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Evolutionary Physiology.

THE presidential address to Section I (Physiology) delivered by Prof. Leathes at the recent meeting of the British Association in Oxford was so remarkable in its width of view that it may be said to constitute a landmark amongst such addresses. In the past the main objects of physiological research have seemed to be two: first, to investigate the chemical nature of the substances which enter into living matter, or perhaps it would be better to say recently killed matter, and secondly, to invent imaginary machines which in their working would resemble the functions of living beings. But even if the second object were completely attained (and in no single case has this been done) it would still leave unanswered, as Sir Charles Sherrington has pointed out, two fundamental questions—namely, first, how such machines are built up out of the formless protoplasm of the egg, and secondly, how mind inserts itself in matter. Leaving the second question aside as too profound to discuss even in the pages of NATURE, we may note that Prof. Leathes boldly grapples with the first: he discusses the nature of life itself, as well as the chemical nature of the substances with which it deals.

The first and most obvious explanation of the phenomena of life which suggests itself is that these peculiarities are due to the chemical nature of the compounds which enter into protoplasm. These Prof. Leathes classifies as proteins, nucleic acid, lipoids (compounds of the higher fatty acids), and sterrols. The proteins have been shown to be long chains of as many as 100 links, each link consisting of an amino-acid. As all chemists are aware, these acids have, as their name implies, a basic as well as an acid 'hand'; and they owe their ability to form chains to the fact that each with its basic hand can grasp the acid hand of its neighbour. But, as Prof. Leathes tells us, there are only about twenty amino-acids known, and the infinite variety of proteins must be due to the order and number of the links which are put together. In this order lies one of the fundamental secrets of life, namely, that of assimilation. Modern research on digestion seems to show that the organism does not incorporate ready-made blocks of protein into its structure, but first of all breaks up these blocks into their constituent amino-acids and then mysteriously reassembles them in its own proper order. The analogy of a crystal segregating from its mother-liquor will not help: in this case we have to do with a rigid framework of similar atoms, bound together in a solid, and life is only manifested in a fluid medium. In a crystal, moreover, the particles which are added to the crystal face are similar to those already constituting it and previously exist as such in the liquid, but in the formation of protein chains a varied assortment of amino-acids is added according to a definite scheme. To build up the chain by the elimination of the elements of a molecule of water as each link is fastened to the next requires the expenditure of energy. This energy is supplied either by the radiant energy of the sun or by the oxidation of some other compound.

The mystery of life is not the creation of energy; it is essentially, as Prof. Driesch put it in a forceful address delivered this year, in London, the 'control' of energy—the control which out of a chaotic assemblage of materials whirling about in the fluid builds up a definite specific structure. The word 'regulation' coined by Driesch about 1895 to denote the mode of activity of his 'entelechy' and abhorred by the mechanistic school of which the late Prof. Loeb was the principal ornament and exponent, is creeping back into the vocabulary of experimental embryologists like Spemann and von Uebisch because the facts with which they deal will allow of no other explanation. The number of possible permutations of the order of the links in these protein chains is almost infinite, so that, as we have seen, the structure of the unfinished chain discloses no necessity of giving rise to a completed chain of a particular kind. Prof. Leathes speculates as to whether an occasional 'permutation' in this order may not be the cause of what biologists term a mutation. It is greatly to be desired that this word 'mutation' should either be rigidly defined or else removed from the scientific vocabulary altogether. Sometimes it is used to denote any change whatever in the hereditary potentialities of an organism, and then to say that evolution is explained by occurrence of 'mutations' is merely a truism. But the fact that this is a truism is often illegitimately employed to support another theory, namely, that violent and sudden divergences from type such as Prof. Morgan encounters in his cultures of Drosophila, and which form the 'sports' familiar to every breeder and gardener, constitute the raw material of evolution.

All the evidence at our disposal is hostile to such a suggestion: these 'mutations,' though they are certainly hereditary, are all characterised by weakened vitality and are unable to hold their own in the struggle for existence. This fact has been ably expounded in the address to Section D given by Prof. Osborn and printed in NATURE of August 21, who, in order to emphasise the view that the changes by which animals evolve are widely different from mutations, introduces the word 'speciation.' Further, as Prof. Leathes wisely says, a mutation is not eternal. This, indeed, is its most interesting characteristic; the sport transferred to a natural environment, where, in the absence of dangerous competitors, it has a chance to survive, will after a certain number of generations revert to type; like the feral pigs of Jamaica, which have gone back to the wild boar. We submit that the cause of a mutation is to be sought in the weakening or inhibition of one of the processes which make up the activity of the germ plasm, a weakening which under better conditions is transmitted to succeeding generations in ever-lessening degree and eventually disappears.

Prof. Leathes eloquently discourses on the relation of function to structure and rightly states that physiologists who study function are well fitted to make contributions to the doctrine of evolution. What is 'functional' survives, and structure is the expression of function. This is shown by the fact that the same type of cell, the fibroblast, or, as embryologists term it, the mesenchyme cell, will develop connective tissue fibres and give rise to tendon at the end of a muscle, and will form and deposit calcium phosphate where bone is required. Every particle of bone, he asserts, is the response of the organism to the strains which the exercise of its members brings about. These products of cell-secretion he terms irreversible, as opposed to the reversible changes which take place in muscle.

The difference, however, is after all only one of degree. Cary has shown that muscular fibrils are developed in mesenchyme cells under the influence of strain. In the embryo pig the gut-tube grows faster than its mesenchyme envelope, and the development of these cells into smooth muscles takes place in accordance with the elastic tension to which they are exposed, and, on the other hand, are not the osteoclasts which remove superfluous bone essentially identical with the osteoblasts which deposit it where it is required?

The fact is that the early exponents of evolution, being naturalists and morphologists, had only a superficial knowledge of function, and any structure the meaning of which was not obvious was ascribed to 'chance' variations which 'happened' to suit the environment. An example of this method of reasoning was the explanation of the dark pigmented spots found on the forehead of certain carnivora as devices intended to deceive their enemies into the delusion that the animal whilst asleep was really awake and gazing at them. As our knowledge of comparative physiology has progressed it has become more and more obvious that the whole body of the animal is an expression of its functions; or, as a zoologist would phrase it, of its reactions to its environment.

That the functions of cells are reactions to their environment was demonstrated in a beautiful way by Nageotte. He took a piece of sterilised bone from a rabbit's digit and implanted it in the cartilage of the ear. The introduced bone was soon surrounded by 'fibroblasts' drawn from the neighbouring fibro-cartilage and connective tissue. These invaded it and actually deposited new bone around it, although in the normal course of affairs these cells never would form bone.

Prof. Leathes also discusses 'the conditioned reflex.' This is most simply described as a new association of ideas, though Prof. Leathes interprets it as the establishment of a new machinery in the nervous system. The classical example is, of course, Pavlov's wonderful work on the dog, in which the animal was made to associate the sound of a bell with the arrival of food. As a consequence copious secretion of saliva was produced by the sound of the bell. Koffka in his book "The Growth of the Mind" has shown how powerless is the conception of fixed reflex arcs when examined in detail to explain the formation of the new associations. Leaving this special difficulty on one side, however, Prof. Leathes truly remarks that the establishment of new functional relations is only of importance in evolution if this rise of new functions—in a word, the acquisition of new habits in the parent—affects the offspring so that the establishment of the same functional relations in them is effected more and more easily as the generations succeed one another. As most people are aware, this is at once the most fundamental and at the same time the most hotly disputed question in biology. Pavlov has asserted that he has demonstrated, by his experiments on mice, that conditioned reflexes in parents do affect the children. These results have been received by the supporters of 'chance variations' with the same incredulity with which they have received other similar results obtained by investigators of less world-wide fame than Pavlov. Prof. Leathes, as an impartial outsider, whilst awaiting confirmation by Pavlov himself of his own preliminary work, seeks to conciliate the more violent opponents by the use of the phrase 'parallel induction.'

By this phrase is meant the theory that whilst the body is incapable of affecting the germ cells which are embedded in it, yet an external influence may at one and the same time affect the body so as to provoke a new reaction and thus initiate a new structural change, and also affect the germ cells so that the next generation will show the same structural change. Surely this theory may aptly be described as the last ditch in which the opponents of the inheritability of acquired habits are prepared to die. Can it be seriously maintained that external changes in light and temperature can penetrate the somatic tissues and alter the deeply-seated germ cells, and yet that the body, which is in close physiological relation to these cells, is powerless to affect them?

Prof. Leathes makes a striking reference to the coincidence of the rediscovery of Mendel's laws of segregation in the hybrid offspring of different breeds, and the cytological discovery of the coming together of paternal and maternal chromosomes in the maturation of the germ cells and their subsequent disjunction into different cells. This 'meiosis' has been widely accepted as the physical basis of this segregation, and it is an hypothesis of seductive simplicity to take this view. But the whole history of physiology ought to warn Prof. Leathes of the peril of accepting simple mechanical explanations such as these. Again and again physiologists have believed themselves to be on the verge of simple physical explanations of vital functions, such as the diffusion of water from the blood through the glomerulus of the kidney, or the passage of oxygen through the alveolus of the lung, and each time closer examination has proved how disappointing and illusory such explanations are. In that wonderful school of cytological and Mendelian research established by the late Dr. Bateson in the John Innes Horticultural Research Institution, a body of devoted students have been studying Mendelian problems for years, and Miss Sverdrup's discovery that there are nine sets of independently segregating characters in the pea, but only seven chromosomes, is a result of just the same kind as physiologists have obtained in other fields.

In conclusion we may say that Prof. Leathes' eloquent appeal to his fellow physiologists to study functional evolution has our warmest sympathy. We feel convinced that if they respond to this appeal the whole aspect of evolutionary philosophy will be enormously changed and improved.

The Assaying of Brabantius.

The Assaying of Brabantius and other Verse. By C. S. Sherrington. Pp. iv+67. (London: Oxford University Press, 1925.) 4*s*. 6*d*. net.

THIS book of poems should interest all lovers of literature, not merely because of its author's eminence in the scientific world, but also for its own artistic quality. It contains the most accomplished verse that has been published in England by any man of science; and one of the most remarkable facts about it is that the point of view throughout is purely artistic. The poem on Keats, for example, is a poem of joy in the artistic handling of words. It shows them "in music swayed attire," shadows moved by the fire of thought. It shows them as "raised trumpets blown at morn," or as "foamed sea-capes calling through mist." It does not talk philology, but it finds them "still across this day of ours weaving fancy's storied woof."

The philistine who prides himself on his superiority