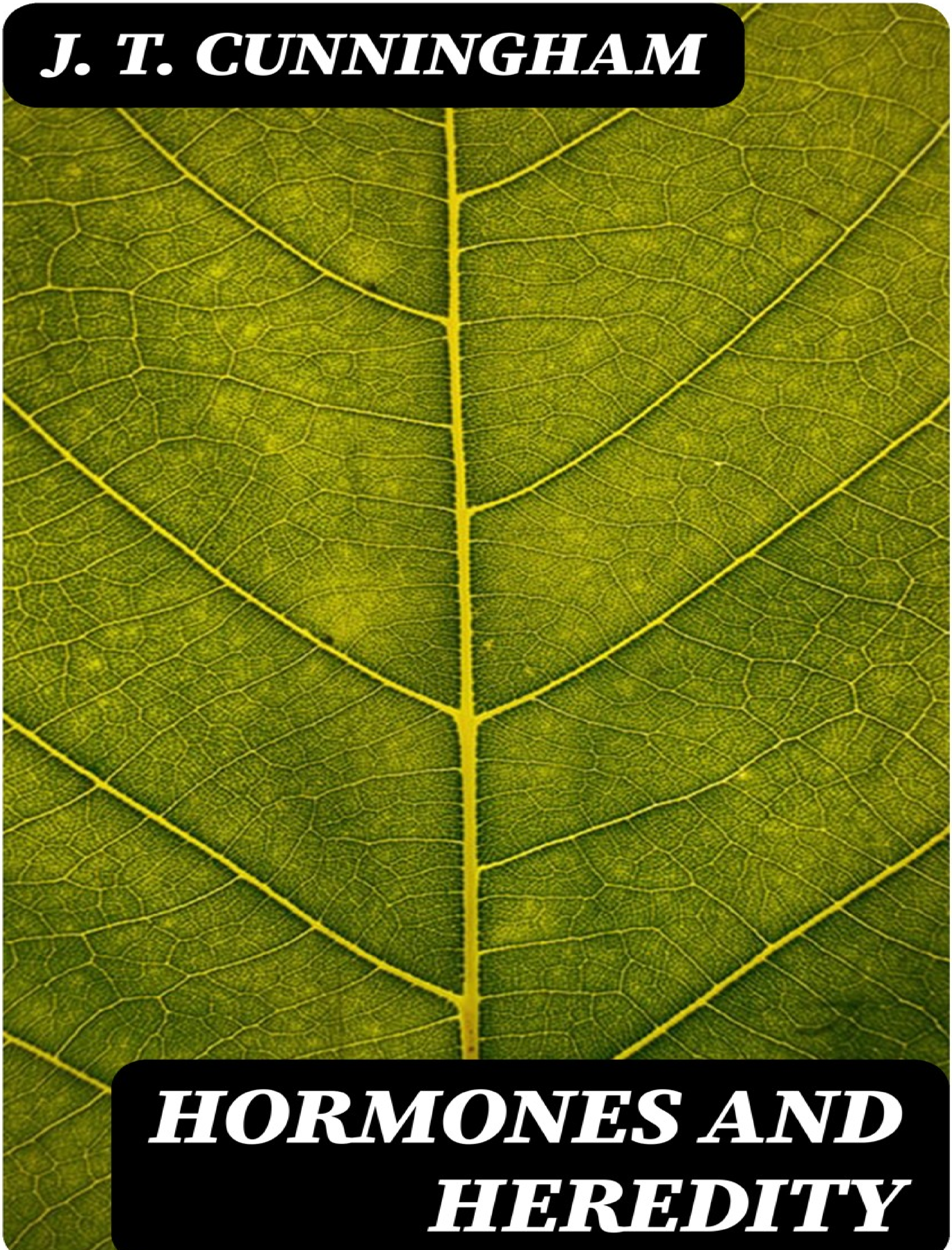
J. T. CUNNINGHAM



HORMONES AND HEREDITY

J. T. Cunningham

Hormones and Heredity

A Discussion of the Evolution of Adaptations and the Evolution of Species

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PREFACE

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My chief object in writing this volume was to discuss the relations of modern discoveries concerning hormones or internal secretions to the question of the evolution of adaptations, and on the other hand to the results of recent investigations of Mendelian heredity and mutations. I have frequently found, from verbal or written references to my opinions, that the evidence on these questions and my own conclusions from that evidence were either imperfectly known or misunderstood. This is not surprising in view of the fact that hitherto my only publications on the hormone theory have been a paper in a German periodical and a chapter in an elementary text-book. The present publication is by no means a thorough or complete exposition of the subject, it is merely an attempt to state the fundamental facts and conclusions, the importance of which it seems to me are not generally appreciated by biologists.

I have reviewed some of the chief of the recent discoveries concerning mutations, Mendelism, chromosomes, etc., but have not thought it necessary to repeat the illustrations which are contained in many of the volumes to which I have referred. I have made some Mendelian experiments myself, not always with results in agreement with the strict Mendelian doctrine, so that I am not venturing to criticise without experience. I have not hesitated to reprint the figure, published many years ago, of a Flounder showing the production of pigment under the influence of light, because I thought it was desirable that the reader should have before him this figure and those of an example of mutation in the Turbot for comparison when following the argument concerning mutation and recapitulation.

I take this opportunity of expressing my thanks to the Councils of the Royal Society and the Zoological Society for permission to reproduce the figures in the Plates. I also desire to thank Professor Dendy, F.R.S., of King's College for his sympathetic interest in the publication of the book, and Messrs. Constable and Co. for the care they have taken in its production.

J. T. CUNNINGHAM.

London, *June* 1921.

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INTRODUCTION

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Historical Survey Of Theories Or Suggestions Of Chemical Influence In Heredity

Weismann, strongly as he denied the possibility of the transmission of somatic modifications, admitted the possibility or even the fact of the simultaneous modification of soma and germ by external conditions such as temperature. Yves Delage [Footnote: Yves Delage, *L'Hérédité* (Paris, 1895), pp. 806-812.] in 1895, in discussing this question, pointed out how changes affecting the soma would produce an effect on the ovum (and presumably in a similar way on the sperm). He writes:—

'Ce qui empêche l'oeuf de recevoir la modification reversible c'est qu'étant constitué autrement que les cellules différenciées de l'organisme il est influencé autrement qu'elles par les mêmes causes perturbatrices. Mais est-il impossible que malgré la différence de constitution physico-chimiques il soit influencé de la même façon?'

The author's meaning would probably have been better expressed if he had written 'ce qui paraît empêcher.' By 'modification reversible' he means a change in the ovum which will produce in the next generation a somatic modification similar to that by which it was produced. It seems natural to think of the influence of the ovum on the body and of the body on the ovum as of similar kind but in opposite directions, but it must be remembered always that the development of the body from the ovum Is not an influence at all but a direct conversion by cell-division and differentiation of the ovum into the body.

Delage argues that if the egg contains the substances characteristic of certain categories of cells of the organism it ought to be affected at the same time as those cells and by the same agents. He thinks that the egg only contains the substances or the arrangements characteristic of certain general functions (nervous, muscular, perhaps glandular of divers kinds) but without attribution to localised organs. In his view there is no representation of parts or of functions in the ovum, but a simple qualitative conformity of constitution between the egg and the categories of cells which in the body are charged with the accomplishment of the principal functions. Thus mutilations of organs formed of tissues occurring also elsewhere in the body cannot be hereditary, but if the organ affected contains the whole of a certain kind of tissue such as liver, spleen, kidney, then the blood undergoes a qualitative modification which reacts on the constitution of the egg.

Suppose the internal secretion of a gland (*e.g.* glucose for the liver, glycolytic for the ferment for the pancreas) is the physiological excitant for the gland. If the gland is removed in whole or in part the proportion of its internal secretion in the blood will be diminished. Then the gland, if the suppression is partial, will undergo a new diminution of activity But in, the egg the specific substance of the gland will also be less stimulated, and in the next generation a diminution of the gland may result. Thus Delage states Massin found that partial removal of the liver in rabbits had an inherited effect. In the case of excretory glands the contrary will be the case, for their removal causes increase in the blood of the exciting urea and uric acid.

The effects of disuse are similar to those of mutilations and of use vice versa. Delage, as seen above, does not consider that increase or decrease of particular muscles can be inherited, but only the muscular system in general. If, however, in consequence of the disuse of a group of muscles there was a general diminution of the inherited muscular system, the special group would remain diminished while the rest were developed by use in the individual: there would thus be a heredity produced indirectly. With regard to general conditions of life, Delage states that there are only two of which we know anything—namely, climate and alimentation—and he merely suggests that temperature and food act at the same time on the cells of the body and on the similar substances in the egg.

H. M. Vernon (*Variation in Animals and Plants*, 1903, pp. 351 *seq.*) cites instances of the cumulative effects of changed conditions of life, and points out that they are not really instances of the inheritance of acquired characters, but merely of the germ-plasm and the body tissues being simultaneously affected. He then asks, Through what agency is the environment enabled to act on the germ-plasm? And answers that the only conceivable one is a chemical influence through products of metabolism and specific internal secretions. He cites several cases of specific internal secretions, making one statement in particular which seems unintelligible, viz. that extirpation of the total kidney substance of a dog leads not to a diminished secretion of urine but to a largely increased secretion accompanied by a rapid wasting away which soon ends fatally.

Whenever a changed environment acts upon the organism, therefore, it to some extent affects the normal excretions and secretions of some or all of the various tissues, and these react not only on the tissues themselves, but also to a less degree upon the determinants representing them in the germ-plasm. Thus the relative size of the brain has decreased in the tame rabbit. This may be due to disuse; the excretions and secretions of the nervous tissues would be diminished, and the corresponding determinants less stimulated. Another instance is afforded by pigmentation of the skin in man; which varies with the amount of light and heat from the sun to which the skin is exposed. Specific excretory products of pigment in the skin may stimulate the pigment determinants in the germ-plasm to vigour. But only those characters of which the corresponding tissues possess a specific secretion or excretion could become hereditary in this way. For instance, the brawny arm of the blacksmith could not be transmitted, as it is scarcely possible that the arm muscles can have a secretion different from that of the other muscles.

In 1904, P. Schiefferdecker [Footnote: P. Schiefferdecker, *Ueber Symbiose*. S.B. d. Niederrhein. Gesellsch. zu Bonn. Sitzung der Medicinischen Sektion, 13 Juni 1904.] made the definite suggestion that the presence of specific internal secretions could be very well used for the explanation of the inheritance of acquired characters. When particular parts of the body were changed, these modifications must change the mixture of materials in the blood by the substances secreted by the changed parts. Thereby would be found a connexion between the modified parts of the body and the germ-cells, the only connexion in existence. It is to be assumed, according to this author, that only a qualitative change in the nutritive fluid of the germ-cells could produce an effect: a quantitative change would only cause increased or decreased nourishment of the entire germ cells.

In my own volume on *Sexual Dimorphism in the Animal Kingdom*, published in 1900, I attempted to explain the limitation of secondary sexual characters not only to one sex, but usually to one period of the individual life, namely, that of sexual maturity; and in some cases, as in male Cervidae, to one season of the year in which alone the sexual organs are active. It had been known for centuries that the normal development of male sexual characters did not take place in castrated animals, but the exact nature of the influence of the male generative organs on that development was not known till a year or two later than 1900, when it was shown to be due to an internal secretion. My argument was that all selection theories failed to account for the limitation of secondary sexual characters in heredity, whereas the Lamarckian theory would explain them if the assumption were made that the effects of stimulation having been originally produced when the body and tissues were under the influence of the sexual organs in functional activity, these effects were only developed in heredity when the body was in the same condition.

About the year 1906, when preparing two special lectures in London University on the same subject, I became acquainted with the work of Starling and others on internal secretions or hormones, and saw at once that the hormone from the testes was the actual agent which constituted the 'influence' assumed by me in 1900. In these lectures I elaborated a definite Lamarckian theory of the origin of Secondary Sexual Characters in relation to Hormones, extending the theory also to ordinary adaptive structures and characters which are not related to sex. Having met with many obstacles in endeavouring to get a paper founded on the original lectures published in England, I finally sent it to Professor Wilhelm Roux, the editor of the *Archiv für Entwicklungsmechanik der Organismen*, in which it was published in 1908.

In his volume on the Embryology of the Invertebrata, 1914 (*Text-Book of Embryology*, edited by Walter Heape, vol. i.), Professor E. W. MacBride in his general summary (chapter xviii.) puts forward suggestions concerning hormones without any reference to those who have discussed the subject previously. He considers the matter from the point of view of development, and after indicating the probability that hormones are given off by all the tissues of the body, gives instances of organs being formed in regeneration (eye of shrimp) or larvae (common sea-urchin) as the result of the presence of neighbouring organs, an influence which he thinks can only be due to a hormone given off by the organ already present. He then states that Professor Langley had pointed out to him in correspondence that if an animal changes its structure in response to a changed environment, the hormones produced by the altered organs will be changed. The altered hormones will circulate in the blood and bathe the growing and maturing genital cells. Sooner or later, he assumes, some of these hormones may become incorporated in the nuclear matter of the genital cells, and when these cells develop into embryos the hormones will be set free at the corresponding period of development at which they were originally formed, and reinforce the action of the environment. In this way MacBride attempts to explain recapitulation in development and the tendency to precocity in the development of ancestral structures. His idea that the hormones act by 'incorporation' in the genital cells is different from that of stimulation of determinants put forward by myself and others, but it is surprising that he should refer to unpublished suggestions of Professor Langley, and not to the publications of authors who had previously discussed the possible action of hormones in connexion with the heredity of somatic modifications.

Dr. J. G. Adami in 1918 published the Croonian Lectures, delivered by him in 1917 under the title 'Adaptation and Disease,' together with reprints of previous papers, in a volume entitled *Medical Contributions to the Study of Evolution*. In this work (footnote, p. 71) the author claims that he preceded Professor Yves Delage by some two years in offering a physico-chemical hypothesis in place of determinants, and also asserts that 'the conclusions reached by him in 1901 regarding metabolites and, as we subsequently became accustomed to term them, hormones, and their influence on the germ-cells, have since been enunciated by Heape, Bourne, Cunningham, MacBride, and Dendy, although in each case without note of his (Adami's) earlier contribution.' These somewhat extensive claims deserve careful and impartial examination. The paper to which Dr. Adami refers was an Annual Address to the Brooklyn Medical Club, published in the *New York Medical Journal* and the *British Medical Journal* in 1901, and entitled 'On Theories of Inheritance, with special reference to Inheritance of Acquired Conditions in Man.' The belief that this paper had two years' priority over the volume of Delage entitled *L'Hérédité* appears to have arisen from the fact that Adami consulted the bibliographical list in Thomson's compilation, *Heredity* 1908, where the date of Delage's work is as 1903. But this was the second edition, the first having been published, as quoted above, in 1895, six years before the paper by Adami.

Next, with regard to the claim that Adami's views as stated in the paper to which he refers were essentially the same as those brought forward by myself and others many years later, we find on reading the paper that its author discussed merely the effect of toxins in disease upon the body-cells and the germ-cells, causing in the offspring either various forms of arrested and imperfect development or some degree of immunity. In the latter case he argues that the action of the toxin of the disease has been to set up certain molecular changes, certain alterations in the composition of the cell-substance so that the latter responds in a different manner when again brought into contact with the toxin. Once this modification in the cell-substance is produced the descendants of this cell retain the same properties, although not permanently. Inheritance of the acquired condition has to be granted, he says, in the case of the body-cells in such cases. But this is not the question: inheritance in the proper sense of the word means the transmission to individuals of the next generation.

On this point Adami says we must logically admit the action of the toxins on the germ-cells, and the individuals developed from these must, subject to the law of loss already noted, have the same properties. He admits that inherited immunity is rare, but says that it has occasionally been noted. Here we have again merely the same influence, chemical in this case, acting simultaneously on somatic cells and germ-cells, which is not the inheritance of acquired characters at all. Adami remarks that Weismann would make the somewhat subtle distinction that the toxins produce these results not by acting on the body-cells but by direct action on the germ-cells, that the inheritance is blastogenic not somatogenic, and calls this 'a sorry and almost Jesuitic play upon words.' On the contrary, it is the essential point, which Adami fails to appreciate. However, he goes further and refers to endogenous intoxication, to disturbed states of the constitution, due to disturbances in glandular activity or to excess of certain internal secretions. Such disturbances he says, acting on the germ-cells, would be truly somatogenic. In the case of gout he considers that defect in body metabolism has led to intoxication of the germ-cells, and the offspring show a peculiar liability to be the subjects of intoxications of the same order. Now, however important these views and conclusions may be from the medical point of view, in relation to the heredity of general physiological or pathological conditions, they throw no light on the problems considered by myself and other biologists—namely, the origin of species and of structural adaptations.

There is no mention anywhere in Adami's short paper of the evolution or heredity of structural characters or adaptations such as wing of Bird or Bat, lung of Frog, asymmetry of Flat-fish or of specific characters, still less of secondary sexual characters, which formed the basis of the hormone theory in my 1908 paper. He does not even consider the evolution of the structural adaptations which enable man to maintain the erect position on the two hind-limbs. He does not consider the action of external stimulation, whether the direct action on epidermal or other external structures or the indirect action through stimulation of functional activity. All his examples of external agents are toxins produced by bacteria invading the body, except in the case of gout, for which he suggests no external cause at all.

Only once in the last of the part of the paper considered does Adami mention internal secretions. His actual words are: 'We recognise yearly more and more the existence of auto-intoxications, of disturbed states of the constitution due to disturbances in glandular activity or to excess of certain internal secretions or of the substances ordinarily neutralised by the same.' The only example he gives is that of gout. How remote this is from the discoveries concerning the specific action of hormones on the growth of the body or of special parts of the body, or on the function of glands, and from a definite hormone theory of heredity as proposed by myself, is sufficiently obvious.

CHAPTER I

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Classification And Adaptation

The study of the animals and plants now living on the earth naturally divides itself into two branches, the one being concerned with their structure and classification, the other with their living activities, their habits, life histories, and reproduction. Both branches are usually included under the terms Natural History, or Zoology, or Botany, and a work on any group of animals usually attempts to describe their structure, their classification, and their habits. But these two branches of biological science are obviously distinct in their methods and aims, and each has its own specialists. The pursuit, whose ultimate object is to distinguish the various kinds of organisms and show their true and not merely apparent relations to one another in structure and descent, requires large collections of specimens for comparison and reference: it can be carried on more successfully in the museum than among the animals or plants in their natural surroundings. This study, which may be called Taxonomics, deals, in fact, with organisms as dead specimens, and it emphasises especially the distinguishing characters of the ultimate subdivisions of the various tribes of animals and plants—namely, species and varieties. The investigation, on the other hand, of the different modes of life of animals or plants is based on a different mental conception of them: it regards them primarily as living active organisms, not as dead and preserved specimens, and it can only be carried on successfully by observing them in their natural conditions, in the wide spaces of nature, under the open sky.

The object of this kind of inquiry is to ascertain what are the uses of organs or structures, what they are for, as we say in colloquial language, to discover what are their functions and how these functions are useful or necessary to the life of the animals or plants to which they belong. For example, some Cuttle-fishes or Cephalopoda have eight arms or tentacles and others ten. The taxonomist notices the fact and distinguishes the two groups of Octopoda and Decapoda.

But it is also of interest to ascertain what is the use of the two additional arms in the Decapoda. They differ from the other arms in being much longer, and provided with sockets into which they can be retracted, and suckers on them are limited to the terminal region. In the majority of zoological books in which Cephalopoda are described, nothing is said of the use or function of these two special arms. Observation of the living animal in aquaria has shown that their functions is to capture active prey such as prawns. They act as a kind of double lasso. Sepia, for instance, approaches gently and cautiously till it is within striking distance of a prawn, then the two long tentacles are suddenly and swiftly shot out from their sockets and the prawn is caught between the suckers at the ends of them. Another example is afforded by the masked crab (*Corystes cassivelaunus*). This species has unusually long and hairy antennae. These are usually tactile organs, but it has been found that the habit of *Corystes* is to bury itself deep in the sand with only the tips of the antennae at the surface, and the two are placed close together so as to form a tube, down which a current of water, produced by movements of certain appendages, passes to the gill chamber and provides for the respiration of the crab while it is buried, to a depth of two or three inches. The results of the investigation of habits and functions may be called Bionomics. It may be aided by scientific institutions specially designed to supplement mere observation in the field, such as menageries, aquaria, vivaria, marine laboratories, the objects of which are to bring the living organism under closer and more accurate observation. The differences between the methods and results of these two branches of Biology may be illustrated by comparing a British Museum Catalogue with one of Darwin's studies, such as the 'Fertilisation of Orchids' or 'Earthworms.'

Other speculations in Biology are related to Taxonomics or Bionomics according as they deal with the structure of the dead organism or the action of the living. Anatomy and its more theoretical interpretation, morphology, are related to Taxonomics, physiology and its branches to Bionomics. In fact, the fundamental principles of physiology must be understood before the study of Bionomics can begin. We must know the essential nature of the process of respiration before we can appreciate the different modes of respiration in a whale and a fish, an aquatic insect and a crustacean. The more we know of the physiology of reproduction, the better we can understand the sexual and parental habits of different kinds of animals.

The two branches of biological study which we are contrasting cannot, however, be completely separated even by those whose studies are most specialised. In Bionomics it is necessary to distinguish the types which are observed, and often even the species, as may be illustrated by the fact that controversies occasionally arise among amateur and even professional fishermen on the question whether dog-fishes are viviparous or oviparous, the fact being that some species are the one and others the other, or the fact that the harmless slow-worm and ring-snake are dreaded and killed in the belief that they are venomous snakes. Taxonomics, on the other hand, must take account of the sex of its specimens, and the changes of structure that an individual undergoes in the course of its life, and of the different types that may be normally produced from the same parents, otherwise absurd errors are perpetrated. The young, the male, and the female of the same species have frequently been described under different names as distinct species or even genera. For example, the larva of marine crabs was formerly described as a distinct genus under the name of *Zoaea*, and in the earlier part of the nineteenth century a lively controversy on the question was carried on between a retired naval surgeon who hatched *Zoaea* from the eggs of crabs, and an eminent authority who was Professor at Oxford and a Fellow of the Royal Society, and who maintained that *Zoaea* was a mature and independent form. In the end taxonomy had to be altered so as to conform with the fact of development, and the name *Zoaea* disappeared altogether as that of an independent genus, persisting only as a convenient term for an important larval stage in the development of crabs.

These two kinds of study give us a knowledge of the animals now living. But we find it a universal rule that the individual animal is transitory, that the duration of life, though varying from a few weeks to more than a century, is limited, and that new individuals arise by reproduction, and we have no evidence that the series of successive generations has ever been interrupted; that is to say, the series in any given individual or species may come to an end; species may be exterminated, but we know of no instance of individuals coming into existence except by the process of reproduction or generation from pre-existing individuals. Further, we know from the evidence of fossil remains that the animals existing in former periods were very different from those existing now, and that many of the existing forms, such as man, mammals, birds, bony fishes, can only be traced back in the succession of stratified rocks to the later strata or to those about the middle of the series, evidence of their existence in the periods represented by the most ancient strata being entirely absent. Existing types then must have arisen by evolution, by changes occurring in the succession of generations.

These three facts—namely, the limited duration of individual life, the uninterrupted succession of generations, and the differences of the existing animals and plants from those of former geological periods whose remains are preserved in stratified rocks—are sufficient by themselves to prove that evolution has taken place, that the history of organisms has been a process of descent with modification. If the animals and plants whose remains are preserved as fossils, or at any rate forms closely related to these, were not the ancestors of existing forms, there are only two other possibilities: either the existing forms came into existence by new creations after the older forms became extinct, or the ancestors of existing forms, although they coexisted with the older forms, never left any fossil remains. Each of these suppositions is incredible.

In view of these plain facts and their logical conclusion it is curious to notice how Darwin in his *Origin of Species* constantly mingles together arguments to prove the proposition that evolution has occurred, that the structure and relations of existing animals can only be explained by descent with modification, with arguments and evidence in favour of natural selection as the explanation and cause of evolution. In the great controversy about evolution which his work aroused, the majority of the educated public were ultimately convinced of the truth of evolution by the belief that a sufficient cause of the process of change had been discovered, rather than by the logical conclusion that the organisms of a later period were the descendants of those of earlier periods. Even at the present day the theory of natural selection is constantly confused with the doctrine of evolution. The fact is that the investigation of the causes of evolution has been going on and has been making progress from the time of Darwin, and from times much earlier than his, down to the present day.

Bionomics show that every type must be adapted in structure to maintain its life under the conditions in which it lives, the primary requirements being food and oxygen. Every animal must be able to procure food either of various kinds or some special kind—either plants or other animals; it may be adapted to feed on plants or to catch insects or fish or animals similar to itself; its digestive organs must be adapted to the kind of food it takes; it must have respiratory organs adapted to breathe in air or water; it must produce eggs able to survive in particular conditions, and so on.

One of the most interesting results of the study of the facts of evolution is that each type of animal tends to multiply to such an extent as to occupy the whole earth and adapt itself to all possible conditions. In the Secondary period reptiles so adapted themselves: there were oceanic reptiles, flying reptiles, herbivorous reptiles, carnivorous reptiles. At the present day the Chelonia alone include oceanic, fresh-water, and terrestrial forms. Birds again have adapted themselves to oceanic conditions, to forests, plains, deserts, fresh waters. Mammals have repeated the process. The organs of locomotion in such cases show profound modifications, adapting them to their special functions. One thing to be explained is the origin of adaptations.

It is, however, necessary to distinguish between the adapted condition or structure of an organ and the process by which it became adapted in evolution; two ideas which are often confused. The eye would he equally adapted for seeing whether it had been created in its actual condition or gradually evolved. We have to distinguish here, as in other matters, between being and becoming, and, further, to distinguish between two kinds of becoming—namely, the development of the organ in the individual and its evolution in the course of descent. The word 'adaptation' is itself the cause of much fallacious reasoning and confusion of ideas, inasmuch as it suggests a process rather than a condition, and by biological writers is often used at one time to mean the former and at others the latter. We may take the mammary glands of mammals or organs adapted for the secretion of milk, whose only function is obviously the nourishment of the offspring. Here the function is certain whatever view we take of the origin of the organs, whether we believe they were created or evolved. But if we consider the flipper or paddle of a whale, we see that it is homologous with the fore-leg of a terrestrial mammal, and we are in the habit of saying that in the whale the fore-limb is modified into a paddle and has become adapted for aquatic locomotion. This, of course, assumes that it has become so adapted in the course of descent. But the pectoral fin of a fish is equally 'adapted' for aquatic locomotion, but it is certainly not the fore-leg of a terrestrial mammal adapted for that purpose. The original meaning of adaptation in animals and plants, of organic adaptation to use another term, is the relation of a mechanism to its action or of a tool to its work. A hammer is an adaptation for knocking in nails, and the woodpecker uses its head and beak in a similar way for making a hole in the bark of trees. The wings and the whole structure of a bird's body form a mechanism for producing one of the most difficult of mechanical results, namely, flight. Then, again, there are stationary conditions, such as colour and patterns, or scales and armour, which may he useful in the life of an animal or flower, but are not mechanisms of moving parts like a bird's wing, or secreting organs like mammary glands. Unless we choose or invent some new term, we must define adaptations apart from all questions of evolution as any structures or characters in an organism which can be shown either by their mere presence, or by their active function, to be either useful or necessary to the animal's existence. We must be on our guard against assuming that the word 'adaptation' implies any particular theory or conclusion concerning the method and process by which adaptations have arisen in the course of evolution. It is that method and process which we have to investigate.

On the other hand, when we look primarily at differences of structure we find that not only are there wide and distinct gaps between the larger categories, such as mammals and birds, with few or no intermediate forms, but the actual individuals most closely similar to one another naturally and inevitably fall into distinct groups which we call kinds or species. The conception of a species is difficult to define, and authorities are not agreed about it. Some, like Professor Huxley, state that a species is purely a mental conception, a generalised idea of a type to which actual individuals more or less closely conform. According to Huxley, you cannot lock the species 'horse' in a stable. Others regard the matter more objectively, and regard the species merely as the total number of individuals which possess a certain degree of resemblance, including, as mentioned above, all the forms which may be produced by the same parents, or which are merely stages in the life of the individual. There are cases in which the limits of species or the boundaries between them are indistinct, where there is a graduated series of differences through a wide range of structure, but these cases are the exception; usually there are a vast majority of individuals which belong distinctly to one species or another, while intermediate forms are rare or absent. The problem then is, How did these distinct species arise? How are we to explain their relations to one another in groups of species or genera; why are the genera grouped into families, families into orders, orders into classes, and so on?

There are thus two main problems of evolution: first, how have animals become adapted to their conditions of life, how have their organs become adapted to the functions and actions they have to perform, or, at least, which they do perform? The power of flight, for example, has been evolved by somewhat different modifications in several different types of animals not closely related to one another: in reptiles, in birds, and in mammals. We have no reason to believe that this faculty was ever universal, or that it existed in the original ancestors. How then was it evolved? The second great problem is, How is it that existing animals, and, as the evidence of the remains of extinct animals shows, these that existed at former periods of time also, are divided into the groups or types we call species, naturally classified into larger groups which are subdivisions of others still larger, and so on, in what we call the natural system of classification? The two problems which naturalists have to solve, and which for many recent generations they have been trying to solve, are the Origin of Species and the Origin of Adaptations.

Former generations of zoologists have assumed that these problems were the same. Lamarck maintained that the peculiarities of different animals were due to the fact that they had become adapted to modes of life different to those of their ancestors, and to those in which allied forms lived, the change of structure being due to the effect of the conditions of life and of the actions of the organs. He did not specially consider the differences of closely allied species, but the peculiarities of marked types such as the long neck of the giraffe, the antlers of stags, the trunk of the elephant,