action-specific production of endogenous stimulation whose quantity is quite exactly calculated to meet the demand which is to be expected in the organism's normal environment"? What kind of explanation does Lorenz intend this circular proposition to convey, and where does it differ logically from the usual concept of instinct? As Skinner¹ has recently pointed out, it is equally misleading to assume that one can safely infer the "phylogenetic contingencies" by which a given pattern of behaviour may have been transformed over thousands or even millions of years; or because the behaviour pattern appears "adaptive", decide what kind of evolutionary process was responsible for it. In so far as it conveys this impression Lorenz's book is, as Skinner implies, simply misleading. How, for example, can Lorenz blandly write that it is quite "certain" that the Indian pipe of peace "developed into a fixed ritual which became law for every Indian and prohibited aggression after pipe-smoking"—on the basis of a speculation that one of two angry Indian chiefs may once have forgotten his pipe, and was allowed a puff by his enemy in order to enjoy the "tension relieving catharsis of smoking"? On what foundations of historical knowledge does this "certainty" rest? Speculations such as these—and Lorenz indulges in many—may be fascinating, but they are not science. The complexities of human social and political behaviour cannot be explained on the basis of oversimplified homologies and analogies with highly selective aspects of animal behaviour.

Lorenz is the acknowledged father of what is now called ethology. If this is to become more of a science than it sometimes appears, its disciples, while acknowledging their debt to him, would be wise to be more rigorous than he usually is in showing where observation gives way to *a priori* interpretation, and how casual is differentiated from controlled observation. This, it is true, may lead to duller writing than that which we get from Konrad Lorenz himself. But in the end ethology will benefit less from his assurance, the assurance which allows him to write that he is "being far from presumptuous" in professing the conviction that "in the very near future not only scientists but the majority of tolerably intelligent people" will treat his views as "obvious and banal truth", than it will from "scientific humility", whose virtues one vainly expects Lorenz to extol in the chapter which he heads "On the Virtue of Scientific Humility". Ethology is hardly likely to prove an exception to that major lesson of science which warns that the beliefs of today are not necessarily those of tomorrow.

S. ZUCKERMAN

¹ Skinner, B. F., *Science*, **153**, 1205 (1966).

LEARNING, INSTINCT AND EVOLUTION

Evolution and Modification of Behavior

By Konrad Lorenz. Pp. 121. (London: Methuen and Co., Ltd., 1966.) 25s. net.

"WHAT is preformed in the genome and inherited by the individual is not any 'character', such as we can see and describe in a living organism, but a limited range of possible forms in which an identical genetic blueprint can find its expression in phylogeny."

This opening sentence displays many of the troubles which beset Professor Lorenz's book. The assertion is a simple one, and no one is likely to want to contradict it: the genetic code for a given phenotypic detail does not embody a rigid and inflexible representation, but a set of instructions for development; the final developmental outcome depends on the manner in which these commands are carried out; the commands themselves may interact with other developmental commands, or with information from the environment; the phenotypic expression of such instructions, therefore, may be variable. Thus, it is

incorrect to say that the character itself is preformed and inherited.

The trouble with Professor Lorenz's formulation of all this is twofold: in genetic terms, it is trite; as conceptual analysis, it is insufficiently sophisticated. We are told that it is not any "character" which is inherited, but "a limited range of possible forms". In fact, of course, it is neither; what is inherited, within this frame of reference, is the genetic blueprint. Throughout the text, attempts at precision are constantly undermined by semantic confusions of this kind.

The book has two objects. The first, which is valuable, is to emphasize the role of inborn behaviour, and of species-specific patterns, to the experimental psychologist who is not familiar at first hand with comparative studies. The second concerns the old, sad matter of definitions. Professor Lorenz wishes to give force and precision to the concept of inborn behaviour as distinct from behaviour which is learned. He gives some telling examples of complex behaviour patterns which spring, fully armed, from the central nervous system on the first occasion when they are elicited by an appropriate context. He describes behaviour which must be learned in the course of the lifetime of the individual, and makes the important point that such learning requires not only that the environment should, in nature, present the features necessary for it to take place, but also that there must be some phylogenetic programming of the nervous system which will enable the environment to be so exploited.

In between these two extremes, there is a grey no-man's-land of patterns which appear to combine both inborn and learned mechanisms. Here, above the fog of semantic distress, two flags appear to be hoisted: (a) a given case can be analysed experimentally, and the relative contribution of the two processes assessed, but the very nature of the deprivation experiments which must be used invites ambiguity; (b) surely, we must be able to arrive at some powerful generalization about inborn behaviour which will cover the multitude of special cases?

No one, I imagine, will query the first proposition. There is a short answer to the second—why bother? The problem is scarcely different, in principle, from that posed by the interaction between genetic instructions, forces imposed by the environment, and stresses provided by the intrinsic musculature in the ontogeny of the adult skeleton. But the ontogeny of the skeleton suggests experimental problems private to itself. Philosophically, it is of trivial interest.

This essay, then, will be of great concern to American psychologists and others who are unfamiliar with biological disciplines; it will, at least, draw their attention to the very important contribution of Professor Lorenz's school of comparative ethology. For British ethologists, however, it will not achieve any great clarification of their present concepts. The battle, for the younger generation, has long since been lost and won, but Old Kasper still counts the bodies with gloomy astonishment. A. D. BLEST

SCIENCE AND PEOPLE

Science and Man

By Lord Brain. Pp. 109. (London: Faber and Faber, Ltd., 1966.) 18s. net.

The Identity of Man

By J. Bronowski. Pp. xi+107. (London: Heinemann Educational Books, Ltd., 1966.) 15s. net.

EACH of these books is a series of four essays, which were first given as public lectures. Both are primarily concerned with man as a natural phenomenon, and both authors were very impressed with new ideas about mechanisms in the retina for the analysis of images. Both, too, are successors to previous books of essays. The treatment, however, is very different.