several objections to this view, which to me hedges a mechanical concept essential to the understanding of genic transmission with unproven physiological restrictions ; but one of them must surely be decisive in itself. Heterozygotes do not show this same property in inbreeding species. Lerner seeks to meet this objection early in his book when he writes " . . . the hypothesis . . . is applicable only to populations with genetic systems based . . . on cross-fertilisation ". (p. 7). Inbreeders, apomicts and so on, do certainly differ from outbreeding species but hardly in the basic genetic principles upon which they depend. To argue to the contrary is to see rye resembling, for example, *Drosophila*, but differing in the basic properties of heterozygosity from wheat, with which it is so closely related as to give hybrids ; or to endow heterozygosity in the tomato, as we know it in this country, with properties unknown in its South-American ancestor and yet shared by *Galeopsis* and barley. Inbreeding and outbreeding are too closely interwoven in evolution to allow of this escape.

Yet object as we may to Lerner's final conclusions, we cannot fail to thank him for his book. It not only sets facts before us but it makes us think about them, and it does so with a style of scholarly presentation which is all too rare. *Genetic Homeostasis* may be criticised, but it will not be ignored. No geneticist should fail to read it : we all have much to obtain from Dr Lerner. KENNETH MATHER.

EVOLUTION AS A PROCESS. Ed. Julian Huxley, A. C. Hardy, and E. B. Ford. *Contributors* : G. R. de Beer, E. J. H. Corner, H. B. Cott, James Fisher, Sir Ronald Fisher, E. B. Ford, J. B. S. Haldane, A. C. Hardy, Julian Huxley, David Lack, Ernst Mayr, Bernhard Rensch, P. M. Sheppard, H. N. Southern, N. Tinbergen, T. S. Westoll, E. N. Willmer, J. Z. Young and S. Zuckerman. London : George Allen and Unwin. Pp. 367. 25s.

At some stage in the preparation of this volume, its contributors hit upon the happy idea of turning it into an unpremeditated *Festschrift* in honour of its senior editor, Dr J. S. Huxley ; and if all else in the book were controversial, the truth of its foreword, which speaks of Huxley's own splendid contributions to biology and the contributions which he has caused others to make, is surely not to be denied.

Huxley's own article, like Fisher's later on, deals with certain popular misconceptions about the content and import of the genetical theory of evolution by selection. Misconceptions there must be, in a world in which we cannot all be geneticists ; but why are they so distressingly popular? How did it get about, for example, that evolution is the outcome of the blind selection of chance mutations (for that is what Darwinism is popularly supposed to be)? Perhaps geneticists themselves are partly to blame. Huxley writes :

> " Natural selection has certain obvious limitations. It can only produce results which are of immediate biological utility to the species ; and being blind and automatic, it is incapable of purposeful design or foresighted planning."

I should be sorry to be obliged to defend this view against the censure of a determined critic. What can " blind " mean except undiscriminating and undirective, exactly that which selection is not? And why should its results be confined to those which are of immediate biological utility to the species? Even micro-organisms could be said to enjoy the possession of a genetical system of high mutability—or anyhow of great evolutionary versatility—which will protect them just as effectively from antibiotics which have yet to be discovered as from those which they have coped with hitherto. And is not the evolution of a certain kind of nervous or antibody-forming system a safeguard against emergencies yet to happen as well as against those which prevail to-day? Amidst the minutiæ of specific *ad hoc* adaptations we must not lose sight of the evolution of genetic and physiological systems: natural selection is not such an improvident, hand-to-mouth affair.

One of the most popular misconceptions about evolution by natural selection is that which treats it as the *dénouement* of the following train of thought: (*a*) organisms produce offspring in numbers vastly in excess of their needs; (*b*) not all these offspring survive; therefore (*c*) only those survive which are the best equipped to do so, the "fittest". The catch in this plausible little syllogism (pointed out years ago by Fisher) lies in its major premise (*a*). So far from producing a vastly excessive number of offspring, most organisms produce the number approximately most apt to perpetuate their kind. Degree of fecundity is one of the consequences of natural selection: it is not its cause. Nidicolous birds, Lack tells us in his article, illustrate this truth with particular clarity, for they do in fact lay clutches of a certain size, although they could lay more eggs—the egg industry preys upon the inexhaustible gullibility of the domestic fowl—and could, of course, lay less. Having regard to all the exigencies of giving birth to and rearing eggs and young, the size of its clutch is just about that which gives each species its greatest likelihood of self-perpetuation. Lack's article in this book must stand as representative of the half-dozen odd that deal in part or in whole with birds, but it will not escape notice that the two other co-Fairy Godmothers of ornithology also appear in person. Mayr's article is of the distinction which, from him, we have come to take for granted; and it is most satisfactory, too, that Tinbergen should have made such convincing progress in the interpretation of a subject that was one of Huxley's earliest interests: the significance of courtship display in birds.

On the principle that those incompetent to criticise are impertinent to commend, I shall say nothing of the articles written by professional geneticists. It is more to the point to ask what geneticists themselves can learn from the remainder.

Hardy does well to remind us of the very profound contribution that Garstang made to our understanding of the modes of evolution: the idea that organisms may slough away the latter ends of their life-histories and build their lineages and adult lives anew upon larval or even embryonic forms. Indeed, I believe that neoteny, so far from being a curiosity, is an entirely fundamental tactic of the evolutionary process. "There is an impressive list", writes Hardy, "of animal groups which may have been evolved in this way: Siphonophora, Ctenophora, Cladocera, Copepoda, Insecta, the Chordata as a whole, and (within the Chordata) the Larvacea, Enteropneusta, Cephalochordata (*Amphioxus*), some lampreys, the recent Dipnoi, the Urodeles, the Monotremata, and Man himself." Molluscs are added, and I am surprised to see no mention of Nematodes and Rotifers, which have many of the stigmata of pædomorphic forms. These several groups are of very unequal systematic standing, and very unequal too is the evidence which might entitle one to regard them as pædomorphic forms. Garstang's case was first built upon, and most convincingly argued for, the derivation of true chordates from animals akin to sea-squirts, and I believe that no well-informed zoologist now challenges the correctness of his view. I am therefore particularly sorry to see that Hardy should have made so much of Garstang's rash guess that *Amphioxus* (via an ammocoete larva) was a pædomorphic derivative of a lamprey-like or Cephalaspid form. The reasons given in favour of such a derivation are insubstantial ; so gravely damaging an objection as the lack of neural-crest tissue and its derivatives in *Amphioxus* receives no mention at all. Needless to say, most of the affinities that were at one time considered good evidence of recapitulation can now be inverted and treated as evidence of pædomorphic transformation : Haeckel is still the hero, though his portrait now hangs upside down ; but neither way up can the argument carry the slightest conviction unless it is backed by that thorough analysis of early embryonic development for which one looks in vain.

I fear also that I cannot share de Beer's enthusiasm for the evolutionary doctrines of Dr Jovan Hadzi, as they are expounded here. It is just possible to see the sense of a derivation of Ctenophores by neoteny from Polyclad Turbellaria ; but the evolution of Anthozoa from Turbellaria, and of Turbellaria themselves from ciliate protozoans, indicates nothing more than that evolutionary speculators set themselves somewhat lower standards of scientific rigour than prevail elsewhere. There is indeed nothing that *proves* these derivations false ; the trouble is that I can see not the faintest reason for supposing them to be true. According to de Beer, Hadzi makes a clean sweep of what might be called the " Coelenterate " theory.

> " Of all the views currently and generally held in Zoology, hallowed by a long period of acceptance and teaching it would be hard to find one more firmly established than that which holds that the most primitive Metazoa, the animals which evolved from the Protozoa and stand on the main line of evolution towards the higher Metazoa, are the Coelenterates."

I do not agree. Zoology would be in a sorry plight if the Coelenterate Theory of the origin of Metazoa were indeed one of its chief theoretical ornaments. The theory is not by any means a hallowed doctrine. It is a desperate guess made by those who have felt obliged to express some opinion on the matter. Others, more fortunate, remain silent, knowing that there is very little that can be usefully said on the matter at all.

It would be a salutary thing if no-one were allowed to indulge in phylogenetic speculation until a prolonged course of experimental studies had shown him how merciless facts can be to even the most plausible and passionately held hypothesis. Without that discipline, there is always a danger that the unravelling of phylogenies can degenerate into a kind of scholarly indoor pastime. If the energy at present dissipated in uncalled-for and unverifiable musings on the affinities of animals were directed towards seeking new methods of analysis, no-one would complain, however unsuccessful they might be ; and although biochemical interpretations of phylogeny have so far led almost nowhere, it would reveal poor judgment to suppose that a scrupulous analysis of protein and nucleoprotein structure and behaviour might not one day provide us with the clues we need.

It is pleasant therefore to turn to the present volume's principal scholarly document, Zuckerman on the interpretation of the descent of man, and in particular of the standing of the Australopithecines. Zuckerman's article will be read with pleasure by all those who, like myself, have contemplated with incredulous disgust the reasoning which purports, for example, to have revealed the intimate domestic habits of the Australopithecines. What Zuckerman attempts to do (admittedly on a tiny scale, compared with the grand affinities that have so far been in question) is to *reason out* judgments that have hitherto been entrusted to the act of intuition. The trouble with intuition, and with informed judgments based upon a lifetime's experience, is that these secret inner voices speak to their several owners in several different tongues ; so that, paradoxically enough, intuition reveals no truth more clearly than that the intuition of others is at fault.

The contributions that have been commented upon above must serve as a sample of the book's contents, but one more must be mentioned because it is of a very different kind : Young on memory, heredity and information. The terminology of information theory has caught on in many departments of biology, a good sign that it lives up to its subject matter by having something informative to say. Its principal virtue is that it has revealed an abstract general similarity of procedure in a wide variety of biological activities. The similarities, once they have been pointed out, are obvious ; once expounded, most good ideas are so. Thus to conceive of the hereditary process as a handing on of genetic information is, in my opinion, telling and to the point. Fastidious biologists do not speak of the " inheritance of characters " but may get involved in tedious circumlocutions in attempting always to speak of inherited character-differences. These terminological irritations can to some extent be mollified by speaking of the inheritance of genetic information ; and this has the added advantage that we may now easily conceive of the distinction between a secular change in the nature of the information that is transmitted and in the nature of the system of transmission itself. There are still some conceptual crudities in the biological applications of information theory. By what trick of topsy-turvy reasoning, for example, did it ever come to be suggested that the brain was a kind of (" endosomatic ") calculating machine, when the simple biological truth is that the calculating machine is a sort of exosomatic brain ? It performs brain-like functions, much as telescopes have eye-like and forceps hand-like functions, and motor cars the functions endosomatically performed by limbs. We *may* learn something about the brain by studying calculating machines, just as we have quite unmistakably learned something about the eye by studying glass lenses ; but it need not be so ; limbs do not work at all like internal combustion engines. Young and others are concerned to find out just how much can be learned of the working of the brain by using the concepts and practical findings of communications engineering. There is no *a priori* right or wrong about such an endeavour ; we can only judge it by how successfully, after trial, it proves to work.

P. B. MEDAWAR.