it follows that "Blessed are the meek, for they shall inherit the earth" is better Darwinism than the opinions of Gobineau, Galton, Grant, Gates, Garrett, and Gayre. Stark takes refuge from these myths in the muddled idealism of page 56. All the authors cited flatter their readers. Lapouge's thought, if followed up, would lead to the conclusion that the finest specimens of humanity, not degraded by civilization, were to be found among tropical Africans. This may of course be true, and I suspect that, now there is a market for books flattering Africans, they will be written.

Waddington and Ginsberg are largely occupied with "social evolution", that is to say changes in the structure and function of human societies with time. It is a fact about European thought that the acceptance of Darwinian biological evolution made it easier for thinkers on human politics to accept the idea of orderly change in political systems. But I fully agree with Thomas Henry Huxley, quoted by Ginsberg on p. 102, when he said "That progressive modification of civilization which passes by the name of the 'evolution of society' is, in fact, a process of an essentially different character, both from that which brings about the evolution of species, in the state of nature, and from that which gives rise to the evolution of varieties, in the state of art". I personally

think it most unfortunate that the word "evolution" was used to describe social changes. I regard this choice as a piece of wishful thinking by well-to-do academic writers who had persuaded themselves that the history of the last few centuries in western Europe could be extrapolated indefinitely. It is at least possible to take the view held by such very different thinkers as Herbert Spencer and V. I. Lenin, that the state is a parasite on the community, and to hope that it will not evolve, but perish. For those who hold either this view, or several religious views, the sociological articles in this book are of the most dubious value. Their authors would have done better to explain why Darwin's contemporaries preferred to compare social change to evolution rather than to geological history or individual ontogeny.

The last article, by Banton, is nearly as good a museum of ignorance as the introduction. On p. 169 he refers to subjects "in which one cannot experiment like astronomy". Even before the first sputnik, radio waves had been sent to the moon, and their echoes detected. This is however trivial compared with his endorsement of Popper's statements about research. "Science cannot start with observations, or with the "collection of data". Before we can collect data, our interest in data of a certain kind must be aroused. The problem always comes first". Professor Popper has probably never made a scientific discovery. I have. Sometimes I have had a problem in mind. If so I have generally discovered something for which I was not looking: often I had no problem. But I prefer to cite the work of my colleagues. S. K. Roy (1959) had cut off the ears of a number of rice plants in the course of an experiment on interaction. He noticed that they regenerated, giving a second crop comparable with the first. K. R. Dronamraju (1960) was trying vainly to cross two varieties of *Lantana camara* when he noticed that they were being visited by different species of butterflies. H. Spurway (Spurway and Dronamraju, 1962) saw a wasp smearing mud on a table in our dining room, and watched it, with a little help, for 12 hours a day during 15 days. In each case the motivation for recording the facts was quite as much love and admiration as curiosity. The problems came after the observations. They were largely problems of statistical significance, partly of animal motivation (Dronamraju and Spurway, 1960). Banton goes on to say that "zoologists are interested in zoological problems." The best zoologists, in my opinion, are interested in animals, and would be happy to spend their lives watching them and playing with them. They study "problems" for three reasons. The first is given in the opening sentence of Herodotos' Histories, one of the most scientific books ever written. Another is that their curiosity as well as their admiration is aroused. A third is that they earn money by investigating "problems". If Banton will take the trouble to read Darwin's works he will find that Darwin was quite as much moved by admiration as by curiosity. For a deeper analysis of the motivation of research I recommend him to read Spurway (1960) which may give him hints towards an understanding of some aspects of his own subject of social anthropology.

Banton goes on to state that genetic factors are of no significance for the explanation of some "problems" of human behaviour, "such as the study of the mother's brother—sister's son relationship as it occurs in many tribal societies". They are significant.

If a man is colour-blind, the probability that any of his sister's sons will be colour-blind slightly exceeds one quarter, whereas his own sons are no more likely to be colourblind than anyone else. The same is true for other sex-linked characters. This fact may not be causally connected with the importance attached to the relationship, just as the production of recessives by inbreeding may not be relevant to its prohibition, but it is as shameful for a social anthropologist to ignore one possibility as the other.

Banton (p. 176) no more than Waddington (p. 76), or Smith (p. 86) appears to have heard of N. K. Bose's work on transmission of cultural items from one culture to another. He found that the same rules held for transmission from caste Hindus to Indian tribals, and from Europeans to caste Hindus. Bose may be wrong, but it is no more permissible to ignore his views than those of his namesake S. N. Bose after whom most of the particles in the universe are named.

The proof reading is as might be expected. The following are examples: "cryptograms" (for cryptogams), "genetypes", "genteic", "advance societies", "simulutors", "communcation", "perculiar".

As in Sodom, there are redeeming features. I found Shepperson's article on "The Intellectual Background of Charles Darwin's student years" most exciting, though hardly relevant to the subject of the book. It contains such important and generally unknown facts as that Charles and Erasmus Darwin borrowed more books from the University Library than any other students in the year 1825-26. This article is a fine bit of scholarship. Maynard Smith's Figure 3 is, I believe, incomplete. But it is a real contribution to the philosophy of biology. It is a pity that he did not know that Lorenz' "psycho-hydraulic" model of the brain was taken, without acknowledgement, from MacDougall (1923). Barnett's article on communication in animal and human societies is a reasonable follow-up of Darwin's "Expression of the emotions". But he underestimates the importance of learning the bee "language". Lindauer found that bees commonly signalled too long a distance after their first successful foraging expedition. Burns on "Social norms and social evolution", is often worth reading, though I do not know why he describes (p. 156) the officers of crack German regiments, and Japanese nobles, as "moral élites" rather than immoral élites. Both have led their countries to disaster in the past, and may do so in future.

I have perhaps devoted too much space to this book. But it has received many favourable reviews, which in my opinion, reflect the ignorance of its reviewers rather than the excellence of its contents.

REFERENCE

Bose, N. K. (1929). *Cultural Anthropology*. Revised edition (1961). Asia Publishing House, Bombay.

Dronamraju, K. R. (1960). Selective visits of butterflies to flowers, a possible factor in sympatric speciation. *Nature*, **186**, 178.

Dronamraju, K. R. and Spurway H. (1960). Constancy to horticultural varieties shown by butterflies and its ecological significance. *Journ. Bomb. Nat. Hist. Soc.*, **57**, 137-143.

Macdougall, W. (1923). An outline of psychology (pp. 107-109). London, Methuen.

Roy, S. K. (1959). Second flowering in *Oryza sativa*, (var. *indica*). *Nature*, **183**, 1410.

Spurway, H. (1960). Vaisnava and Saiva Biology. *Zool. Jahrbücher*, **88**, 107-116.

Spurway, H. and Dronamraju, K. R. (1962). The building of one nest of *Sceliphron madraspatanum*. (in Rend. IV Congresso unione internaz. stud. insette sociale, Pavia).

BUY THESE BOOKS

PAPERS ON BACTERIAL GENETICS

Selected by Edward A. Adelberg. Little, Brown, and Co., Boston, Toronto, 1960, pp. xlvi+400, **n.p.**

PAPERS ON BACTERIAL VIRUSES

Selected by Gunther S. Stent. Little, Brown, and Co., Boston, Toronto, 1960, pp. xxx+519, **n.p.**

I have been accused of being unduly critical in these columns, and also of anti-American bias. Here, at least, are two books of North American origin which I welcome without qualification. Both are anthologies, and very good ones. They do not contain some papers which I regard as fundamental, including some recent ones involving marking with radioactive isotopes. But this is not necessarily the fault of the selectors. An author may not permit republication of his work, or may insist that if paper A is published, it should be accompanied by paper B, which no one but he admires. Perhaps the work whose omission I most regret is that of Pollock on acquired penicillin resistance.

The book on bacterial genetics contains a few classics which have been reprinted elsewhere, such as Lederberg's on segregation in *E. coli* in Genetics for 1947, and is sufficiently up-to-date to include the first announcement of Monod's "operon" hypothesis. Most of the papers are on processes more or less peculiar to bacteria, and a student of population genetics in higher organisms may neglect them without much harm. So I am glad to see that Atwood, Schneider, and Ryan's paper on "Selective mechanisms in Bacteria" is included. No serious student of evolution can possibly neglect this. Clearly the process of periodic selection described by them can be of little importance in sexually reproducing organisms, but it may occur in clonally reproducing ones, including many higher plants, and possibly aphids.

Most of the papers here reprinted were published in the last ten years, and I regret to say that I had not read several of them. Although inevitably a good deal of biochemical, and some immunological knowledge is assumed, the emphasis is on genetics, and every geneticist should know enough biochemistry and immunology to understand them.

The volume on bacteriophage is rather different. It includes the papers in which Twort and d'Herelle announced its discovery. But the later work reprinted is almost entirely genetical. Little or nothing is included as to the practical use of phage in determinative bacteriology or its part (if it has one) in checking natural epidemics. Thus the book may well be a disappointment to pathologists who buy it on its title. It will not be a disappointment to geneticists.

The books are paper bound, and are not heavily glazed, a practice which adds to the weight of a book, and causes its rapid deterioration in a humid climate. Not only every genetical library, but every self-respecting university library, should have a

copy of each. But their price is nowhere stated. It is probably not very high. Yet its omission is likely to diminish the sales of the books in poorer countries. And it is precisely in poor countries, where the journals from which the papers are reprinted are rare, that such books are most needed. If publishers will not look after their own interests, the U. S. administration should urge them to state prices in the interests of its foreign policy. For such books are as good propaganda for the United States as cheap paperbound works of sadistic fiction are bad.

J. B. S. HALDANE

EDUCATING PHYSICIANS

CLINICAL ASPECTS OF GENETICS

Edited by F. Avery Jones. Pitman Medical Publishing Co., 1961, pp. xii+190, **n.p.**

Medical genetics has suddenly become very fashionable, largely, I suppose, because infectious diseases are now being controlled. This is the record of a conference held at the Royal College of Physicians in March 1961. Seventeen papers were read, all good. But they do not cover by any means the whole ground of human medical genetics. For example H. Harris' most interesting paper on Drug Sensitivity was entirely devoted to the genetics of sensitivity to one drug, "suxamethonium". A review of all drug sensitivities by a competent geneticist would be of the greatest interest.

Five papers on chromosomal abnormalities are already somewhat out of date. Some hypotheses have been confirmed, others perhaps rendered less probable. It will be several years before it is safe to summarize this fascinating field. Lehmann's summary of our knowledge of the abnormal haemoglobins in only twelve pages is a remarkable feat, but again new knowledge is accumulating so fast that it already needs some revision.

The papers which I personally found most interesting were those of Slater on mental diseases, Carter on "congenital abnormalities", meaning fairly gross morphological abnormalities, and Platt on neurological abnormalities. The first two are mainly concerned with rather common conditions such as schizophrenia and harelip, which do not show simple Mendelian inheritance. These are challenges to statisticians and geneticists. But I am not convinced that some of the authors cited have eliminated all possible biases. Platt's paper marks an epoch in human genetics as giving evidence for a gene which is now very rare, and should be widespread, in fact one where simple positive eugenics is possible. This is an apparently recessive gene causing indifference to pain with no other sensory loss. Anyone who will arrange for a person with this condition to act as a donor in artificial insemination will rank as "τέκτονα νωδυνίας", to quote Pindar's phrase, meaning contriver of analgesia, for future generations.

I am sorry that Professor Fraser Roberts' opening talk on the elementary principles of genetics was not included. Such talks are very necessary. The chairman of the opening session, Sir Robert Platt, the President of the Royal College of Physicians,

is unaware of these principles. Otherwise he would not have made the statement (p. x) "if it is entirely genetically determined there should be the same proportion amongst the siblings, amongst the parents, and amongst the children". This is the kind of nonsense which one expects to hear from a cabinet minister or a state governor. It is unfortunately not an isolated fatuity. Other medical men who took part in the discussion clearly knew very little genetics, but unlike Sir Robert, they knew that they knew very little. It is clearly necessary to educate the medical profession in genetics, but the inclusion of evidence for this need in a book of this kind will not increase its value to geneticists.

The book appears to be photostatted, which makes its reading rather difficult, and the failure to state its price will also detract from its sales outside Britain. Non-medical geneticists will find some matter to interest them, but the book is not sufficiently comprehensive to warrant its inclusion in all genetical libraries.

J. B. S. HALDANE

MONUMENTUM AERE PERENNIUS

Selected papers of A. H. Sturtevant, Genetics and Evolution. W. H. Freeman and Co., San Francisco and London, pp. iv+334, $ 7.50.

Thirtythree of Sturtevant's 137 scientific papers are reprinted on his seventieth birthday, with brief sketches of his life. On looking back on his own papers, a biologist is often liable to feel that he could have done the work very much better 20 years later. Because this is not so for Sturtevant's most important papers, they are classics, which any student of genetics may read with profit, and which were rightly reprinted.

The most important of all is the first, on the linear arrangement of six sex-linked factors in *Drosophila melanogaster*. Sturtevant not only discovered linear arrangement, but what was later called interference. Morgan was responsible for the hypothesis that linkage was due to linear arrangement. Unfortunately neither author referred to Correns (1902) who had suggested linear arrangement in a figure which he did not try to justify. Other classics are the discovery, first of "crossover reducers", and later, of the fact that they were inversions, and the mapping of the fourth chromosome. In mapping it he discovered what is now called negative interference. Double crossovers were far above expectation, the coincidences for three regions being 20·4, 35·2, and 2·8. Next year Spurway and Philip (1952) discovered and named it in *Drosophila subobscura*. Since then it has been repeatedly rediscovered, especially in fungi. None of the later authors appear to recognize the pioneer work here cited.

It is characteristic of the classics of science that the observations are so well recorded that the author's conclusions can be extended. Brahé's astronomical observations are the most celebrated case. Sturtevant's work is no exception. Let us turn to p. 35, Table IV, in a paper on sex recognition and sexual selection in Drosophila. If we

summarize the first 8 numbers relating to matings where a fly had a choice between a wild type and yellow mate, we find the following figures:

		chooser +	chooser *y*
chosen	+	85	37
	y	43	38

Here each "chooser" had a choice between wild type and yellow flies of the opposite sex. There was quite a significant tendency to homogamy, with $\chi_1^2 = 5{\cdot}75$. The figures for each sex alone indicate assortative mating, but neither is significant by itself. Nor of course is the assortative mating as intense as that which Rendel later found for the homologous gene in *Drosophila subobscura*. This example is not intended as a criticism of Sturtevant, but as a demonstration that his experiments were so simple, and are so clearly recorded, as to be worth reading in detail after 47 years.

I am sorry for one omission. I believe that Sturtevant is color-blind, but am not sure which allel he exhibits. I think that such facts should be put on record about great observers in all fields. The book can be recommended unreservedly for any genetical library. And students may be told "Even if you don't discover anything as important as Sturtevant did, at least try to express yourself as clearly."

J. B. S. HALDANE

REFERENCES

CORRENS, C. (1902). Modus und Zeitpunkt der Spaltung der Anlagen bei Bastarden der Erbsen-Typus. *Bot. Zeit.*, **60,** II, 65-82.

SPURWAY, H. AND PHILIP, U. (1952). Genetics and cytology of *Drosophila subobscura*. VII. Abnormal gene arrangements in element A. *J. Genet.*, **51,** 198-215.