NATURE SERIES

EXPERIMENTAL EVOLUTION

LECTURES

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BY

HENRY DE VARIGNY, D.Sc.

DEMONSTRATOR IN THE PARIS MUSEUM D'HISTOIRE NATURELLE, MEMBER OF THE SOCIÉTÉ DE BIOLOGIE

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PREFACE

THE following pages contain a record of lectures which I delivered in August, 1891, in Edinburgh, before the very cultivated and attentive audiences afforded by the *Summer School of Art and Science* which Professor Patrick Geddes is evolving with great expense of energy and devotion to science, aided by some personal friends whose interest he has well awakened in the organisation and diffusion of knowledge. I wish to be reckoned among these; and I took part with much pleasure in the proceedings, while on a visit to Edinburgh, Oxford, Cambridge, and London, where the French Government had commissioned me to investigate the University Extension Movement.

These lectures do not cover the whole ground of the subject: I have purposely given most attention to documents and facts of French origin, as they are certainly less familiar to an English audience, although the similar facts and documents of English origin are, if anything, more familiar to myself, and also much more extensive in some lines. My desire has been more specially to show what should be done, in future, on behalf of the Evolution Theory, so that I may be excused if I have not gone entirely through the facts of the past ; and as I consider that experiment is now the only method of securing any further advance in solving the problems of organic evolution, I have wished to state the matter clearly, and to give some circulation to the statement in the country where this line of study has most followers.

And now I should ask of my readers to excuse such literary or grammatical defects as they may meet with in this volume. A foreigner can scarcely be expected to master all the niceties of the English language. However, my friends Prof. Patrick Geddes and J. A. Thomson having been kind enough to look over the proofs—and this I most sincerely thank them for—I feel the most important inelegancies or errors have been excluded, although English readers must doubtless perceive that the author is not writing in his native language.

H. de V.

Montmorency (Seine et Oise), France,
October 10*th*, 1891.

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DURING countless ages, of which centuries are mere moments, and whose number and length we can yet by no known method pretend to appreciate, our planet—an atom amidst an infinite world of similar bodies—has been teeming with life. Innumerable millions of plants and animals have lived and died, on the earth, in the waters, in the air; and if we can hardly estimate the number of the forms of life, it is impossible to obtain any idea of the enormous—although finite—number of the individuals. How many plants were required to form a square foot of coal; and of how many Protozoa and sponges is a cubic inch of chalk the only vestige? Who could dare to form an estimate of the number of organisms which have disappeared and died without leaving a single vestige, whose bodies, through the slowly disintegrating processes of decomposition, aerial or submarine, have abandoned their elements to the atmosphere, the water, and the soil,—the materials of life whence they have unceasingly returned to new organisms in the course of that circulus which, like life itself, knows neither rest nor immobility? The very elements which at the present moment are parts of ourselves, of our bones, of our flesh, of our blood, brain, or nerve, were part, not very long ago, of our ancestors—further back still, of prehistoric man; and in a remote past, of that inconceivable number of organisms of part of which the sedimentary strata are the enormous burial-ground. And when we come to consider that the circulation of matter is unceasing and continuous between the earth, the air, and the water on the one hand, and all living organisms, animals, or plants, on the other, we cannot help coming to the conclusion that all present life is made up from the elements of past life—that we are verily the flesh and blood of the dead, recent or remote, and that the air we inhale, the water we drink, the food we eat, are for the greater part made with elements derived from these dead.

This notion is a very simple one, and certainly familiar to all. And yet, its origin is not very remote. Not to speak of our ugly brute of an ancestor, that prehistoric man, who struggled hard for dear life—and this we must thank him for—under hard times and against many foes, without tools without weapons, "*sans* everything" in fact, and who, we may imagine, had but little spare time left for philosophical meditations after he had provided the food necessary to his companion and progeny, and settled his little accounts with troublesome neighbours, bipedal or quadrupedal, did this notion ever cross the brain of the Gauls, Celts, or Britons of old? Did it even suggest itself to that old father of science, to Aristotle, or to his commentators of mediaeval times? Surely not, and in fact, no exact idea of the circulation of matter could be obtained even a century ago, when chemistry was yet only entering upon life, and acquiring the dignity of a science.

The same is true of the greater part of our modern ideas and science.

The philosopher of Geneva has said[1] that "the condition of him who reflects is an anti-natural condition, and that the man given to meditation is a depraved animal." Some doubts may be entertained concerning the second term of this sentence, although the unhealthiness of meditation is often obvious ; but it is certainly true that under natural conditions, very little time is available for meditative purposes. Even in our so-called civilized life, how very small is the number of those who do often really think and ponder on topics which are neither food, nor dress, nor money. Most men live, in fact, without troubling themselves with any of the really great questions which force themselves on the attention of the few who think, and they leave those vexed questions of force, matter, movement, space, life, consciousness, death, will, memory, morality, right and wrong, to the physicist, biologist, and moralist, with some disdain, pitying them most thoroughly for devoting their time, life, and energy to problems which apparently do not admit of being solved.

The thinkers are however not of the same mind as the multitude ; they entertain the opinion—absurd in the eyes of many—that human science has yet a

[1] J. J. Rousseau, *Discours sur l'Origine et les Fondements de l'Inégalité parmi les Hommes*, 1754.

long road to travel, that unexplored fields are yet innumerable, and that no problem can be considered insoluble so long as it has not been subjected to a thorough investigation by means of all available methods. If we consider that the number of these methods increases each day, if we remember that discoveries which seem quite insignificant are often pregnant with the most important deductions—from Galvani to the telegraph and the telephone one small century only has elapsed—we may fairly conclude that a problem which hardly admits to-day of any investigation may suddenly be solved to-morrow. I may be allowed to quote an instance among many. Some forty years ago a young man spent a long time in the seemingly very speculative and idle study of dissymmetry and symmetry in various crystals. The practical value of such investigation seemed to be nought, and at all events it had no interest save for the elucidation of some points in crystallography. But this investigation led logically to a study of fermentation, and the final outcome of Pasteur's earliest work has been—leaving out the stepping stones—the discovery of the real cause of a large number of diseases, the cure of one of them, and the expectation, based on facts, that all of these diseases can be defeated by appropriate methods. Little causes have great effects, and no ascertained fact is useless ; this must be kept in mind.

To return to our question, we may conclude that no problem is out of man's reach : there is none he may not grapple with, more or less successfully.

But this notion, also, is a very modern one, and while the many at present certainly disagree with it, we do not require to go centuries back to find the thinkers themselves of a different opinion on the matter, and sharing the creed of the mass.

This explains how it is that man has so long delayed to investigate the problem of the present day, which is the problem of the world. He considered the problem as admitting of no possible investigation, and accepting the Scriptures as a scientific text-book as well as a book of morals, he even perceived no problem at all, and lived in quietness and repose. But some were at work, observing, comparing, and noting facts. Their names are familiar to all, and the outcome of their diverse work and tendencies broke upon the world in 1859. For Darwin, while seemingly inquiring only about the origin of cats and dogs, and pigeons, and their probable relationship with antecedent similar animals, had opened new prospects. This was immediately understood ; and behind the cats, dogs, pigeons, swine, and cattle, all beheld a new system of the world, quite different from that of the currently accepted creed.

The general system we shall leave out in the course of these lectures ; that which concerns us here is its application to the organic kingdom.

How did animals and plants come to life, and how are we to explain the present state of nature ?

As Professor Huxley clearly states in his *American Addresses*, there are only three hypotheses concerning this matter.

The one is that the present state of things has always existed, and, I presume, never began. This is one of the propositions which Herbert Spencer terms unthinkable. There must have been a beginning, because we know there is an end of all things ; but in fact, it would be perfectly Quixotic to argue against this windmill, as no one works it. I am not aware that any scientists maintain this position.

The second hypothesis is that the present world of animals and plants began suddenly in some past epoch, in the course of the days or periods of creation : this is the theory of the book of Genesis, of Milton's *Paradise Lost*, the orthodox theory of the greater part of the civilized millions.

This hypothesis is a more rational one, and no objection could be raised against it, in my opinion, if the facts we are acquainted with were not in direct contradiction with it. As to the feasibility of a creation after this mode, we can entertain no doubt. Miraculous and incomprehensible is this theory: but all theories which pretend to explain the beginnings are so; they cannot avoid recurring to the hypothesis, either of the spontaneous generation of matter, energy, and life—of the whole intricate and complex system of things, or of the creation of all this by some mighty being, the watchmaker of Paley's well-known argument. But who made the watchmaker? it is naturally asked. To the last question neither you nor I are prepared to answer, and as none can answer it, many come to the conclusion that the question is absurd and must be dismissed as untenable. We must confess that this question is above our reason. But what of the first alternative? Can any one of us show matter, life, or energy spontaneously generated out of nothing? No! and all the progress of physics and chemistry goes to show how numerous are the transformations of energy and matter, and to confirm steadily the axiom *ex nihilo nihil*. If we are asked to believe in spontaneous creation of life, matter, or energy, we merely answer that this belief goes against our reason, because we are asked to believe in things which are contrary to all our experience.[1] And

[1] This does not prevent Basile Conta, in his interesting *Origine des Espèces* (Paris, Alcan, 1888), from supposing that spontaneous genera-

between two creeds, the one above, the other against our reason, we prefer the first, willingly admitting that our present intelligence is not able to understand most of the phenomena we are acquainted with—though we sincerely expect it to become so in the course of ages—and that any adequate idea of the origin of the world is a thing much above the grasping powers of our intellect. Suppose some savages discovering one morning on a sea-beach a watch stranded from some shipwreck. No doubt, they all will come and cluster eagerly around it, and if the unfortunate machine be water-proof, they will listen to the ticking with great wonder, and will furthermore believe that this watch is some new and curious sort of animal which takes more pleasure in ticking than in anything else. If those savages are not mere brutes I presume some old wise man will express his astonishment, and immediately advise his slaves and followers to go and give fruit and hogs to the priests, because this is an extraordinary event which has some extraordinary origin. And if you were to tell him that this watch had been made by the rushing together of fragments of stone and sand, he would not believe it. At least, I

tion has taken place, and is even taking place daily. He supposes that the inferior organisms are of very recent, contemporary origin, while the superior are derived from similar inferior organisms which have sprung into life in periods which are remote in proportion to the degree of perfection reached by the higher species.

hope not. We are such savages. The more we study nature in its broadest sense, the more its wheels seem intricate, and its movements complex. Of course we do not understand how it was made so, we do not understand the watchmaker, nor even his design and purpose in making the watch, but we perceive the watch, we understand part of its movements, and so are compelled to believe in the existence of the watchmaker, although we can form no definite idea concerning him.

But the fact of believing in the watchmaker's existence, which is forced upon us by the fact that we never have seen a single watch come spontaneously into existence, and that our experience shows that no single element—wheel, axis, or spring—has ever spontaneously appeared, does not necessarily compel us to accept the second, above-named, of the three hypotheses which Prof. Huxley recognizes. We may very well accept the watchmaker's existence without being obliged to believe that he made the watch in the particular method described in the Scriptures, and assumed by the adherents of the Special Creation Theory. And we cannot—so long as neither energy nor matter can be shown to arise spontaneously out of nothingness—we cannot, upon any theory, dispense with the existence of a Creator.

The third hypothesis is the hypothesis of Evolution.

I cannot do better than quote Prof. Huxley's own words. It "supposes that at any comparatively late period of past time, our imaginary spectator [supposed to be a witness of the history of the earth] would meet with a state of things very similar to that which now obtains, but that the likeness of the past to the present would gradually become less and less, in proportion to the remoteness of his period of observation from the present day; that the existing distribution of mountains and plains, of rivers and seas, would show itself to be the product of a slow process of natural change operating upon more and more widely different antecedent conditions of the mineral framework of the earth; until at length, in place of that framework he would behold only a vast nebulous mass, representing the constituents of the sun and of the planetary bodies. Preceding the forms of life which now exist, our observer would see animals and plants, not identical with them, but like them; increasing their difference with their antiquity, and at the same time, becoming simpler and simpler, until finally the world of life would present nothing but that undifferentiated protoplasmic matter which, so far as our present knowledge goes, is the common foundation of all vital activity."[1]

To put it briefly: the evolutionary hypothesis sup-

[1] *American Addresses. Three Lectures on Evolution*, p. 10.

poses that from matter and force, the entire world and the life it contains—their past, present, and future—have been, are, and will be evolved by a natural process, without any special interference of the Creator, whose existence seems to me necessarily assumed.

To quote again Professor Huxley:[1] "The hypothesis of evolution supposes that in all this vast progression there would be no breach of continuity—no point at which we could say, 'This is a natural process,' and 'This is not a natural process,' but that the whole might be compared to that wonderful process of development which may be seen going on every day under our eyes, in virtue of which there arises, out of the semi-fluid, comparatively homogeneous substance which we call an egg, the complicated organization of one of the higher animals."

Excluding the first hypothesis, which explains nothing, and merely ignores the problem to be solved, the whole discussion is between the second and third. And we cannot wonder at the rivalry displayed on both sides—we cannot wonder at the passionate fighting which has been carried on at each discussion of the matter, when we consider that in fact the question is not merely zoological, but metaphysical and speculative, and that the real

[1] *American Addresses. Three Lectures on Evolution*, pp. 10, 11.

matter under discussion is not the origin of species, but the origin of the world and of all it contains, man himself included.

It would require much time to convey a correct idea of the gradual evolution of the Evolution theory itself. It did not spring out of the brain of one man, fully equipped and ready, as Minerva is said to have come to life. On the contrary, it developed slowly, cautiously, very timidly, we may say. And this for good reasons, prominent among which was the unanimous assault its defenders had to receive each time they tried to say a word in its favour. No doubt these fights and defeats were unpleasant, but they turned to the advantage of the vanquished, and each defeat became a tonic to him, by which his forces were invigorated and freshened. The idea of evolution, and more specially of organic evolution, is, however, of recent origin. At first it was a very vague and unscientific notion. Although I have no intention of giving an historical account of the evolution of the Evolution theory, I may be allowed to give some instances, to show some stepping-stones. Similar instances are certainly to be found in English and German literature, but I shall be content with quoting here some facts belonging to French literature, as they may be less familiar to most English readers.

In 1609 Mons. Claude Duret, President of the Bench in the town of Moulins, in central France, published a quaint book with still quainter illustrations. This *Histoire admirable des Plantes* . . .[1] contains evolutionary notions of a very queer sort. He fully believes that many aquatic birds, as well as many sorts of insects, are generated from the rotten wood of trees. In Scotland it seems—perhaps some of you have heard of the fact and are ready to vouch for it—in Scotland there is one sort of tree more peculiar than others; the leaves which fall on the ground yield birds, while those which fall into water are soon changed into fishes. There is no doubting the fact, as the scene is very distinctly depicted in an old wood-engraving. A photograph, however, would be more convincing, but then Daguerre and Niepce had not made their appearance at that time.

It may be remarked that some seventy years later Father Kircher, in his *Mundus Subterraneus*,[2] still believed in many strange notions of the same sort, and depicted the genesis of birds, apes, and men by means of the transformation of some orchids. He had been

[1] The full title is: *Histoire admirable des Plantes et Herbes esmerveillables et miraculeuses en Nature, mesmes d'aucunes qui sont vrays Zoophytes ou Plantes Animales . . . . avec leur Portraits au naturel.* 18°. Paris, Nicolas Brion, 1609.

[2] Amsterdam, 2 vols. in folio, 1678.

struck with the resemblance of these strange flowers to many animals, and therefore concluded that the latter were derived from the former.

In the meantime De Maillet, French consul in Leghorn and in Egypt during a number of years, wrote, at the end of his life, a strange book called *Telliamed*[1] (his own name reversed). The greater part of it has little to do with the matter under discussion, but in the last ninety pages, after having considered the real nature of fossils—a question at that time much discussed, and concerning which the truth became established only after numerous difficulties—De Maillet concerns himself with the origin of man and animals. His main idea is that all terrestrial and aerial animals have their origin in some corresponding marine form. For instance, birds are derived from flying fishes, lions from sea-lions, &c., and man from the "*homme marin*," the husband of the mermaid. The reason he gives for these derivations is curious enough. Considering the many islands—there were more of them in his time than at the present day—which, although uninhabited by man, contain animals and plants, he argues that if these animals and plants are not derived from marin

[1] *Telliamed; ou, Entretiens d'un Philosophe Indien avec un Missionaire Français, etc., mis en ordre sur les Mémoires de feu M. de Maillet.* Basle, 1749.

forms, "we must assume a new creation, which is absurd" (p. 313).

Some years after De Maillet, another French writer gave utterance to more valuable notions concerning evolution. This author was J. B. Robinet. There is but little to interest us in his book, *De la Nature*, published in 1766 (four 8vo volumes, Amsterdam,) but his *Vues philosophiques de la gradation naturelle des Formes de l'Etre, ou les Essais de la Nature qui apprend à faire l'Homme* (1768, Amsterdam,) contain curious passages. For instance, he clearly recognized the fact that all animals are in many points similar, and that if the similarity between any two animals at the opposed ends of the organic scale is difficult to perceive when they are considered apart from the others, numerous transitional forms occur, and are real connecting links when the whole scale is taken into consideration.

Robinet supposes that Nature has an aim, a constant tendency towards perfection, and towards perfection of a given type. Since the beginning the aim of Nature has been to prepare man, and the proofs thereof are not wanting, according to Robinet. These proofs are the numerous stones or fossils which bear a more or less vague resemblance to the organs and various parts of man, monstrous turnips and extraordinary cabbages, in the form of a hand, a nose, or an ear, or other parts of the body, whether internal or external. Robinet is not very clear about the method which Nature followed in order to attain her object, but the last part of his story is quite fluent, and the ape appears as the last effort of Nature before she succeeded in making man.

This is very crude and elementary evolutionism, to be sure, and the names of Robinet, De Maillet, and Duret[1] have but slight historical interest, but it must be remembered that between Robinet and Darwin not a century elapsed, and there lies the reason for which I have wished to recall briefly the quaint notions of these transformists of the past. A word, however, may be said in their defence; we must remember that at the time they wrote, little was known concerning species, and no idea could be obtained concerning their origin and derivation, so long as their nature was ignored.

Evolutionism, scientific and really deserving this name, appeared only a few years after the publication of Robinet's ungainly views, and here the French scientists took a prominent part.

Buffon comes first. Much has been said and written concerning the orthodoxy of the great naturalist, and contradictory statements have been made, so

[1] For details concerning their theories *cf.* Henry de Varigny : *La Philosophie Biologique aux xvii^e et xviii^e Siècles, Revue Scientifique*, August 29th, 1889. Also De Quatrefages, *Charles Darwin et ses Précurseurs français*, 1870, of which a new edition is in the press (1892).

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that many know not whether he is to be accounted as a friend or as a foe. The truth is that Buffon's views on the unity of species lacked unity themselves. From 1753 to 1756, it is quite clear, as Geoffroy Saint Hilaire has shown, that he believed in their immutability. "Species in animals," he says, "are all separated from each other by an interval which Nature cannot cross." Later on, his writings show a different turn of mind, and from 1761 to 1766, more particularly give evidence thereof. "One is surprised at the promptness with which the species vary, and at the ease with which they become altered and assume new forms."

This theory of the mutability of species he considered, some years later, to be rather exaggerated, and he returned to more moderate views, though not abandoning the theory of variability and mutability, which, in his opinion, are due to the direct influence of environment.

After Buffon comes Lamarck, a friend and pupil of the former. Lamarck was the first to state distinctly, in any developed form, the theory of the variability and transmutation of species, which many had before him briefly proposed or supposed, and he tried to discover the cause of this variability.[1] The facts of varia-

[1] See his *Philosophie Zoologique*, 1809; *Introduction à l'Histoire naturelle des Animaux sans Vertèbres*, 1815; and *Système de Connaissances positives*, 1820.

bility were supplied to him by all sorts of animals, and in part by the domesticated forms, and among these the pigeon and fowl, of which, later, Darwin made great use. Lamarck believed in spontaneous generation—under laws given by a Creator—of elementary organisms which became gradually perfected and transformed into higher beings, under other laws which Lamarck recognized and stated. Among these laws is that of the hereditary transmission of acquired characters, which is at present so much discussed, after Weismann's opposition. As to the cause of variability, it is to be found, says Lamarck, in "new needs of the organism," so that the influence of environment plays but an accessory part. Whatever opinions may be entertained as to the views of this naturalist concerning the causes and methods of variation, it must be conceded that he was the first clearly to perceive and state the problem of the origin of species.

Geoffroy Saint Hilaire (Etienne) was rather a disciple of Buffon than of Lamarck. He believed much in the influence of environment[1] and fought hard against Cuvier and his views, while Bory de Saint Vincent upheld the views of Lamarck. Of Geoffroy Saint Hilaire we shall have to speak again further on. We do not pretend to give here any com-

[1] See his *Sur le Degré d'Influence du Monde ambiant pour modifier es Formes animales.*

plete account of the progress of the Evolution theory—more especially after having announced our desire to restrict ourselves chiefly to French naturalists—and shall dwell no longer on this point of history. It must be recalled, however, that Linnaeus, in 1762 (*Amœnitates*) had expressed the idea that all species of the same genus *ab initio unam constituerunt speciem*, without saying, however, how the differentiation of he primitive one species into many had taken place. Moreover many writers whose names are given by Darwin in his *Origin of Species*, anticipated him more or less, not in his explanation of the *quomodo* of transmutation, but in the statement of the fact, or theory.

There is, however, one name to which attention must be called; it is that of Naudin, the veteran French botanist, who, in 1852, published a very interesting paper in the *Revue Horticole* (1852, p. 102). As he recently wrote to me[1] this paper was published by the editors of the *Revue* with much diffidence; they cared little about theoretical discussions, and the hypothesis of transmutation was "nowhere" in their opinion. Some passages are of much interest, and may be quoted here.

[1] "The editors of the *Revue Horticole* did not feel inclined to allow such heretical notions to be expounded in it; they accepted the paper, however, throwing the whole responsibility upon myself, and fearing to injure their orthodoxy through an impure alloy."—(Letter, dated March 6th, 1891.)

"We do not think that Nature has made her species in a different fashion from that in which we proceed ourselves in order to make our variations. To say the truth, we have practised her very method. When we wish, out of some zoological or botanical species, to obtain a variety which answers to such or such of our needs, we select (*choisissons*) out of the large number of the individuals of this species, so as to make them the starting point of a new stirp, those which seem already to depart from the specific type in the direction which suits us, and, by a rational and continuous sorting of the descendants, after an undetermined number of generations we create types or artificial species, which correspond more or less with the ideal type we had imagined, and which transmit the acquired characters to their descendants in proportion to the number of generations upon which our efforts have been bearing. Such is, in our opinion, the method followed by Nature; as well as by ourselves; she has wished to create races conformable to her needs, and, with a comparatively small number of primitive types, she has successively, and at different periods, given birth to all the animal and vegetable species which people the earth." . . .

This says nothing of the reason for which Nature follows such a method, but the method is exactly that which we know under the name of natural selection and artificial selection. It seems fair to say that Naudin's name deserves a high place in the history of the rise and progress of evolutionary thought, and the paper to which I allude is not generally well known, even to writers familiar with the subject of evolution. Of Darwin's work I shall say nothing: all are familiar with the principles which lie at the root of his theory; but it would be unfair not to put Herbert Spencer's name on the list, close to his.

And now we may briefly recapitulate the theories which have been proposed on both sides to explain the present condition of the organic world.

On the special creation side we meet with four distinct views:

(1) Our planet, long uninhabited, has become peopled with the types and forms it now contains, from another planet in which these existed, and which has fallen on ours.

This hypothesis might be discussed by savages or by lunatics; to me it seems useless to show its failure, of which the least is that it merely puts off the problem without any attempt towards solving it.

(2) All species have been specially created from the beginning of the world—a very elastic term, to be sure—and have lived in part, or in whole, until the present day, without any alteration.

This hypothesis is untrue, as it is known that most living species occur only very seldom in strata of some antiquity, and that the present fauna, for instance, is quite different from the various faunas which lived in the different geological epochs.

(3) A large number of living beings specially created at the beginning, having been killed by various cataclysms, they have been created anew after the catastrophes which, in Cuvier's opinion, are the necessary concomitants of every geological epoch. This is Cuvier's hypothesis of the "revolutions of the earth." It may seem somewhat puerile to suppose that the Creator has seen His own doing turn against Him, and oblige Him to begin His work anew, on repeated occasions.

(4) All species have been specially created from the beginning, but while some die out gradually, other new ones put in a sudden appearance, for reasons hitherto unexplained.

This is only a part of an hypothesis, as it does not explain why or how new species appear, which is exactly the problem to be solved.

On the adverse, non-special creation side, we have only one hypothesis, the evolutionary one, which supposes all living species to have been evolved from antecedent and different organisms—all organisms having perhaps been evolved out of a single elementary one, born we know not how, but certainly created, unless we can believe that matter, energy, and life can originate spontaneously.

Between the Special Creation and the Evolution theory the contest has been a fierce one, for reasons already given; but it may be said that at the present moment the last-named has gathered around it the greater army.

But it must be confessed, at the same time, that no positive and direct proof of the truth of the evolutionary theory has yet been given. It is true none can be given either of the opposite theory. In the very year, 1852, in which Naudin gave utterance to his theory of the origin of species by means of selection, Herbert Spencer published a short essay on the *Development Hypothesis*, which has been republished in his recent edition of *Essays*, as the first of the whole series. In this essay he speaks of the anti-evolutionists, who argue that "as in all our experience we know no such phenomenon as transmutation of species, it is unphilosophical to assume that transmutation of species ever takes place," and forget that "as in all our experience we have never known a species created, it is unphilosophical to assume that any species has ever been created." We cannot exactly adhere to this reasoning. If species have been created, they may have been so before man could see them, while if species are derived from each other by evolution, there is no reason why the process should not be at all times going on, and why man should not witness it. So, on that point, creationists are entitled to ask of evolutionists demonstrations which, conversely, the latter cannot require from the former.

Without proceeding to discuss more amply the matter so very well discussed by Herbert Spencer in this essay, I wish to recall briefly to your memory the general proofs of organic evolution as they are known at present.

One of these proofs, or arguments, is that which results from palaeontological studies. Broadly speaking, an evolution in the animal and vegetable kingdoms is indicated by the fact that the older strata of the earth contain organisms which are simpler than those which are contained in the newer, or are living at present. For instance, no Vertebrates are known in the Silurian strata save some lowly-organized fishes, and it is only in later deposits that the other groups put in an appearance. Of course, much may be said, even now, concerning the provisional condition of our palaeontological knowledge. We know but little of the contents of the geological strata, and of the greater part of the globe we are totally ignorant. Future investigations and discoveries may considerably alter the present situation; and, on the other hand, our geological notions may require important alterations as concerns synchronism and heterochronism of the strata and of their contents. It may happen that vestiges of animals which we consider as very recent may be found in much older strata; it may also happen that some types have been evolved in very limited portions of the globe at different times and with different characters. It may be granted that our geological conceptions require to be revised, and in many cases altered. But however fragmentary and imperfect our present knowledge may be, it nevertheless yields important conclusions. Through palaeontology we perceive in some cases the passage from one group of animals to another, and while theory shows that birds are probably in close relationship with reptiles, the Jurassic strata yield *Archæopteryx lithographica*, which partakes of the character of both groups, and in the more recent Tertiary deposits we meet with many forms which have now disappeared, but are in intimate connection with the existing species of many orders, and seem positively to be the ancestors from which the latter have been evolved with slight modifications. It is enough to recall the important investigations of Gaudry, Leidy, Falconer, Cope, Marsh, Boyd-Dawkins, and Lartet, who have traced, with the utmost probability, the exact line of descent from those fossil forms of older strata to those which live at the present time.

Palaeontology shows in some cases the process of evolution in much detail. I may refer to the researches of Hilgendorf and Hyatt, at Steinheim in Würtemberg. They show that while the different species of *Planorbis*, when considered in the most recent of these strata, are very dissimilar, if the series is studied from the lower to the upper ones, it is easily seen that out of four initial forms, not very different from each other, slightly different forms have in the course of time originated, becoming, as we consider more recent strata, always more diversified, always more different from their ancestors, and from one another. While the origin is the same, the results are quite dissimilar, and if the older strata were wanting, no possible link could be found between the very dissimilar forms which co-exist in the more recent deposits, no line of descent or of relationship could be established. Investigations of less recent date than those of Hyatt have afforded identical results. In his important work on the Foraminifera,[1] Terquem has shown the forms which are intermediate between types which at first glance seem very dissimilar; and Rupert Jones more recently, in his " Remarks on the Fora-

[1] *Recherches sur les Foraminifères du Lias du Département de la Moselle.* 1858-1866.

minifera, with especial reference to their Variability of Form, illustrated by the Cristellarians" (*Monthly Microscopical Journal*, 1876), has worked out the same matter, with the same general result.

Palaeontology, upon the whole, although yet very fragmentary and incomplete, testifies to the truth of evolution, showing an unmistakable line of descent from ancient types of life to more recent types, and from these more recent types to those which live now. I do not mean to say that in the case of every animal we are enabled to trace its ancestry with exactness to the most remote times, but in many cases this ancestry admits of being very satisfactorily traced, and, with the future progress of geology and palaeontology, many gaps will be filled up, and many connecting links discovered.

Among recent books—French books—well illustrating the preceding statements, I would recommend those which M. Gaudry, professor of palaeontology in the French Natural History Museum, and one of the leading evolutionists in France, has published, under the significant title of *Les Enchainements du Monde Animal dans les Temps Géologiques.* These three volumes are entirely devoted to the question of palaeontological descent, and are most ably written and reasoned.

Another argument for evolution is derived from the facts of embryology. Embryology is merely an evolution, and to study the development of any given organism is to study its evolution from a single elementary cell—the egg-cell—to the stage when this has become capable of leading an independent or semi-independent life, and has acquired a form and complexity of structure which are truly marvellous. In many cases this evolution lasts some weeks, some months at the longest, and the organism thus evolved merely needs to acquire larger dimensions by growth; but in many cases also there are breaks in the evolution process, and when one point is attained the process stops for some time, and is resumed later on. Such is the case in most butterflies where the evolution or development takes place in two or three periods, the adult period being singularly short, and sometimes hardly exceeding a few hours, during which reproduction is the only function accomplished, and, in fact, the butterfly stage of life has no other object than reproduction.

This process of evolution is a most marvellous one. While the brain—the minute speck of brain—of an ant may well cause the naturalist and thinker to wonder, by reason of the varied and complex acts it originates, the mere cell out of which a most complex organism with innumerable functions develops in the course of a few years, yielding a brain such as that of a Pascal, a Lavoisier, a Newton, a Goethe, a Shakespeare, a Pasteur, or a Darwin, becomes to the naturalist a subject of meditation still more extraordinary and astounding. This form of evolution is to be seen in all organisms, save in the simpler ones where no process of reproduction is present except mere division, and where the organism consists of mere cells—one or more—without any specialized organs and functions. There is a striking sameness in the development of animals of the same group, however much they may differ from each other when they attain the adult form. Such is the case, for instance, with many parasitic crustaceans. While the adult *Sacculina*, for example, is a mere mass of suctorial appendages converging towards an alimentary canal, and presents not a single one of the external characters of any adult crustacean, development shows the characteristic form of the crustacean larva, and no doubt can be felt as to the real nature, affinities, and systematic position of the degenerate adult, however unlike the general crustacean type it may be.

This individual evolution is named *ontogeny*, as all know, and evolutionary naturalists consider it as repeating, under a condensed and abridged form, the evolution of the species, or group, that is to say the *phylogeny* or palaeontological evolution. And while the study of the transitional phases in individual evolution shows the real relation between forms sometimes very dissimilar in adult age, it shows also the probable origin of the group or species under consideration. Why should a tadpole begin as a fish—having gills and the circulatory system belonging to fishes—although destined to become something very different from a fish, if there is not some intimate relationship between amphibians and fishes, if amphibians have not their origin in fishes, if amphibians are not transformed fishes ?

And, if we turn towards man, who, according to the evolutionary hypothesis, is no more than the last result of the evolution of higher vertebrates, we meet with facts identical in nature, but more surprising still. Mammals must be considered as having been evolved out of lower vertebrates, exactly as amphibians must have been evolved out of fishes, and as all vertebrates must, in different lines, have been evolved from fishes, man's development or embryology should retain some trace of this long and varied ancestry. And it does retain such traces ; this is a very plain and precise fact. Haeckel, in his *History of Natural Creation*, and in his *Anthropogenie*, has well summarized the facts bearing on this question, and it is useless to go over the details which are familiar to all. In the course of the few months during which the primitive egg-cell becomes evolved into a new-born child, the human organism offers unmistakable evidence of its animal ancestry down to the fishes themselves, as, for instance, in its temporary branchial slits and arches, in the primitive circulatory apparatus of the earlier stages of development, in the various forms which the central nervous system presents at various periods. The evolution of the circulatory apparatus is wonderful. At the beginning, during the first hour of evolution, the heart is a mere tube or bulb, exactly similar to the heart of the ascidians. Through some modifications, it then presents the typical aspect of the heart of mud fishes or Dipnoï. Later on, we meet with the condition persistent in adult amphibians; then follows a stage which corresponds to that of reptiles, and finally the heart corresponds to that of birds and mammals. The same process is to be seen in the evolution of the principal blood-vessels which are attached to the central organ of circulation, and the same stages are successively gone through. Classical as these facts may be, they may be briefly recalled, as their signification is of great weight. All fishes, it is well known, have a number of gill-arches on each side. In the amphioxus or lancelet, the lowest of known fish-like forms, there are very numerous slits, doubtfully homologous with those of true fishes, which have seven, five, four, or three. Their use is quite clear: the blood flows through the arches and the fringes they support, and thus becomes aerated. If we consider amphibians, we notice that the gill-arches and corresponding blood-vessels are retained in the tadpole, and do not wonder at it, since the tadpole, during its early life, is a gill-breather. But when we consider reptiles—a lizard for instance—we meet with the same vessels. Why? No reptile, at any time of its life, is a gill-breather, and the use of these vessels is not easy to understand. It cannot be said that they are useful to circulation, since the circulatory function is much more effective in birds or mammals, where these vessels are profoundly modified. And no explanation can be given except that reptiles are derived from amphibians and fishes, and have retained a large part of the anatomy of their ancestors. A closer study of the amphibians shows that this explanation is acceptable. When the gills shrivel and disappear, while lung respiration becomes established, the vessels do not entirely disappear: they remain and persist exactly as before: the gill-arches *minus* gills are known as aortic arches. The need of these aortic arches is gone; a much better circulation might be provided otherwise, but this would require a miracle, and as none occurs, we readily understand how it is that these arches persist; they have been useful and necessary, and their presence explains itself. So, then, if these aortic arches are present in the reptiles, we must interpret them as we have interpreted

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those of the frog, as having been useful at some time, when they were rendered efficient by the presence of gill-arches and gills, the only difference being in this last case, that they have been useful not some days or weeks or months ago, not in the same individual at an earlier stage of life, but in remote ancestors, and the remote ancestors are the amphibians, and, further still, the fishes. If any other intelligible explanation can be given of the presence of these aortic arches in reptiles, which never, at any stage of life, are gill-breathers, we certainly shall listen to it with great attention. But the argument does not stop here, and things may be pushed further still. Useless as circulatory organs, and useless as respiratory organs, these aortic arches are not limited to adult amphibians and reptiles ; we meet them in birds, in mammals, and even in man himself. At an early stage of their development the latter all have on the side of the neck several gill-slits and aortic arches. Will some creationist explain why these arches, most of which are destined to disappear, put in this temporary appearance? Evolutionists explain it as we have briefly pointed out : but creationists must explain in some way or other the temporary presence of these arches of which the larger part rapidly disappears, while the remainder goes to build the principal blood-vessels which originate in the heart.

The development of the central nervous system furnishes us with another important argument out of many in favour of evolution. The brain of man, during the development of the embryo, passes through a series of stages of increasing complexity, and a careful study shows that these stages, which are temporary in the embryo, are permanent in the principal groups of animals. One may easily detect in the evolution of the human brain a stage corresponding to that of the brain of fishes; but while the fishes permanently retain this brain-structure, an advance occurs in man, and the brain acquires the characters of the reptilian encephalon; later on it progresses again, and acquires bird characters, then mammalian characters, and finally it acquires those characters which are peculiar to mankind. Here again, ontogeny demonstrates phylogeny, and phylogeny, that is, derivation from the lower vertebrate forms, must be admitted to be true, unless some better explanation can be proposed.

Many other embryological facts do not admit of any explanation, if the hypothesis of derivation and descent is not admitted. For instance, on the special creation theory, why have baleen whales been provided with a full set of teeth which remain rudimentary, and soon disappear in the course of development, and which are never used nor even could be useful? Again, why are there pelvic bones in the whale, and even rudiments of the hind-limbs, when both are totally useless? Innumerable questions of the same sort might be and have been asked; but no satisfactory answer has yet been given by any creationist.

We may consider as belonging remotely to embryology, some pathological proofs of evolution of which a passing word may be said. I refer to those many cases, well known to the pathologist, of tumours, of fistulae, and of various malformations in many parts of the body, which are congenital, and are seen in the child from the moment of his birth, and cannot be ascribed to any disease or accident. Some of these cases are most curious and interesting to the evolutionist. A great number of them have been recently collected in an important work published by Professor Lannelongue and V. Ménard, under the title of *Affections Congénitales*.[1] In the first volume of this work—the only one yet published—the authors deal with the congenital malformations of the head and neck, and, to those who are not familiar with the evolutionary theory, it may seem astounding that such or such malformation of the neck or ears is due to the persistence of the fish or amphibian stage of development of these parts. Such is the case, however, and

[1] *Affections Congénitales*, vol. i. *Tête et Cou, Maladies des Bourgeons de l'Embryon, des Arcs branchiaux et de leurs Fentes.* Paris: Asselin et Houzeau. 1891.

there is no going against the facts of pathology which come to furnish an unexpected support to the theory of evolution. Other pathological, or, at least, abnormal, facts point in the same direction. Some years ago Dr. L. Testut, of Bordeaux, wrote[1] a large work on muscular anomalies in man. It is well known that there are frequent variations in the muscular system, muscles being sometimes differently attached, sometimes absent, while in many cases unusual muscles appear in the human organism. Have the persons who offer these abnormal conditions been specially created with these peculiarities? There is no reason for supposing that they originated by a different method from that with which we are all acquainted, and then what can the creationists say to explain these facts? The evolutionist appeals to descent, and does not much wonder at the occasional presence, in man, of muscles which exist permanently and constantly in other mammals. As Dr. Testut says, "When we consider the facts separately [the facts of muscular variation], we find, in short, that nearly all the muscular anomalies of man are normal dispositions in organisms which are inferior to him in the zoological scale." This means that no condition

[1] *Les Anomalies musculaires chez l'Homme expliquées par l'Anatomie comparée*, by L. Testut, Professor in the Medical School of Bordeaux. 8vo, 850 pages, 1884. Paris: G. Masson.

is exceptionally met with in man, which does not represent the normal condition in apes or in other animals, and this is a fact of great importance to the evolutionist. But it sorely tries the feelings of the creationist, who cannot explain the case, who cannot give any satisfactory reason for the presence, in that specially created creature, man, of muscles which typically belong to some other mammal, ape, bear, or hog, also specially created.

A third argument for evolution is offered by the facts of morphology. Morphology shows the unity of plan of quite different organs, as for instance, the arm of man, the fore-paw of the lion, the wing of the bat, the fin of the whale, and the wing of the bird; it shows that they are all made up of the same elements which are more or less modified in each case according to what is required from them. The same may be said of the numerous homologous organs in any large group: as for instance the mouth-parts of insects, which, although very different in their anatomy and also in their function, when considered in the different orders of insects, are easily seen to be identical fundamentally, whether the mouth is used for biting, for sucking, or for other purposes. Other organs in the same group, and sometimes in very large subdivisions of the animal kingdom, admit of being morphologically compared, and in many cases we find that organs which often subserve very different functions have a common origin, and are identical in despite of the modifications through which they have been adapted to their peculiar uses.

Such are, briefly stated, the general proofs of evolution, or at least the principal among them ; I must be content with this short statement.

Are these proofs satisfactory, are they convincing, and what do they demonstrate ?

To an impartial mind, they prove one thing to begin with, and it is that if we accept the creation theory, we must believe that creation has been going on through the whole series of past ages, and that every type of life has been specially created at some time or other, being in most, if not all cases, very similar to types which have lived before, and must also have been specially created. We must believe that the Creator while obeying a general tendency to progress, has first created some types of life which He, soon after, has diversified in various directions ; and that some of these types were doubtless of inferior order, since they have died out, while the types of new creation, the new species or varieties, have taken their place. But then, these new species also have proved inferior, and again new types have been created, or, again, without proving inferior, they have soon had new companions more perfect. Upon the whole, innumerable creations must have taken place, from the Cambrian to the Quaternary period, during millions of years, and it would seem as if the Creator has been trying to evolve out of each given type the greatest number of forms without altering the fundamental structure of the type Also it would seem as if the Creator evolved the higher types very slowly and gradually, through small modifications in various parts, by a sort of patching, an ever-mending and rearranging process, just as a man generally proceeds. Things stand, therefore, exactly as they should stand if the Creator had been unable to create immediately the desired form or types; if He had begun by inferior forms which required much alteration to attain the desired degree of efficiency. This inability to attain, from the first, the desired result, is very striking; palaeontology amply illustrates it, and embryology also; and to many it may seem surprising, while evolutionists, and believers in natural selection, do not wonder at it.

Palaeontology and embryology therefore, while not disproving the creation theory, render it rather unintelligible to our reason, while they display facts which seem very intelligible upon evolutionary views.

But can palaeontology and embryology, and all the other facts appealed to by evolutionists, disprove special creation, and establish the evolution theory on a firm basis? Can we consider the doctrine of the transmutation of species as firmly established, as demonstrated by fact in an unmistakable manner? Certainly not. Evolutionists are convinced of the truth of their doctrine, they can point to a number of facts which fit with it, but they cannot give the required demonstration. The situation of the creationists is different. If they accept the view—and they must do so—that every species and variety has been specially created, they may say that things stand as they ought to, if special creation has existed; and as none of them claim that special creation is going on now we cannot ask of them to show us a creation of that sort. On the other hand, evolutionists cannot claim that evolution is a process of the past. They believe in its present existence, not only in organic structure, but in mental organization, and also in the inorganic world, and they point to the facts of psychology, zoology, and astronomy, as illustrating the process of evolution. And creationists may rightly demand of them to show precise and unmistakable instances of transmutation.

Are evolutionists prepared to meet this difficulty, this requirement? They may answer that the astronomical facts are not under their control, and that an enormous amount of time is required to yield a single instance of evolution, so that all they can do is to note the present condition of things, and let our descendants do the same and draw their conclusions. So far as psychology is concerned, they may answer that proofs of individual evolution are to be seen every day, and that mental evolution is a positive fact in every individual man, and in the animal kingdom as a whole. And as concerns zoology, they may reply that innumerable facts point to descent and evolution. But the creationists may object to this argument, and say; if species are really evolved from each other—and the case of species is only a very small point in the question—you must show us species arising, by evolution, from former species. In many palaeontological cases we do not find the connecting links of which you assume the existence—in fact, it may be said with truth that their existence is not always required—and, especially, we have not yet seen a new species originate from a preceding one. Show us this, show us a positive case of transmutation through natural means, such as may and do operate under natural conditions, show us a species becoming a new one, *hitherto unknown*,[1] and we will believe in evolution. Such is the answer of creationists.

It might be discussed whether this argument is not

[1] This requirement is necessary to preclude all objection which might be raised—with reason—from the possibility of normal dimorphism.

of the most dangerous sort, more especially for creationists, whether there are not serious inconveniences in refusing to believe in that which cannot be demonstrated by actual, precise, visible and tangible fact. But this point had better be left out, and we will accept the reply of the creationists as it stands.

They ask for a proof of transmutation: we must secure that proof and meet their demand. How so? Through direct experiment, through experimental transformism. The notion is not exactly a recent one, but in the present debate it represents the only line along which we may expect to discover the positive facts which are necessary. As Buffon has said, "Man will never be conscious enough of nature's power, nor of his power on nature." And this statement I believe to be positively true. The only thing to be done, at all events, is to subject the notion to the only possible test of which it admits, and to begin experiments.

I have just said that the notion is not of recent origin. The fact is that we find it clearly expressed in the *Nova Atlantis*, where Bacon advises experimental investigations for the purpose of discovering how the environment reacts on living organisms and forms species. But the most authorized defender of experimental transformism has surely been Isidore Geoffroy Saint Hilaire, and many passages concerning this matter might be quoted from his *Histoire naturelle générale des Règnes organiques*, and his *Influence du Monde ambiant*, etc. One will be enough, "Since Nature," he says," left to herself never allows us to witness considerable modifications in the conditions of life, it is clear that only one way is open to us if we wish to perceive such modifications and to examine their effects on the organism; we must oblige Nature to perform that which she would not spontaneously accomplish." (*Hist. Nat. Gén.* iii. p. 389.)

This is exactly what we require. While facts of observation are sufficiently numerous to give us a fair idea of the amount of natural variability and variation —although much may yet be done to give an adequate notion of the amount of this variability—we require to extend our knowledge concerning the causes of variability (the natural causes, of course), and to discover in what manner, and to what extent they do operate. We are already acquainted with some of these causes, and we know that by selection, crossings, modified environment, much has been done. But still more can be done, and in experimental transformism lies the only test which we can apply to the evolutionary theory. We must use all the methods we are acquainted with, and also those, yet unknown, which cannot fail to disclose themselves when we begin a thorough investigation of the matter, and do our utmost to bring about the transmutation of any species. We do not specially desire to transform any one species into another known at present; we wish to transform it into a new species. And this is necessary, if we do not wish to remain open to an objection suggested by the facts of dimorphism. Many species occur in two or more forms, sometimes very different, and if we were merely to transmute one species into another, it might be said that we had mistaken the two forms of a dimorphic species for two different species, and then our attempt would be useless to a large extent.

Experimental transformism is what we need now, and therein lies the only method we can use.

But it must be demonstrated that this test is available, and it remains to show what are the facts which lie at its basis, and what are the methods to be used.

LECTURE II

Summary—Experimental Evolution based on Three Groups of Facts—First Group : the Facts of Natural or Spontaneous Variation : Organisms are not rigid structures, but exhibit much plasticity. —Facts of Variation in Colour, correlated sometimes, and perhaps always, with variation of chemical composition (Armand Gautier's investigations) ; Variation in Dimensions and Experiments on the real cause of this Variation, Semper and the Author ; Variation in Integuments, Form, Shape of Fruits and Leaves, Flowers — Penzig's *Pflanzen-Teratologie* — Skeleton, Muscles, Internal Organs and Viscera ; Sexuality—Camerano's Neotenia.

THREE groups of facts lie at the basis of experimental transformism and display at the same time its conditions and its methods. The first, and most important, comprises the facts which illustrate variability in the state of nature, natural or spontaneous variability. Spontaneous, we call it, but in fact we use the word only because we are ignorant of the real and positive causes of this variability. The second group includes the facts of variation under domestication and culture ; the third, the facts illustrating the direct influence of environment as a factor of modification and transformation. These three groups of facts require to be briefly stated in order to show how experimental transformism must be carried out. Of course it is of much importance to prove that living organisms display a marked tendency to vary, under natural conditions, in most of their parts, in a more or less marked degree. For this natural variability[1] is that which

[1] Cornevin (*Traité de Zootechnie générale*, 1891, p. 226) establishes the following list of the modes of variation among domestic animals :

I. Morphological variations.

Variations through disappearance.	Absence of horns, ears, hair, pigment, etc.
— — arrested development.	Total. Dwarfing, discolouration. Partial. Niatism, partial discolouration, reduction in the number of limbs, etc.
— — juxtaposition.	Is seen in some hybrids when the characters of both progenitors coexist side by side.
— — fusion.	Diminished number of ribs, teeth, digits, vertebrae, etc.
— — transformation.	Wool replaced by hair ; scales replaced by feathers, etc.
— — hypertrophy.	Total. Giants, melanism, extreme hairiness. Partial. Drooping ears ; very long horns, hairs or feathers of unusual length.
— — division or repetition.	Supplementary vertebrae ribs, teeth, horns, digits, etc.

has allowed natural selection to operate, and allows us to expect to push things further in the way of direct experiment.

I do not intend to recall here all the facts which have been quoted by a large number of naturalists, up to the present day. I shall merely call attention to the most important of them, using again preferably, as they may be less familiar to my hearers, those which I have been able to collect from French sources.

One of the variable characters in most living beings is colour,—in most, not in all, for there is among the human races a strong tendency to the preservation of the race-colour, while among animals, and especially plants, colour varies a great deal. And this is the reason why Linnaeus wrote his ever-quoted *nimium*

II. Physiological variations.

Variations	through	diminished	activity.	Lateness of development; enfeeblement of sexual tendencies; sluggishness.
—	—	earlier	—	Precocity.
—	—	exaggerated	—	Increase in fertility, etc.
—	—	stronger	—	Vigour; immunity from diseases, etc.

To this list, as I shall show later on, we must make an important addition in Group II., and add what I propose to call physiological or chemical variation, although it differs entirely from the sort of variation included by Cornevin under the same name *ne crede colori.* It may be noticed here that persons who want their supposed good sayings to travel far and long, should always say them in Latin; if Linnaeus had written the four words above in good sensible English, or in clear French, his saying, which seems to be a sort of divinely inspired axiom for many, would never have met with the success it has obtained, and it would have been better. Perhaps the fact that these words are a quotation from Virgil (*Eclogues*, ii. 17) has something to do with the matter. Accepting this dictum, many have considered colour as of no importance in the organism, whereas in many cases it is demonstrably of high import.[1] And, as we shall see further on, variations in colour cannot be considered as mere freaks of nature, however abundant they may be, for where colour varies, there is also a more or less pronounced variation in other characters, and more especially in some interior and less easily appreciated characters of chemical nature. And no one can dispute the import of chemical characters, when one knows the influence of chemical media on most organisms. So, while recognizing with Linnaeus that colour is certainly in many cases a very variable character, I would refrain from repeating after so many others *nimium ne crede colori.* For colour is a specific

[1] Concerning the uses of colour see especially A. R. Wallace's *Darwinism*, which contains an excellent account of the matter.

character, or at least it must be one to most of the present systematic naturalists (though I doubt if this state of things is eternal), and as evolutionists, we cannot allow this character to be lightly disposed of, when it is precisely one of the most variable in some cases.[1] In some cases, I repeat, not in all. For while some instances of colour variation may be observed in the state of nature among animals or plants, while some special variations are more especially met with, such as albinism and melanochroism, it must be noted that colour variations are especially frequent among cultivated plants and domestic animals, and are thus due to the results of changing environment. We do not always perceive how far there may be a change in environment, but colour variations show that it exists in many cases where we do not readily detect it.

Among animals in their natural condition, colour variations are of no very rare occurrence. It is known that the common fox in the same country offers marked variations in colour, which are illustrated by the different names which have been conferred upon the principal forms; *Vulpes alopex*, *melanogaster*, and *crucigera*. The beaver also offers important colour

[1] Of course facts concerning colour variation are to be found in a large amount of works. But I would recommend, concerning colour variation in insects, two recent works. The one is Mr. S. H. Scudder's magnificent work on the *Butterflies of New England*; the other is the *Entomologist's Record and Journal of Variation.*

variations, and its fur is in some cases of much lighter colour, in others, of deeper. Colour variations are of no scarce occurrence among insects and fishes, and in a recent number of the *Entomologist's Record and Journal of Variation*[1] an interesting coloured plate may be seen, illustrating more vividly than whole volumes of description, the colour variations which Mr. J. A. Clark has met with among the British species of *Smerinthus*.

Lacordaire records similar facts concerning the *Sphinx elpenor*, Audouin has some concerning *Pyralis vitis*, and Dugès concerning *Phasma*. Among insects, again, Hulst has noticed a large amount of variation. From one and the same *Arctia excelsa* he has obtained a number of eggs and larvae which have yielded adult butterflies belonging to eight or nine different varieties —or, to speak correctly, possessing eight or nine different *specific* names (*Arctia phalerata*, *pallida*, *phyllina*, *flammea*, *decorata*, *nais*, etc.). Of course this merely shows that the makers of these species were wrong in establishing species where mere varieties exist—if even varieties may be spoken of in this case—but does this not show also that variation may be very important?[2] The common cray-fish is a well-

[1] Edited by J. W. Tutt, London. March 16th, 1891.

[2] *Cf.* Hulst: *Variation in the Arctias. American Naturalist*, 1884, p. 193.

known instance of colour variation, being generally brown, but sometimes blue, and even red in its living state. Leeches offer a large amount of colour-variation ; most Helices do the same, and in fact, it may be said that in all groups of animals variations are met with in the colour of their garment. I refer here merely to occasional variations, for it is well known that a large number of mammals, birds, and other animals offer periodical or seasonal colour variations, especially in northern climates, being brown or grey during the summer, and becoming white during the winter. Such seasonal variations Wallace, in his recent and excellent book on *Darwinism*, ascribes to natural selection and to protective necessities. Very numerous instances thereof might be adduced ; and Godron in his *De l'Espèce et des Races dans les Êtres organisés* (1859, two volumes), gives a list—which might be extended of course—of the mammals and birds and other animals which show this seasonal variation, and also a list of animals which offer instances of albinism, melanism, and erythrism. But I may be allowed to refer to Wallace's *Darwinism* for all seasonal colour variations, and for the investigation of the use and origin of colour generally. As the last named cases of colour variation, such as albinism and melanism, cannot be interpreted in a quite satisfactory manner, we had better leave them out. That which most interests us, so far as colour variation is concerned, is the evidence showing that a change of environment causes a change in coloration. Some instances may be adduced: for instance, Gérard states in the *Dictionnaire d'Histoire naturelle* of D'Orbigny[1] that when the small brown honey bees from High Burgundy are transported into Bresse—although not very distant—they soon become larger and assume a yellow colour; this happens even in the second generation. The same author gives some instances from the vegetable kingdom. As he rightly remarks, the roots of beet, carrot, radish, and other plants, are colourless in the wild and natural state, and as soon as they are subjected to the process of culture they become red, or yellow, etc. and Vilmorin in his *Notice sur l'Amélioration de la Carotte sauvage*, originally published in the *Transactions of the Horticultural Society* (1840), has noted the same fact, the red and yellow colours, as well as a peculiar violet hue which has not been permanent, appearing only in cultivated carrots after some time their appearance being at first irregular and transitory.

Moquin Tandon[2] records some instances of change in colour which are due to the influence of environmental change. For instance, he has seen gentians which are blue in valleys become white in the

[1] Article *Espèce*. [2] *Eléments de Tératologie végétale.*

mountains. Similarly *Oxytropis montana* and *Trifolium pratense* are white in the Alps and Pyrenees, and *Geranium batrachoïdes*, which is commonly bluish, becomes variegated, and turns generally white when it grows in unpropitious soil. There are white varieties of many plants, such as *Lamium purpureum* and *Erica vulgaris*, while *Verbascum lychnis*, and *Campanula Trachelium* bear flowers which are blue, violet, or white, according to circumstances.

Such instances might be given by hundreds, as is well known. In some cases it would seem that the influence of environment is very plain, although difficult to explain, for there are places where some natural colours of plants or animals disappear soon, and are replaced by lighter tints, or in many cases by white. M. d'Apchier de Pruns records the fact as having been noticed by himself on his own land, and it seems that at Brassac les Mines, in central France, while oxen become of lighter hue, and pheasants, pigeons, ducks, &c., have more or less white feathers, plants with variegated leaves soon become uniformly green.[1] And some horticulturists and amateurs have complained, similarly, of their garden or grounds, saying that they find it impossible to keep variegated plants—for all return to the ordinary type. The causes of these facts are difficult to ascertain, as the circumstances

[1] *Revue Horticole*, 1883, p. 316.

which determine variegations are themselves not known; but the facts are numerous, well authenticated, and must be taken into account. Climate certainly has some influence on the colour of flowers, and although we do not exactly know yet what we mean when we speak of differences of climate, as Naudin aptly remarks, and though climate includes a large number of very different factors which are combined in different proportions according to localities, there are influences which may be ascribed to it, *in toto*. G. Bonnier and C. Flahault have performed interesting experiments on this point. G. Bonnier[1] has compared flowers of the same species and age, from different altitudes, in the Austrian Alps and Carpathians, and the result has been that while some plants, such as *Rosa alpina* and *Erigeron alpinus*, have the same colour at different heights, others are slightly different: such is the case with *Thymus serpyllum* and *Geranium sylvaticum*; others are very different, such as *Myosotis sylvatica*, *Campanula rotundifolia*, *Ranunculus sylvaticus*, *Galium cruciatum*. Of course the colour is not radically changed a pink flower does not become yellow, but it grows deeper and richer in plants of higher altitude. Microscopical investigation shows that the pigment

[1] *De la Variation avec l'Altitude des Matières colorées des Fleurs chez une même Espèce végétale.* *Bull. Soc. Botanique*, 1880, . 103.

granules are more numerous in the flowers from high altitudes. C. Flahault's[1] experiments are more conclusive, and the conditions under which they have been performed are more satisfactory. His experiments have been made on plants grown in Upsala and in Paris from seeds of same origin. One half of the Parisian seed has been sown in Paris, and the other in Upsala; one half of the Upsala seed has been sown in Paris, and the other in Upsala. With the two experiments the result has been the same, the flowers have always been more vividly coloured in Upsala than in Paris, and the same holds good when flowers of plants spontaneously growing around Paris and around Upsala are compared. In some cases, however, there is but a very slight difference. M. Flahault has had the exact colours represented in his paper, and the comparison of the Upsala and Paris flowers is thus shown to the reader as if he had the flowers themselves.

Concerning colour variation in animals, I must be content with calling attention to some principal facts. One is, that while animals in their natural wild state offer but very slight colour variations in the same region, these variations become very numerous under

[1] *Nouvelles Observations sur les Modifications des Végétaux suivant les Conditions physiques du Milieu. Annales des Sci. Nat.* (*Bot.*) t. ix. 1880, p. 159.

domestication. Of this, horses, oxen, cats, rabbits, guinea-pigs, &c., are instances. And this may be explained by natural selection, at least if colour is always of positive use in some way or other to animals, in escaping dangers which are of daily occurrence in the wild state of life, but which disappear under domestication. Under domestication colour variations, to which a more or less marked tendency may always exist, are of no inconvenience, unless positively repelled by artificial selection, and thus such variations are often present. On this point I may refer to the works of Darwin and Wallace.

Another fact to be taken into account is that of the influence of food on colour. Many bird-fanciers think that by appropriate colour-feeding, as they call it, they can help the production or intensification of colours. For instance, they believe that canary birds can be made to become of a bright yellow when fed with egg, mustard seed, curcuma powder, saffron water, and alcohol, in definite proportions; they even consider it useful to put yellow flowers around the bird's cage. But exact experiments, scientifically conducted, are yet wanting on this subject, as I have but second-hand and rather untrustworthy information concerning the investigations conducted by Dr. Sauermann, which are alluded to in the previous sentence.

A third fact, which must be noticed here, is the positive influence of the colour of environment on that of animals. Mr. E. B. Poulton has recently studied this matter, showing, in his important memoir published in 1887, that many lepidopterous larvae are strongly affected by the surrounding colour. The plates which accompany his memoir illustrate the fact very precisely.

If we consider those freaks of colour which are familiar to horticulturists under the name of variegations, some interesting facts may be noticed. It has been often questioned whether variegations are not pathological symptoms, and whether variegated plants are not more or less diseased. M. E. A. Carrière[1] has carefully considered the matter, and his considerable horticultural experience does not make him feel inclined to consider variegated plants as being diseased at all. It even seems that in many cases, variegated forms are healthier and stronger than the non-variegated: for instance, the variegated *Euonymus* of the Duke of Anjou variety. There is however one fact which has been noticed concerning variegations—it is the impossibility of maintaining such plants with their variegation in some localities. Many horticulturists have recorded the fact; and while some com-

[1] *Les Panachures sont elles des Maladies? Revue Horticole*, 1884, p. 198.

plain that all variegated plants, when grown on their grounds, soon revert to the ordinary type, even when they belong to the most stable varieties, others notice that their garden seems very propitious to the production of variegations. There is some unknown influence at work in these cases, and experiments might show what it is.

But it certainly seems that variegated plants cannot be considered as diseased, and M. Lebas[1] says positively that *Euonymus sulfurea*, *Euonymus radicans variegata*, and *Thujopsis dolabrata variegata* are certainly stronger and hardier than the common non-variegated varieties; and, on the other hand, MM. Carrière and André[2] notice that while *Aspidistra elatior variegata* has a strong tendency, in most places, to revert to the non-variegated type, there are places where it remains quite constant, and where even non-variegated forms become spontaneously variegated. In some cases variegation comes on slowly, and Vilmorin[3] has studied the process with care, but in others it comes on all of a sudden. Carrière[4] has noticed a case of this sort in a garden where thousands of celery plants were growing, and

[1] *De quelques Fusains du Japon à Feuilles panachées. Rev. Horticole*, 1872, p. 139.

[2] *Revue Horticole*, 1888, p. 124.

[3] *Sur les Panachures des Fleurs. Ibid.* 1852, p. 128.

[4] *Panachure du Céleri. Rev. Horticole*, 1882, p. 541.

where all, in a more or less marked degree, at the same time became variegated. This fact, with others which might be quoted, goes to show that variegations depend on some environmental influence.

Colour variations may, however, be noticed in cases where no environmental influence can, as yet, be traced. Every one has seen cases where the same rose-bush yields flowers of dissimilar colours. Carrière and André,[1] to take an instance among many, have noticed a rose of the Mabel Morison variety carrying white flowers and one single pink one. The branch bearing the pink flower has been grafted on another bush, and it has maintained its special character, yielding always pink roses. Such cases are not of rare occurrence. But how can we understand the cause of this variation? Environmental influence seems out of the question, and we are at a loss to account for this important variation.

Similar colour variations are often noticed in fruits, and have often been recorded in connection with grapes. Carrière has quoted a case of this sort, and given a good coloured plate showing well how things stand. In the same bunch of grapes some are black or red, some colourless, and many variegated in different manners. It may seem that these cases are

[1] *Cas de Dichroïsme dans la Floraison d'un Rosier. Rev. Horticole*, 1888, p. 74.

of slight importance, and that I am losing much time in bringing them before you. Truly, if we were to view the things in the light of Linnaeus—*nimium ne crede colori*—such would be the case; but recent facts have gone to show that colour variation is not merely what it seems to be, a variation in pigment deposits; there are more important variations which underlie or accompany it, and these are of much interest in showing that colour variation is of greater import than might be thought at first glance. These variations are of chemical order, and, so far as I know, little has yet been done to investigate this delicate matter beyond a short but excellent paper by Professor Armand Gautier.[1] The author begins by recalling some instances which have a remote relation to this matter, and some of which are familiar to all. It is known, for instance, that when a mare has once given birth to a mule, it may happen that in after years her foals, produced after the usual impregnation from a stallion, present peculiarities which belong to asses. It seems that the single fecundation by the ass has in some manner impregnated the whole maternal organism upon which it has stamped itself. Similar cases are to be observed in the human race as well as among horses or dogs, and these show that a foreign

[1] *Du Mécanisme de la Variation des Êtres vivants, &c.*, in *Hommage à Monsieur Chevreul à l'Occasion de son Centenaire. F. Alcan.* 1886.

influence, which determines no variation at all in the maternal organism, has, however, effected a deep change on the germ-cells, and has operated on an important part of the organism. Instances of the same sort may be met with among plants. It happens, for instance, that after a branch of a variegated variety has been grafted on a non-variegated plant, some variegated branches sprout from the latter. And this shows, as M. Armand Gautier contends, and as all naturalists must admit, that race-variation, or, generally speaking, variation of any sort, or of any importance, is not a mere external fact—a mere external modification—but that the modification makes itself felt in the utmost depths and intimacy of the cells. Otherwise stated, there is not only a mere difference of form; underlying the formal or external difference, there are modifications of much greater importance in the chemistry or physiology of the cell-plasma. These differences may be localized instead of being general. For instance, the same orange-tree may bear oranges and lemons, if some flowers have been impregnated with pollen from lemon-trees; and it may happen even that the variation is more localized still when one half of the fruit is orange and the other lemon—when, as Naudin has seen, one half is *Datura stramonium*, the other *D. laevis*. Of course, by reasoning we also come to the conclusion that there must be physiological or chemical differences accompanying the anatomical or morphological variations. But Prof. Gautier has tried to appreciate these differences, and to measure them in some way. Notwithstanding the Linnaean axiom concerning colour, and the fact that many consider colour variation as of very little import because it is so frequent, Gautier has studied with great care the intimate changes which accompany colour variation. The common grape has been selected by him as his subject, all varieties of grape being varieties of one common stock, however different they may be in many respects ; and after a careful investigation of the colouring matters, he has come to the conclusion that, although they all belong to the same type, and have all been considered as one and the same, they offer important differences. For instance, while some of the pigments are soluble in water, others are not ; while some yield green precipitate with lead salts, others yield a blue one ; while some contain nitrogen, others do not. For instance, again, grapes of the *Carignan* variety contain a colouring matter of the type $C_{21}H_{20}O_{10}$, and another which contains $C_{22}H_{24}O_{10}$. The *Grenache* variety contains a different matter, $C_{23}H_{22}O_{10}$; *Aramon* contains $C_{23}H_{18}O_{10}$; *Teinturier* answers to $C_{22}H_{20}O_{10}$, *Petit-Bouschet* to $C_{45}H_{36}O_{20}$, and *Gamay* to $C_{20}H_{20}O_{10}$. This shows that the slight external differences which are peculiar to each sort or variety, derived from the common stock by selection and culture, are accompanied by important chemical variations. The above-mentioned differences illustrate the fact. It must be added, also, that other differences are to be met with—differences in the amount and nature of sugar, tannin, &c.

These results determined Prof. Gautier to investigate an important matter which is closely related to the foregoing topics, and he has examined the colouring-matter of hybrid forms. For instance, *Petit-Bouschet* is the result of the impregnation of *Teinturier* by *Aramon* pollen; and the question to be solved is the following: Is *Petit-Bouschet* colouring-matter identical with that of one of its parents, or intermediate between both, or is it entirely different? Direct analysis shows that the second supposition is true. *Petit-Bouschet* colouring-matter is nearly exactly intermediate between those of its parents, and its composition is the arithmetical mean between those of the two stocks from which it is derived. Another important fact which has been pointed out by Gautier is that all colouring-matter which may be found in grapes, when subjected to the influence of potash, splits into two sorts of chemical compounds; two of them are constant, and invariably met with—phloroglucin and protocatechuic acid; the other is variable, and consists of potash united to some fatty acid, so that while in all cases phloroglucin and protocatechuic acid are present, the third element varies, and these variations are the cause of the peculiarities which characterize each grape colouring-matter; or at least, if they are not the cause, they are concomitant phenomena. Similar facts have been ascertained by Prof. Gautier concerning a chemical substance which is very abundant in nature, and which would seem to be constant and invariable. Chlorophyll, according to his investigations, is not identical among Acotyledons, Monocotyledons, and Dicotyledons; there are marked chemical differences, marked variations in chemical constitution, and even between two species of two families (Spinach and Mallow) there is a difference. Of course these variations are not very intense, and the different sorts of chlorophyll are always homologous, but they are not identical. Similar instances are to be met with among animals. While we commonly speak of albumen, ossein, syntonin, and other compounds of animal tissues, as being constant and identical, direct analysis and investigation show that all these substances, when considered among different animals, do present common characters, which are permanent and ubiquitous; all possess also special features which distinguish them from the similar compounds. Albumen is not the same in all eggs, for instance, in those of the hen, turkey, and duck; and the flesh of fishes is different from that of mammals or birds. It even follows from Prof. Gautier's experiments that there are differences in the same animal at different times of life, and differences according to the mode of life and nutrition. For instance, if the meat of oxen, fed in the ordinary manner upon hay and grass in the pasture, is compared with that of oxen which have been fed and rapidly fattened with the refuse of beet-sugar factories, as is often done in France and Germany, there is not only a difference in the taste and flavour of the steaks or roasts, there is also a chemical difference which is easily detected. While the flesh of the pasture-oxen rapidly dissolves in water and hydrochloric acid, that of the oxen fed on beet-refuse dissolves very slowly and incompletely; the greater part does not become transformed into syntonin, and in this respect it resembles the flesh of calves, veal being very refractory to the action of dilute hydrochloric acid.

I may have been dwelling rather a long time on M. Gautier's experiments, but they seem to me very interesting and suggestive. Of course we do not yet know anything about the cause of variation, but it is a great deal to have some information concerning the phenomena which accompany it, and especially the chemical and biological phenomena, and it is important also to be able to detect such phenomena even in cases where no external or morphological differences are obvious. Later on, I shall have to revert to these matters, and for the present it is sufficient to show that even in cases where the external variations seem so weak as to be generally considered of no real account by naturalists, very marked chemical variations underlie the slight morphological differences.

Upon the whole, then, if a steady and careful investigation were made of the chemical differences which accompany colour variation, doubtless we should see that the latter is of great importance.

Concerning the causes of colour variation very little is known. Mr. A. R. Wallace has given a detailed discussion of the matter in his recent book on *Darwinism*, and emphasizes the protective value of colours and their consequent relation to natural selection. This factor has doubtless been of much importance, but on the other hand, as Wallace indeed admits, colour variations have much to do also with the general constitution, and some relationship between vigour, or weakness, and colour is commonly recognized. In 1823, Heusinger already contended that the quantity of colour in animals is subject to two laws:

the first of which is that colour is more abundant when the genital functions are most active; and the second, that colour is also in relation with the fatty layer underlying the skin, being less abundant when this layer is thicker, and *vice versâ*. Recent observations, and the general experience of breeders, go to show that there is much truth in this view, as the breeders have remarked that the best breeds for flesh and fat—the Southdown for instance—have less colour than the other breeds of the same origin; and the same obtains among cattle, and among domestic birds, where the white, or faintly coloured individuals are known to be less vigorous than the others. It is also commonly observed that the cause which induces albinism generally affects reproductive functions in a marked manner, and that partial or total sterility is induced: in fact one may say that the absence of pigment and the sterility are correlated expressions of the same constitutional change. Such is the case, for instance, with the Fredericksberg breed of horses. These horses are very peculiar in their colour, which is pure white, although they are not albinos. They are sterile *inter se*, and in order to preserve the breed, which is used by the royal family of Denmark, they are mated with grey or black mares, and among the progeny pure white horses are obtained. The sterility between like and like is not complete, as in some cases a progeny is obtained, but the latter is deficient and lacks vitality.[1] As a last proof of the relation between vigour and colour, we may notice the fact that according to many authors, among them Settegast, Heusinger, and Wyman, animals deeply coloured, animals of black coats for instance, are well able to withstand the influence of poisonous plants, which kill others less coloured. There thus seems to exist a positive relationship between vigour and colour, and this cannot be wondered at, since colour is the result of chemical processes which are carried on in the organism, and since this metabolism certainly varies in intensity and rapidity in different individuals. Other agencies, which may operate on vigour, exert an influence on colour; light is such an agency, but facts seem somewhat contradictory, and we cannot wonder at this when we consider that while light varies, other influences, such as heat, may not vary, or may vary in a different direction. The climate is also operative —although we can certainly not give a precise definition of this term—and an interesting case is that of a herd of Dishleys living in the vicinity of the sea, in France. These Dishleys were always spotted on the face and ears, and had large black spots which rather discouraged buyers. The owner said these spots were

[1] Tisserand: *Études économiques sur le Danemark, le Holstein, et le Slesvig.*

the result of the climate of the sea-shore, and in fact, the descendants of these spotted animals bred inland, at Lyons for instance, were always pure white.[1]

So much, then, for colour variation. But animals and plants vary in many other respects, and one of the most familiar examples is that of dimensional differences.

I am not aware that any naturalist has said—unless in current English or French, happily not in a Latin aphorism—that dimensional differences are of small importance; but I presume many think so. Variations of this sort are very frequent, and although it is possible, by selection of extreme variations, to create varieties of giant or of dwarfed plants for instance, as any horticulturist may testify, one might rightly consider this sort of variation as more secondary than most others, if it could not be shown that, while dimensions vary, other variations are present at the same time, which may very well be of high importance. These dimensional variations are in a large measure correlated with external influences. Darwin has shown how, among horses, the dimensions decrease in northern latitudes, in islands, and on mountains. Man also is smaller in extreme northern climates, and all the organisms of extreme northern parts tend to be small. It may be, as Alcide d'Orbigny

[1] Fact quoted by Cornevin: *Traité de Zootechnie*, p. 278.

believes (*Voyage dans l'Amérique méridionale*, t. iv.), that cold on one hand, and decrease of pressure on the other, exert an unfavourable influence on growth. But certainly food has a great deal to do with dimensional variations. When food is abundant, and easy to get, animals and man are prosperous and attain large dimensions, while when it is scarce they remain smaller. Japanese horticulturists rely in part on this influence of the scarcity of food in their process for the dwarfing of plants. Most persons have seen—or at least heard of—these diminutive plants of theirs, mostly conifers, such as *Thuja*, *Juniperus*, etc., which, while aged 40, 60, 80, 100, or 150 years, are often much less than a yard high, although their relative proportions are well preserved, so that when you look at them it is exactly as if you were looking at a normal large tree through the wrong end of a glass. These dwarfs are the result, in part, of mechanical processes which prevent the spreading of branches, and in part, of a starving process which consists in cutting most roots, and in keeping the plant in poor soil. Many of these Japanese dwarfs may be seen in Europe, and they well illustrate the influence of external conditions on growth and dimensions. Numerous instances show that plants or animals transferred from unfavourable to favourable conditions, or *vice versâ*, acquire larger dimensions, or, on the contrary become smaller. Such differences may be noticed among all sorts of animals, from the highest to the lowest. Gérard has noticed that bees transferred from Burgundy to Bresse become larger in a generation ; A. H. Curtiss has seen, in some places near the Potomac, *Bidens cernua* acquire a height which is six times the common average height of this plant, and he has seen the same in *Oxalis stricta ;* C. Lemaire states in D'Orbigny's Dictionary that, while cultivated hemp grows no higher than a metre and a half in France, in Piedmont it attains three and four metres ; and if Italian stock is planted in France it rapidly reverts to the small variety, in the course of two or three years. Speaking of horses and of their dimensional differences according to climate and environment, De Quatrefages expresses himself as follows: "These contrasts may be interpreted as due to the influence which must be exerted on the first-named [Corsican and Pyrenean stocks] by the stimulating and dry air of the mountains, the frugal food with which they must often be content, and, doubtless also, the hard exercise which is rendered necessary by the roughness of the soil. The others, on the contrary, [he refers to the large heavy horses of the Bresse province,] always immersed in a moist and heavy atmosphere, over-fed with watery plants, and having none but easy work to perform, must surely feel the effects of an environment whose influence exerts itself even on plants." That such is the case, and that the influence of environment on dimensions is a very direct one, is amply shown by the results of a change. Horses and oxen become larger when transferred from Brittany to Normandy, while the reverse happens in the reverse case, for when some oxen were sent from Poitou to Brittany, at the third generation the first named race had acquired all the characters of the Breton stock.

Generally speaking, insular animals are smaller than their continental congeners. In the Canary Islands the oxen of one of the smallest islands are much smaller than those of the others, although all belong to the same breed, and the horses are also smaller, and the indigenous inhabitants are in the same case, although belonging to a tall race. It would seem that in Malta elephants were very small —fossil elephants of course—and that during the Roman period the island was noted for a dwarf breed of dogs, which was named after their birthplace, according to Strabo. In Corsica also horses and oxen are very small, and *Cervus corsicanus*, the indigenous deer, is quite reduced in dimensions, although, according to Polybius, this species was imported from Europe 2,000 years ago, which makes it a descendant of our *Cervus elaphus*; and lastly the small dimensions of the Falkland horses—imported from Spain in 1764

—are familiar to all. The dwarf rabbits of Porto Santo described by Darwin may also be cited as a case in point. Dimensional variations in wild animals are very numerous, and Locard (*Études sur les Variations malacologiques d'après la Faune vivante et fossile de la partie centrale du Bassin du Rhône*) notes among a large number of similar cases, the fact that many molluscs—land or water—common to France and Algeria, are much larger in Africa, where their dimensions are double those of their European congeners.[1] Isidore Geoffroy Saint Hilaire says that *Lymnæa stagnalis* is much larger in ponds than in rivers. Moquin-Tandon notes that in the same country the same species of molluscs exhibits important dimensional variations, and he has seen *Bulimus decollatus* nineteen times larger in Africa than in Europe.

Through careful selection these dimensional variations may become permanent, especially if no change

[1] Such is the case particularly with *Helix aspersa, vermiculata, lactea, melanostoma, Leucochroa candidissima, Physa contorta*, and many others. And when *Leucochroa*, for instance, is transferred from Algeria to France it does not acquire a length of more than one centimetre, while in its African home it is two or three centimetres long. *Cf.* Locard: *L'Influence des Milieux sur le Développement des Mollusques. Société d'Agriculture, Histoire Naturelle, et Arts Utiles de Lyon*, 1891. It has also been issued in pamphlet form by J. B. Baillière, Paris, 1892. A large amount of facts of French origin are quoted in this valuable contribution to the subject, and the author is one of the leading malacologists.

occurs in the environment. M. A. Roujon, of Clermont-Ferrand,[1] by selecting the central, smaller seeds of dwarfed plants of *Helianthus annuus*, *Calendula arvensis*, and *Zea mais*, has been able to obtain very small individuals of these three species. But the most important fact, among those he has observed, is that while the dimensions of the plants decrease, their fertility is much impaired: the number of seeds which are produced becomes smaller, and dwindles down to 4, 3, 2, 1, only, and finally no more seeds are produced: a condition of absolute sterility is induced. This concomitant sexual variation is of great importance, of course, in showing that when dimensions vary they are not alone variable; there are other variations which accompany the differences of dimensions.

One fact must be noticed concerning the point which is now under consideration. It is the fact that while we can easily, through a number of methods, induce unfavourable conditions, it is much more difficult to induce favourable circumstances which lead to a better development. The advance of knowledge, however, may be expected to yield results which shall prove more satisfactory, but we perceive the difficulty of progress through the difficulty we experience when we wish to maintain any natural or

[1] *De quelques Variations considérables observées chez les Végétaux. Journ. d'Hist. Naturelle de Bordeaux*, t. iii., 1884, p. 156.

artificial race at its highest standard, and we all know how readily degeneracy interferes with and ruins the work of man or nature.

Dimensional variations, although very considerable, cannot be regarded as unlimited. We cannot expect to make any species of plant or animal become much larger or much smaller than it is. Of course there are natural or artificial conditions under which all species acquire a better development, and many facts display this. But we cannot expect to be able to increase the dimensions of any species beyond a certain point. Such an increase would require numerous variations in all the systems of the organism, stronger bones for instance, a stronger heart, and so on.[1] And then, on another side, giant forms would require so much food that their number could never become very large, and in fact, much goes to prove that such forms would have much trouble to compete with others, while the smaller forms could more easily live and maintain themselves. So there certainly exists a limit to the increase of dimensions—a physiological limit which cannot be passed without danger to the organism. Conversely, there is also a limit to the decrease of dimensions. Too small animals or plants are too

[1] Paul Bert (*Sur le Maximum de Taille que puissent atteindre les Animaux Vertébrés*: Soc. de Biologie, 1878) considers the maximal dimensions of vertebrate animals as dependent upon the strength of the cardiac muscle.

weak to thrive unless considerable variations also occur in their mode of life; or their fertility may be very much impaired, and thus the species is liable to go to ruin. Thus it seems that, as things are, the condition of every species—including under this word *condition* the state of all parts of the organism—is exactly what it should be to meet the present external circumstances, and departures from this condition are possible only when necessary to the species itself, through a change in circumstances. Concerning decrease in dimensions, we may note that while a continuous decrease must surely end in death, there are cases where a large loss may be sustained without bringing about this result. I made some experiments on this point, a few years ago, and obtained the following results. Wishing to ascertain the loss of weight which animals are able to sustain without losing their life, I weighed a number of Invertebrates, crabs and medusæ among others, and kept them in pure sea-water without any chance to get anything to eat, although I have reason to suspect some more enterprising individuals did eat some of their brethren. But many mishaps befell this experiment, in one way or another—the course of true *experiment* seldom runs smooth—and at the end of a fortnight most of my animals were gone, so that, in order to prevent a complete disaster, I preferred stopping the process and taking note of the results. Of all my animals only two were left: two *Aurelia aurita*, a species of medusa common on the Mediterranean coast. I had originally put three in the aquarium, but one had died. The three I had begun with weighed 98, 82, and 57 grammes at first. At the end of the fortnight the two remaining famished creatures weighed but 25 and 13 grammes. Assuming for accuracy's sake, and in order to prevent an over-estimation of the result, that these two were those which weighed originally 82 and 57 grammes, we see that the loss has been at least two-thirds in one case, and three-quarters in the other.[1] This loss is very considerable, for, as Chossat has shown in his investigations on the effects of inanition, mammals die before they have lost half of their original weight. When the experiment was interrupted, my *Aurelia* were in good condition, and seemed quite inclined to live longer: in fact they did live. This experiment should be repeated with *Beroe ovata*, or some other species of this genus, for I have noticed that these animals rapidly lose in dimensions when in captivity

[1] Henry de Varigny, *Bemerkung über den Gewichtsverlust durch Nahrungsmangel bei Aurelia aurita. Centralblatt für Physiologie*, 12 November, 1887. Of course it must be said that in this case the greater proportion of the loss of weight is due to loss of water, since water is in such animals even more abundant than in higher terrestrial organisms. But it must be noticed that even if the loss of weight is especially due to loss of water, the latter is due to the loss of organic tissues or substances with which the water was combined.

with little to eat. Of course it is quite natural that organisms which do not eat become smaller, and if under-fed as a rule they must doubtless remain of inferior dimensions. But there are cases where, notwithstanding abundant food, animals are unable to grow to their accustomed dimensions. Herbert Spencer says that it is well known by all anglers that trout and other fishes are small in small streams, and large in larger rivers, and many naturalists are of the same opinion. Is it that these animals remain small because they get less to eat? or is there some other reason?

I have also made some investigations on this subject during the past two or three years, and may be allowed to recall them. The starting-point of these investigations was the fact announced by Karl Semper some twenty years ago, in a special paper on the matter, which he has since abstracted in his *Animal Life*, that if the common pond snail is kept in small volumes of water, of less than five or six litres, the animal does not attain its regular development, and remains more or less dwarfed. For instance, if three young pond-snails (*Lymnæa stagnalis*, or *L. auricularia*), of the same brood and age, are put respectively into aquaria containing 500, 1,000, and 3,000 cubic centimetres of water, a difference in their dimensions may be detected even after a few days, and if the experiment is allowed to last some months, we finally see that the inhabitant of the largest volume of water is the largest in all ways, that of the smallest being smallest, and that of the intermediate aquarium being between the two as concerns dimensions. Such is the general fact. But many points are to be considered when we try to explain it. The first explanation which suggests itself is that in the larger space there is more to eat, and that the pond-snails in small aquaria remain small because they cannot secure food enough. This objection and explanation are amply met by the fact that in all my experiments care was taken to provide superabundance of food in the form of aquatic plants, and that the animals, whether in small or large aquaria, had always at their disposal a much larger quantity of food than they could possibly eat, or than they really did consume. So this explanation cannot stand. Prof. Semper has thought of a curious interpretation. He supposes that there exists in common water some matter which, while not possessed of nutritive properties, is conducive to growth and development, and is a sort of incentive to both. If the animal lives in a small body of water it has but a small quantity of this matter at its disposal, and does not grow as much as an animal in a larger quantity of water. This interpretation is contradicted by a very simple experiment. Take two equal volumes of water, 1,000 cubic centimetres for instance, and put one of them into a broad and shallow basin, so that it extends over a large surface, while the other is poured into a spherical glass vessel, so that the horizontal surface is very small. The two volumes are equal, but their form is quite different. Into each vessel, with an abundance of aquatic plants—*Myriophyllum* and *Elodea* especially: always submerged sorts, so that they are not in need of a large surface, and cannot interfere with it—put one pond-snail of the same brood, or cluster of eggs, recently hatched. The difference after a few days is surprising, and in the course of time it is seen that the pond-snail of the large-surface vessel is much larger than the other one. As the volume of water is the same in both cases, we must conclude that in itself the volume is not that which determines the variations of growth, and also that Semper's interpretation cannot be accepted, for, whether spherical or wide-mouthed, the same quantity of the same water should contain the same amount of Semper's hypothetical matter.

If, then, we cannot admit Semper's explanation, what is the cause of the observed facts? This question may be answered by new experiments, in which various conditions may be made to vary at will and in different known degrees. In my first series of experiments I used equal volumes of water, but with different surfaces. One of the volumes, for instance, was poured into a large-surfaced vase of fifteen inches diameter, while the other was poured into a vase of only four or six inches diameter. In such cases I always found that the animals living in the large-surfaced vase became much larger than the others. Why so? Is it that the water has better aeration in the large-surfaced vase? But this is of no account at all. In the first place I would call attention to the fact which I have repeatedly observed since I began this series of experiments, (and of which I am at present a daily witness), that the aquatic plants which I used in my experiments (*Myriophyllum* and *Elodea canadense*) do positively thrive and grow much better in narrow-surfaced vases than in large-surfaced vessels, in spherical glass balloons with a long neck, in which the water has but a very meagre surface contact with the atmosphere (two centimetres diameter for instance), than in twenty centimetres diameter vessels. This shows certainly that in spherical vessels, with small surface, aeration must be very good. On the other hand there is no reason to think that aeration is better in one case than in the other, as the water contains a large amount of plants which ensure good aeration and (in the last instance) the pond-snails care nothing whatever about the aeration of water, since they are not gill-bearers but pulmonated; they breathe at the surface, and do not breathe the air contained in the water. So aeration has nothing to do with the matter. It has so little to do that I have, in some experiments purposely devised, been able to see that pond-snails live exactly as well in two identical vessels (identical in shape, surface, volume of water and amount of aquatic plants) one of which remains open, in contact with the atmosphere, while the other is stopped by a paraffined cork, the amount of air imprisoned between the cork and surface of the water hardly amounting to 50 cubic centimetres. Even if it is argued that some air may pass in and out, through the cork, the quantity is very small, and we may consider the renewal of the air as very inconsiderable so far as penetration from the atmosphere is concerned. Of course the air does and must remain quite suitable for the animals, since they thrive, and the plants are the agents of this continued purification. If the animals can and do live under such conditions, and even live as well as they do when the communication with the atmosphere is not interrupted, does this not show that aeration must be considered as quite sufficient even when the surface is small?

In a second series of experiments I varied the volume of the water, but allowed the surface to remain the same. For instance, into two vases of identical form and diameter I poured unequal volumes of water. The surface was the same, but the volumes were very different. In such cases, while the animals were certainly larger in the larger volume, the difference was not considerable. The influence of volume-variations is thus seen to be much less important than that of surface-variations.

These experiments seem to me to call for the following interpretation. The volume of water is in itself of comparatively small importance, especially for some species of pond-snails, and the real influence is exerted by surface. And surface operates in this manner only: the larger it is, the more exercise the animals are able to take. The *L. auricularia*, to which the above-mentioned experiments refer more especially, seems to dislike deep vessels, and moves usually in the horizontal plane near the surface. If the surface is small it moves but little, while if it is large the animal moves a great deal. On the other hand *L. stagnalis* seems generally to care less about surface or depth, and to prefer living in the deep parts of the vessels where it is always moving about. I have seen it in one case live almost all the time in the deepest part of its prison (a glass balloon with a long narrow neck), and it acquired dimensions certainly equal to those of another one living in a large-surfaced vessel. There is some difference certainly between *L. stagnalis* and *auricularia* in this respect, and I call attention to this point.

I have tested Semper's interpretation in another manner. I have caused pond-snails of the same age and brood to live in unequal volumes of the *same* water in the following manner. In some cases I have used one, or two, or more glass tubes, of same length and diameter exactly (two pieces of the same tube), which were closed at one end with some muslin stretched over the aperture, and made fast by means of a string or thread wound around the tube. The tubes were suspended in a large vessel containing three or four litres, by means of a string, in such a manner as to allow the other end to rise, say two centimetres, above the surface (to prevent the animals from getting out of the tube and going into the vessel). In each tube I put one pond-snail, with a sufficient quantity of aquatic plants (submerged always), and one in the vessel, outside of the tubes. Every day, and many times a day, the tubes were lifted so as to empty them of water, and immediately replunged, so as to ensure the mixture of the water *inside* them with the water *outside;* moreover the water in the tubes was in constant communication with the water around them, through the muslin, whose only function was to prevent the animals from escaping from the tubes into the vessel, or *vice versâ*. In all such experiments, the water being the *same* in both tubes and vessel, food being superabundant, temperature identical, and surface and volume only being different, I have seen the pond-snails in the tubes remain much smaller than those in the vessel. I may even add that in some cases I have had one tube as above described, and another stopped at the lower end with a good cork and wax around it, so that the water in the tube never got mixed with that of the vessel, and have hardly if at all noticed any difference in the dimensions of the animals of both tubes.

This experiment may be performed in another manner by fixing up a sort of cage (with muslin and glass rods), which affords more space than the tubes, and more surface and volume; the communication between the water inside and the water outside is still better, since it is effected through all the available sides of the cubic cage; the results are the same, and the animal inside the cage remains much smaller than the one outside it. This series of experiments answers the objections which might be raised on the ground that in the smaller volume of water the proportion of waste products might be larger, and exert a noxious influence. But these waste products do not require to be taken into account in such cases. I have also seen that unless a given volume of water has been inhabited for a long time or by a large number, it exerts no bad influence on the growth of other pond-snails. For instance, take two identical vessels, and use, in one, one litre of pure fresh water, in the other the same quantity of water in which a pond-snail has been living two or three months; in each put one young *Lymnæa*, of same age and brood; kill both after the same time (three or four months); there is no observable difference. Of course, if the stale water has been much inhabited by pond-snails, the growth of the fresh ones is impaired. But such impairment does not occur in my experiments, and I do not well see how waste products could accumulate more in a narrow-surfaced vessel where aeration is very good, as the plants show, than in a large-surfaced vessel, where it must be also good, the quantity of water being equal in both, or even, as is the case in many of my experiments, superior in the former.[1]

So it seems that Semper's interpretation has to be dismissed as unnecessary, and that a simpler explanation is furnished by the results of my experiments—an explanation which depends upon known principles

[1] All these experiments shall be related in greater detail in a forthcoming memoir, as soon as I have completed the experiments which are yet being continued (March, 1892).

of known influence. It is quite natural that exercise should have an influence upon growth and development, and that in cases where there are physiological or mechanical *impedimenta* to movement, dwarfing should be the result. I think that this is the explanation which must be accepted; and if animals living in confined spaces remain small, this is due to the fact that they cannot move enough. At all events Prof. Semper's interpretation seems to me not acceptable. Further experiments will yield new facts, and time will tell whether this explanation is sufficient.

In this connection I may call attention to the circumstance that an observer who has given some study to dwarfing in *Lymnæa* has pointed out a singular fact connected with this process, but one which requires to be confirmed by new investigations.

It is the fact that dwarfed forms are generally exclusively female, and that their liver offers a considerable amount of degeneration.

So much for dimensional variation.

If we now pass on to consider the integument, we perceive that in this part, and in its appendages, variations are numerous and also important. Many animals, when transferred to warm climates, lose their wool, or their hairy covering is much reduced. In some parts of the warmer region of our earth sheep have no wool, but merely hairs like those of dogs. Similarly, as Roulin notices, poultry have, in Colombia, lost their feathers, and while the young are at first covered with a black and delicate down, they lose it as they grow in great part, and the adult fowls nearly realize Plato's realistic description of man—a biped without feathers. Conversely, many animals, when transferred from warm to cold climates, acquire a thicker covering, dogs and horses, for instance, becoming covered with wool, &c. Such cases are easily observed in Europe when animals from the warm regions are sent to our zoological gardens. In Paris, for instance, we have seen sheep from Senegal acquire, in the course of two years, a long and grizzled cover of hair, while at first they had but a short one. Similar modifications have been observed in a large number of animals, and more precise data could have been obtained if more attention had been paid to the subject. As M. Faivre rightly remarks in his *La Variabilité des Espèces et ses Limites* (1868), while "no truth is better established in natural history than the influence of climate on the superficial character of animal species, on the dimensions, colour, form, nature of integuments and hairs, none has been less investigated and discussed by the naturalists whose business is to distinguish—one might even say, to multiply—species."

Variability and variation of such superficial characters sometimes go to such an extent that animals of the same species have been at times considered as belonging to different species, and even to different genera. Such has been the case, for instance, with two fishes—*Abramis versicolor* and *Stilbe americana*—which C. C. Abbott recognizes as one and the same species which has a great tendency to variation, not only as concerns colour, but in respect of fins and scales, according to its environment.[1] This is doubtless an extreme case, but its interest is considerable, in that it exemplifies, on the one hand, the importance of variation, while, on the other, it shows once more how very artificial and unsound our specific and even generic distinctions in some cases are. While we ascribe most of the superficial, or integumentary, variations to that general and complex factor which we call change of climate—although we cannot in all cases tell which particular factor of the complex operates—there are cases where we can trace the variation to one determined cause. Such is the case with the variation in length of wool. There is a direct relation between the abundance of food and the length of the wool of sheep, for instance. Krocker,[2] in Proskau, has shown that the amount of wool yielded

[1] C. C. Abbott: *Notes on the Cyprinoias of Central New Jersey. American Naturalist*, vol. viii. p. 326.

[2] His paper has been published in the *Annalen der Landwirthschaft in den Koeniglich Preussischen Staaten* for 1869.

daily per 1,000 kilograms of sheep varies in the following proportions according to the food:—

Kg.		
0·691	of wool:	scanty winter food.
0·870	,,	plenty of hay.
0·958	,,	good pasture.
1·080—1·240	,,	fattening process.

If we now turn to plants, we perceive the same variability in the superficial integumentary organs. I merely recall here—because I shall have to refer to it at greater length later on—the considerable differences which many observers have recently noticed in the anatomy and characters of the same parts which successively lead aquatic and aerial lives. In these cases the influence of environment is easily to be traced and appreciated. It is also well known that where mountain-plants are transferred to the valleys and plains they lose the hairy covering which they generally possess, while valley-plants transferred to the mountains acquire this same covering. Linnaeus noticed that *Persicaria* is devoid of this sort of down when living in humid places, while it becomes very villous in dry stations. The same is noticed of *Thymus serpyllum*. Many plants, in short, exhibit two varieties which are readily distinguishable—the glabrous and the villous; such are *Prismatocarpus speculum*, *Isatis tinctoria*, *Jasione montana*, *Onopordon acanthium*.

Similar cases are met with among spiny plants. While some plants, which possess no spines, become markedly spiny when they grow in some localities, others, which are spiny, lose their appendages. Such is the case with *Capparis spinosa* for instance, of which Turrel[1] has described a variety without spines in the Balearic Islands. In all other respects this variety exactly resembles the common form. Whether a *lusus naturae* or not, this peculiar character is hereditary, as the seeds of this form always yield non-spiny plants, in France as well as at Mahon. Another case is that of *Ulex europaeus*, of which there exists a non-spiny form, as Trochu has shown.[2] This form is seldom met with, as it has less chances of success in the struggle for life, for while oxen, rabbits, hares, and other animals are respectful and deferential towards the common spiny form, they have a great liking for the other one, and eat all they can of it. It must be added also that the last-named bears but few seeds, and thus cannot become very abundant. De Jussieu considers this *Ulex nanus* as a variety of *Ulex europaeus*, and Vilmorin has made some investigations concerning this form,[3] which may become

[1] L, Turrel : *Sur le Caprier sans Epines. Bull. Soc. Zool. Acclimatation*, vol. viii. p. 448.

[2] L. Vilmorin : *De l'Ajonc sans Epines. Revue Horticole*, vol. xii. p. 151.

[3] *Note sur un projet d'Expérience ayant pour but de créer une Variéte*

very useful, as it can be given as food to animals which will not eat the common spiny form. His first experiments have not proved satisfactory, for the seeds of *Ulex nanus* have always yielded plants of *Ulex europaeus*. But since the tendency to vary is strong enough in *Ulex europaeus* to afford some plants varying in the direction of non-spinosity, we may hope that by means of careful selection *Ulex nanus* may become an abundant and permanent form.

So much for integumentary variations. While considering variability of external and superficial characters, we may now say a word of form-variations. These are very frequent among many groups of animals, and particularly among molluscs. Locard, in his interesting and valuable *Variations malacologiques* (vol. ii.), has collected many instances of form-variations noticed by himself and by others. In his opinion, *Lymnæa frigida* and *thermalis* are mere varieties of *L. peregra*, while *Ancylus rupicola* and *thermalis* are varieties of *A. simplex*, the only difference being a matter of mere form. Brot has noticed that in the cool mountain waters, *Lymnæa auricularia* has only four whorls to its shell instead of five, and the Marquis de Folin observes that the pond-snails of

d'Ajonc sans épines, se reproduisant de graines. Bulletin de la Société Industrielle d'Angers, 1851. Also in *Notices sur l'Amélioration des Plantes par le Semis*, 1886.

the Lake of Constance are less regular in form, more abrupt, he thinks on account of the movements of the water of the Lake.

Baudin also considers *Pisidium pulchellum* and *cinereum* as two forms of the same species, and Locard himself has discovered through experiments that *L. turgida* and *elophila* are mere varieties—due to environment—of the common *Lymnæa stagnalis.* He says: "These are not new species, but merely different aspects of a common type, which is capable of modification and of adaptation according to the nature of the media in which it has to live." Bateson has recently observed similar facts concerning *Cardium edule;* Locard shows how extensively any one species —*Unio rhomboideus* for instance—varies *in forma* and *in colore* according to its habitat, lake, river, or torrent, and an indefinite number of such instances might be quoted here. The same may be said concerning plants. All know that in different stations the same species exhibits considerable differences in form, in the comparative height of the stems, in the number, length, distance of the branches, and so on, and experienced practical botanists easily recognize through these differences the origin of an individual plant, detecting whether it has grown in a valley or on the Alps, in dry or in moist soil, in an exposed or in a protected station. As the well-known fungologist,

M. Boudier, of Montmorency, says, in valuable notes which he has kindly written down for me in answer to many queries, "plants growing in dry, unprotected soil are small and dwarfed, while the same species living in moist soil are more vigorous, more developed, and especially much taller. A common species, *Serratula tinctoria*, grows indiscriminately in dry and in moist soil; in dry and unprotected stations it seldom is over ten or twenty centimetres in height, while in moist soil it easily attains one metre (100 centimetres). The common dandelion (*Taraxacum dens leonis*) has in dry soil leaves which are much more irregular and incised, while they are hardly dentate in marshy stations, when it is called *Taraxacum palustre*." Individuals of the same species growing near the sea-shore differ markedly from those growing far inland. Similarly species, such as some *Ranunculus*, which can live under water as well as in the air, exhibit marked differences when considered in their different stations, as is well known to all. These differences may be important enough to induce botanists to believe in the existence of two different species when there is only one. A century and a half ago, G. Bauhin and Tournefort described two different species of Coriander. But Fabrejou, a botanist of that time, who has written a large treatise on systematic botany, under the title, *Description des*

Plantes qui naissent ou se renouvellent aux environs de Paris,[1] was able to show that the two so-called species are one and the same. "Here is the proof," he says: "When the same seed, sown in fertile and infertile soil, yields the two so-called species, one must conclude that there is but one single species, and that that which seems to establish a difference between the two so-called species, can only come from the climate and culture. It is certain, as I have often witnessed the fact, that the same seed, sown in fertile and infertile soils, produces the two alleged species." And Prof. Bonnier, recently, in his *Etudes sur la Végétation de la Vallée de Chamounix et de la Chaîne du Mont-Blanc* (1889), says, corroborating others, that "in high altitudes the appearance of the same species is dissimilar: the stems straggle on the ground, leaves are narrower and thicker, flowers are comparatively large and of higher colour, and most of the plants even lose many morphological characters which they possess in the plains. . . . The characters of plants of high altitudes are even different enough to have induced many writers to describe these alpine forms as particular species." Prof. Bonnier's statements are of especial value from the fact that they are based on facts derived from experiments made in stations situated at different altitudes; they are not facts of mere observation.

[1] 1740, 6 vol. in 18, vol. iii. p. 244.

I have alluded to the considerable morphological variations which are observed in *Ranunculus aquatilis*. Godron, who, in 1839, published an important monography of the *Ranunculus* group, has studied these variations with great detail. When the plant develops wholly under the surface of water, all its leaves are delicately laciniated. If the plant is able to send some of its leaves to the surface, they float and assume a very different form, being kidney-shaped and lobed. The same plant when growing entirely out of water presents a very different appearance: the stem is short, much divided into branches, which bear a large number of small leaves, cylindrical, much divided, and somewhat thick. If it were not for the floral organs, one would certainly believe in two or three species. There is but one, however, which varies greatly according to external circumstances, and this is shown by the fact that the same individual plant under different circumstances presents the different appearances which have been mentioned. Lamarck believed that *Ranunculus aquatilis* might be transformed into *R. hederaceus* through changes in the environment, but Godron denies the fact. *Rubus fruticosus* seems also to vary considerably. *Sagittaria sagittæfolia*, when growing in deep water has also ribbon-shaped leaves, while in shallow water it has also arrow-shaped leaves, which rise vertically instead

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of floating horizontally. Similar variations in form are to be observed in *Myriophyllum verticillatum* and *Juncus supinus*, and many other plants. *Polygonum amphibium* also exhibits important morphological variations. When growing out of water it has lanceolated, downy leaves, with short stalks, and covered with stomata on both faces, while the same leaves, if the plant is growing under water, are deprived of hair, have a long stalk, and are obtuse, without stomata on the lower side. These two forms of leaves are often met with on the same plant, where it has been, through accidental circumstances, growing for some time under water and for some time out of water. Ch. Martins[1] notices similar facts concerning *Jussiæa grandiflora*, where the variations are even more important. Every one may notice in our common ivy considerable variations in the form of leaves, and these variations are also to be seen in other plants, in fact they are more or less present in most plants, and careful investigation will disclose their number and importance.[2] These variations, which seem to be of no account as far as the general life of the plant is concerned, may however be accompanied by important

[1] *Observations sur la Jussiæa grandiflora*, in *Bull. Soc. Botanique de France*, vol. xiii. p. 176.

[2] G. Fournier: *Recherches Anatomiques et Taxonomiques sur la Famille des Crucifères*, 1868, and also Faivre: *La Variabilité des Espèces et ses Limites*, 1868.

differences in the physiology of the plant. For instance, Carrière,[1] after having noticed the formal variations of the leaves of the ivy according to its mode of life (climbing, or entirely independent and tree-like), adds the significant fact that the leaves of the climbing plant when inserted in the soil readily start an independent life, and emit roots very soon, while those of the independent form do so only with great difficulty. Here is certainly an inportant physiological difference; not perhaps in itself, but as indicating differences in the structure and life of the whole plant.

Fruits vary as well as leaves; the same branch of a peach-tree, for instance, bears peaches and nectarines; the same branch of an orange-tree bears oranges and lemons; the same branch of an apple-tree bears quite different varieties of apples. Prof. Decaisne, who was an authority in the matter of fruit trees, especially apple and pear, observed[2] considerable variations among the descendants of seeds of the same sort; in the course of a few years, from the same seeds, he obtained six different forms of pear-tree, in which the fruits were unlike, while differences also existed in the general morphology of the plant.

[1] *Polymorphisme des Végétaux. Revue Horticole*, 1886, p. 209.
[2] *De la Variabilité de l'Espèce dans le Poirier. C. R. Acad. des Sciences*, 1863.

Among flowers the same variability obtains. Colour may be different, but there are also important variations of a morphological order, and many botanists have pointed out the more interesting cases in all parts of the world. Maxwell Masters has collected a number of them in his *Teratology*, and Udo Dammer has added many in the German edition of this work; more recently, Dr. O. Penzig, of Genoa, has collected all known cases anew, in his important *Pflanzen-Teratologie* (1890). In this book, of which only the first half has yet been published, we find a very complete list of teratological cases, of cases of variation in all parts of the plants, and of every sort, so that I may refer to this book once for all, as concerns all plant variation. Some idea of its value may be gathered from the fact that 166 large octavo pages are filled up with the mere titles of papers referring to variation, and that the whole work is devoted strictly to facts, so that it may really be considered as a list—as complete as possible—of all departures from the normal types. Of course variations of floral structures are numerous, and cases abound in this work, but I prefer referring, as an instance, to a case which is not noted by Dr. Penzig, and which is of great interest, as it concerns important variations observed in the floral structures of one and the same individual plant, a *Tradescantia virgi-*

nica.[1] "This plant," says Mr. G. A. Brennan, "presents, as the result of thirteen years' cultivation, the curious aspect of a monocotyledonous plant bearing in bloom at the same time flowers of dimerous, trimerous, tetramerous, pentamerous, hexamerous, and heptamerous types respectively, each flower bearing twice as many stamens as sepals, petals, or carpels of the ovary. The plant was set out in 1872, and received very rich treatment, so that it gave forth blossoms measuring two inches in diameter. In 1874 it began to depart from the original trimerous type and to assume the tetramerous one, by developing another petal, and instead of doing this at the expense of the pistil or stamens, it *added* another sepal, another carpel with style, and two stamens, thus making a typical tetramerous flower. The plant has since then continued to differentiate in a greater degree each succeeding year." In 1876 it became pentamerous, in 1879 hexamerous, in 1882 dimerous, in 1886 heptamerous; thus you perceive that there has been no regular order in the course of differentiation. At present, while the pentamerous type is dominant in this plant, dimerous and heptamerous flowers are scarce. It seems that further variation is forthcoming, for an octamerous ovary has been detected in one flower. This fact is certainly

[1] G. A. Brennan: *Variations of Tradescantia virginica. American Naturalist*, vol. xx. 1886, p. 55.

one of the most striking which can be quoted in respect of floral morphological variation. Such variation is always present among different varieties of the same species. For instance, C. E. Bessey[1] has investigated the floral structures of different varieties of apples, and while it is generally thought that no difference obtains, he has detected considerable differences in the form of the stigmas and styles, and found that the pistil varies much in length, breadth, hairiness, &c. And the proof thereof is seen in the engravings which accompany his paper, and refer to five well-known varieties of apples—red Canada, Talman Sweet, Rambo, Wagner, &c.

The very smell of flowers is also subject to variation, as Dalibard[2] showed by direct experiment nearly a century and a half ago. He planted mignonette in different soils, using seeds from the same mignonette plant, possessing its well-known fragrancy. While the seeds sown in rich garden soil became vigorous, and were well perfumed, the seeds sown in sandy soil produced plants which remained weak and small, and had no perfume. It even seems that the latter did not acquire any odour when transferred to rich

[1] *Can Varieties of Apples be distinguished by their Flowers? American Naturalist*, vol. xx. 1886, p. 162.

[2] *Observations sur le Réséda à fleur odorante*, in *Mémoires de Mathématiques et de Physique de l'Académie des Sciences*, 1750, p. 95.

garden soil. Similar facts have since been repeatedly observed and noticed.

In the more internal functions and organs of animals —and of plants as well—the same variability shows itself. In man himself, as Mantegazza has shown, teeth vary considerably, and a careful study of the third molar tooth has shown that there is a strong tendency towards the disappearance of this part, and while among inferior races all that concerns it is normal in 50 to 54 per cent., abnormality becomes considerable among superior races, where the normal state is only met in 37·09 per cent., leaving 62·91 abnormal in one way or other. No doubt, we could find numerous cases of variation in the dentition of mammals, although the number and form of teeth is considered as a specific character. But teeth may be considered as external organs in some sense, just as fur or feathers; and it is even more interesting to see that more internal parts vary perhaps as much, if not more. Such is the case with the bones which go to make the skeleton of mammals and other animals. Some instances are referred to by Darwin, and by Wallace in his recent and valuable *Darwinism*; St. George Mivart has shown that the number of ribs varies among the apes; in man himself the number varies from twelve to thirteen; and concerning whales, Georges

Pouchet[1] says: "Balænidæ certainly are among those higher vertebrates whose skeleton exhibits least fixity; this is a peculiarity which cannot be denied." The same writer also says that in many species which live in limited regions the same skeletal variability exists in a marked degree, and although the individuals are absolutely similar so far as exterior characters are considered, they may display a variability which may be said to be unlimited, in the number and relations of the bones. And Pouchet and Beauregard[2] say also that it would prove difficult to meet with two skeletons of the Anteater which were exactly similar as concerns the number of the ribs or vertebræ, or the connections of the ilium or ischium with the vertebral column.

As to differences in weight and length of the skeleton in different individuals of the same species, Darwin and Wallace have also collected numerous data of which all are cognizant. The soft parts of the body display the same tendency. All have heard of John Hunter's experiments on the sea-gull (*Larus tridactylus*). He fed it during a year on grain, with the result of hardening to a large extent the inner coat of the

[1] *A propos de deux Photographies de Baleine Franche: Compt. Rend. Soc. Biologie*, 1890, p. 705.

[2] *Traité d'Ostéologie Comparée*, 1889, p. xii.

stomach of this animal, which, being a flesh-eater, does not require to have the hard and horny coating of the gizzard of the pigeon or fowl. This experiment is repeated each year in nature, and without man's operation, by another gull (*Larus argentatus*), of the Shetland Islands, which, according to Dr. Edmonstone, changes the structure of its stomach twice every year, according to its food, which consists of grain during part of the year, and of fish during the other months. So the stomach may vary considerably in its use and functions, and Holmgren's experiments show that the gizzard of a grain-eater, such as the pigeon, may be converted into a carnivorous stomach, such as that of one of the birds of prey.

I have already said that there is great variability in the muscular system. Some anatomists have made a special study of this variability: Wenzel Grüber in Germany, Testut in France, Cunningham of Dublin, and many others. Not only are there variations in the mode of attachment and course of every muscle of the human body—which has been more especially studied in this connection—but supernumerary muscles are often found which are all exactly similar to muscles which normally exist in lower animals, but do not as a rule exist in man. Testut has dwelt upon this fact, which is of great significance in the evolution theory, and a very large number of instances might be given of man having muscles which are considered as peculiar to the ape, horse, dog, bear, &c.[1]

Variations also occur frequently in the anatomy of many internal organs. Wallace refers to the variability in the length of the digestive system in the giraffe and other animals, and in the nature and position of the gall-bladder, which in the same species is sometimes present, either single or double, sometimes absent. These variations are not confined to higher animals. Claus observes that *Æquorea forskalea*, a Cœlenterate, varies much in the number of the radiating canals[2]; and many botanists have noticed the important structural variations which obtain in plants. E. Mer has carried his investigations into great detail in regard to *Isoëtes lacustris*, and other plants.[3] It results from these investigations that the internal anatomy of plants may vary considerably. This variability displays itself also in regard to sex; for it has been shown that external influences play a large part in the determination

[1] *Cf.* R. Wiedersheim, *Der Bau des Menschen als Zeugniss seiner Vergangenheit* (Freiburg i. B., 1880).

[2] *American Naturalist*, vol. xvi., 1882, p. 147.

[3] *De l'Influence exercée par le Milieu sur la Forme, la Structure, et le Mode de Reproduction de l'Isoëtes lacustris. Comptes Rendus* 1881, p. 94 (Jan.–July). See also *Des Causes qui modifient la Structure de certaines Plantes aquatiques végétant dan l'Eau. Bull. Soc. Botanique*, 1880, p. 194.

thereof. Whilst among tadpoles left to themselves, the females are in a slight majority, the proportion increases from 54 to 78 per cent. when the tadpoles are fed with beef, to 81 per cent. when fed with fish, and, when fed with frog-flesh, to 92 per cent.[1] Thus food, and the nature of food, has much to do in the determination of sex. The same is the case with bees, where the production of queens, workers, and drones is in great part a matter of nutrition. A worker-larva may be reared into a queen, if royal food is provided. Other facts show similarly that external influence must be at work to operate in the determination of sex. Fisch has noted the sex of 66,327 plants of hemp, and he finds there are 154 female against 100 male plants. Among other plants, such as *Spinacia oleracea* and *Rumex acetosella*, the proportions vary much, as in some cases the one sex, in others the other, is predominant. In the human species males are constantly in slight excess over females—105 against 100. The same condition obtains among oxen, sheep, hogs, and domestic birds. But in the case of the latter, the constancy is less, and during some particular years there is a very large number of individuals of one sex against a small number of the other. There

[1] See Yung, *Propos Scientifiques*, 1890, Reinwald, Paris, quoted in *Evolution of Sex*, P. Geddes and J. A. Thomson, Lond., 1889.

are some external causes in operation which are not yet detected. M. C. Cornevin thus summarizes the proportion of males to 100 females in the following species:—

Horses	101	males	against	100	females.
Oxen	104·6	,,	,,	,,	,,
Sheep	115·4	,,	,,	,,	,,
Hogs..................	104·9	,,	,,	,,	,,
Turkeys	120	,,	,,	,,	,,
Guinea-fowl.........	102	,,	,,	,,	,,
Common fowl	101	,,	,,	,,	,,
Duck..................	115	,,	,,	,,	,,

That external influences do play an important part in the determination of sex is shown by numerous facts. Spallanzani, Bernardi, and Autenrieth have shown that female plants of hemp when mutilated bear male flowers, and Müller has in some cases seen male plants bear female flowers. Müller has also observed female plants of *Zea Mays* bearing male flowers when nutrition was deficient. Hoffmann has noticed that in *Lychnis*, *Spinacia*, and *Rumex* the proportion of sexes varies according to the greater or less interval between each individual plant; and Cornu, Giard, and Magnin have shown that in *Lychnis vespertina*, under the influence of parasitic "rust" (*Ustilago antherarum*), female flowers bear stamens. Prantl has seen similar facts among Cryptogams. While the seeds of ferns develop into male plants when the soil is poor in nitrogen, or when the seeds are very near each other, they yield female plants when nitrogen is abundant, and the seeds somewhat distant.

Yung's experiments on tadpoles had already been performed by Born with similar results, and it seems that in the human species, a change of climate is often conducive to a larger production of females. In Java, for instance, European or white children are born in the proportion of five females against two males; in Yucatan, in the proportion of eight females against two males.

All these facts go to show that sexuality is in great part determined by external factors, whatever these may be, and that much variability is here present. This variability may be readily seen in the same species, under different conditions, and even in the same individual. For instance, Carrière has pointed out that variability is common in *Ailanthus glandulosa*, a well-known plant in which the distribution of sexes is very inconstant. While some individuals bear a large number of female flowers, many bear but few, and it is a curious fact that they are all to be found on the same branch, instead of being on different branches, interspersed with male flowers. There seems to be some special condition in one or more branches which determines the production of female flowers.[1] This condition may vary from one year to another, in the course of the lifetime of the plant. It even seems that in normally male plants, this condition may put in an appearance. Ch. Martins[2] observed at Montpellier a male *Chamærops humilis* which yielded only male flowers from 1851 to 1861; in 1861 this plant produced some female flowers, quite normal, since the seed from these flowers yielded vigorous young plants; and in 1862 a large proportion of female flowers were to be seen. This last fact is of real importance, as showing that sexual variability may exist to a high degree.

Another very interesting form of variability is that which may be observed in individual evolution or development. Although there are numerous cases of this sort, and although a large number of instances might be quoted where the individual evolution is readily arrested or modified through different circumstances, none seem more carefully ascertained than those which Camerano has published. This writer has investigated what Kollmann has called *Neotenia* in Amphibians. Neotenia is the lengthening (for an indefinite time) of the period during which Amphibians are gill-breathers. Every one knows that, at first,

[1] Carrière, *Sur l'Ailanthus glandulosa à propos des Sexes. Rev. Horticole.* 1872, p. 234.

[2] Ch. Martins, *Transformation d'un Chamærops humilis male en polygame. Rev. Horticole,* 1862, p. 353.

frogs, toads, &c., breathe as tadpoles, by means of gills, and that after a few weeks the lungs develop and the gills disappear, while the animal becomes an adult, and acquires new characters and organs. But as every one can ascertain, this gill-breathing period may be considerably lengthened under natural or artificial and experimental circumstances. I have myself kept toads in the tadpole state for over two years, merely by feeding them very scantily. They were born in the spring of 1889, and remained all the time in an aquarium in the laboratory, having water enough at their disposal, being always sufficiently provided with aquatic plants, and enjoying heat enough: it can by no means be said that their evolution was arrested by the cold of winter, as often happens in mountain ponds, when the cold of autumn sets in before the tadpoles have achieved their development, so that they become frogs or toads only in the course of the following year. In the case of my tadpoles, it seemed that the completion of development was due to my imprudently feeding them in the spring of 1891 on the very substantial flesh of their congeners; and in the course of some three weeks at most, the limbs were evolved, the long tail disappeared gradually, the very colour and appearance of the skin underwent considerable changes, and my superannuated tadpoles became toads at last. This Neotenia has been observed by many physiologists in different Amphibians. Mlle. de Chauvin has reared larvæ which were the progeny of Amblystoma, and while some of them became Amblystoma, others remained Axolotls in consequence of being kept in very well aërated water, where the gills had no tendency to atrophy or retrogression. Similar experiments have been performed by a large number of naturalists on different species of *Triton*, *Salamandra*, *Pelobates*, *Alytes*, *Hyla*, *Rana*, and *Bufo*; and the result is that while there are Amphibians, such as *Salamandra atra*, in which the length of the branchial or gill-bearing period is very short, and others, such as *Proteus anguineus*, and some Tritons and Axolotls, where gills exist normally in adult and even in aged individuals, there exist also a number of Amphibians among which the gill-bearing period, normally short, may be much lengthened. But in Urodela (newts and salamanders) this lengthening may and does occur without seriously modifying the evolution of the remainder of the body; and the result is that these tadpoles are sexually mature, while among Anura (frogs and toads) this lengthening interferes with the general development, and sexual maturity does not seem to occur among the tadpoles. New investigations are required to ascertain how far this sexual immaturity exists, and to what extent it may be retarded by the lengthening of the gill-breathing period.[1] If it could be shown that sexual maturity may occur although the tadpole state is lengthened, and that sexual reproduction may take place, although this is on obvious *a priori* grounds very improbable, we might perhaps try to obtain a new species which would exhibit very marked physiological features.

[1] Camerano, *Sur le Développement des Amphibiens, et sur ce que l'on a nommé chez eux la Néotenie. Arch. Italiennes de Biologie*, vol. v., 1884, p. 27. *Recherches sur la Prolongation de la Période branchiale ces Amphibiens. Ibid.*, p. 29. *Recherches sur le Developpement et les Causes du Polymorphisme des Têtards des Amphibiens anoures. Ibid.*, xv. 1891, p. 165.

LECTURE III

Summary: The Facts of Natural or Spontaneous Variation (*concluded*)—Physiological or Chemical Variation—Not always easily detected.—May be noticed in all parts of the Body, even between very closely related Forms—Exists not only between different Species, but between Varieties of the same Species, Individuals of the same Variety, and even different ages of the same Individual—Chemical Variation explains Racial Immunity to peculiar Diseases—This Chemical or Physiological Variation in some cases of much higher import than any Morphological Variation—Chauveau's Experiments on *Bacillus anthracis*—Physiological Differences between Brown and Green Frog towards Poisons and Heat—Tarchanoff's Experiments—Variation generally exists at all Ages, in all Groups of Beings, at all Geological Epochs—Sudden Variation.

THERE is a last form of variability to which I wish to call attention, and which has not been enough taken notice of up to the present time. I refer to what I have mentioned under the name of chemical or physiological variability. Under this name I include all facts which indicate a difference in chemical or physiological constitution, expressed through differences in the reaction of the organisms towards definite and common external influences. Such chemical variation must certainly exist at the basis of all specific or even racial characters, and if I dwell somewhat upon the topic, it is owing to the fact that this sort of difference has not been as much investigated as it ought to have been. Between two species, however closely allied, between two varieties or races of the same species, there are not only those slight external differences upon which so much stress is laid by morphologists; there are internal, chemical and physiological differences which are most likely of greater importance. For instance, Naudin cultivates in his gardens at Collioure, in the south of France, a number of plants of a species of *Echium;* part are indigenous, part come from the Canary Islands; they all exactly resemble each other, no external difference is perceptible; they differ in origin only. During the night, the frost comes; all the Canary plants die, while the plants of France resist. There is some difference in their constitution or physiology, some difference due to habit, to adaptation, however it may be called, and the result is that life may continue under circumstances which cause it to cease when this difference does not exist. This is one fact among a thousand, and horticulturists and breeders could provide many similar examples. It shows that, even in cases where no external differences are perceptible, variations do exist in given circumstances which may be of the highest importance, and decide life or death. Though they are not always of equal magnitude, they admit of being detected with real accuracy by appropriate methods. While calling this variation *physiological*, I understand it to be, in fact, *chemical;* the difference is in chemical constitution—most probably—but it displays itself mostly in *physiological* differences. I have collected some cases of this variability, endeavouring specially to obtain widely different instances, in order to show the extent to which this kind of variation occurs.

Of positive chemical variability among animals, I meet with a good instance provided by two well-known *savants*—Ch. Robin, the histologist, and Sainte-Claire Deville, the chemist—who, at Sanson's request, examined comparatively the structure and composition of the bony structures of the common breed, and of a perfected breed, of sheep. While the microscope detected no difference at all between the two breeds, chemical analysis showed considerable variations in the respective percentages of organic and inorganic matters, as follows:—

	Organic Substances.	Inorganic Substances.
Perfected breed	32·3 per cent.	67·7 per cent.
Common breed	38·6 ,,	61·4 ,,

Similarly, considerable variations obtain in the chemical constitution of the integumentary appendages of different varieties of animals. Here follows, for instance, the percentage of the principal components of the wool of some breeds of sheep (after Müntz and Girard):—

	Dishley.	Merinos.	South-down.	Solognot.	South Solognot.
Water	15	10	14	15·5	16
Nitrogenous matters	63	48	53	64	53
Fatty substances	8	30	19	6	10
Ash	11	10·5	15	13	18
Potash	6	4·5	7	6·5	4·5

While some components vary but slightly, such as water and potash, others, such as fatty substances, are found in very different proportions; the difference being from six to thirty, or from one to five.

Similar variations are to be observed in the muscular or fleshy parts. Sir R. Christison made chemical analyses of salmon in good health and condition, and of salmon after spawning. The results are as follows:—[1]

	Healthy Salmon.	Salmon out of Season.
Oil	18·53 per cent.	1·25 per cent.
Nitrogen	19·70 ,,	17·07 ,,
Salts	0·88 ,,	0·88 ,,
Water	60·89 ,,	80·80 ,,

It is not necessary to have studied physiology very deeply to understand that such differences in the flesh or skeleton, as are shown above, may be of great im-

[1] From a paper read in the Royal Society, quoted in the *American Naturalist*, vol. vii., p. 372.

portance under definite conditions, and may determine life or death in some circumstances, while at all events they must, in every-day life, put the animals which exhibit them in very different positions as regards the struggle for life and success in it.

Such differences are common in the chemical constitution of the different species of the same genus, and the following analysis by Forchammer well illustrates this :—

	Fucus digitatus.	*F. vesiculosus.*	*F. serratus.*
Potash	20·66	13·01	3·98
Soda	7·65	9·54	18·67
Magnesia	6·86	6·12	10·29
Lime	10·98	8·36	14·41
Phosphoric acid	2·36	1·16	3·89
Sulphuric acid	12·33	24·06	18·59
Ferric oxide	0·57	0·28	0·30
Silica	1·44	1·15	0·38
Sodium chloride	26·18	21·45	16·56

And again, among different individuals of the same species considerable differences may obtain according to the mode of life, and particularly, as Hermbstaedt has seen, according to food. This fact is well displayed by the results of Hermbstaedt's experiments on the influence of different manures on the proportion of gluten and starch in wheat. Wheat from common soil, neither rich nor poor, has 9·20 per cent. of gluten to 66·69 of starch; manuring with human urine yields gluten 39·10 and starch 39·30, and each different manure more or less alters, between these two extreme data, the proportions of these two important elements of the plant.

The foregoing instances afford an example of positive measurable variation in chemical constitution; and if we were better acquainted with the details of the life-history of any species, we should readily perceive the corresponding effects from the physiological side: we should see, for instance, how such and such chemical variation, which we can measure and weigh, is of real advantage to those which possess it; while those without it suffer definite and generally disadvantageous consequences.

In other cases we perceive the physiological effects, while we are not yet acquainted with the degree or even the nature of the variability. We all know that different animals of the same or of different groups react quite differently under similar unfavourable circumstances. We know, for instance, that it takes a much longer time to drown a frog than a reptile or a bird, and we understand why; we know also why a duck or penguin can withstand submersion a longer time than a hen or a quail. There are physiological reasons for these facts, and we are familiar with them. But in other cases such reasons must also exist, although we cannot tell what they may be. For instance, if different insects are subjected to the same process which is injurious to life, as in Gratacap's experiments,[1] considerable differences are easily perceived, though not explained. While the common fly withstands living in pure oxygen less than thirty hours, *Doryphora decemlineata* survives easily for three whole days, and *Colias phyllodoce* cannot stand it more than twelve hours. While the same *Doryphora* can live twenty-four or even forty-eight hours in pure hydrogen, a species of *Noctua* cannot live more than twenty minutes, nor *Pompilus unifasciatus* more than ten minutes. Why, we cannot tell, but there certainly is some physiological and chemical reason accounting for the fact.

Every physiologist knows well that the same poison exerts very different influences on different organisms. For instance, while brucine acts on dog or frog in the same manner as strychnine (although stronger doses are required than of the latter) it acts very differently on the common crab (*Carcinus maenas*), which exhibits no convulsions but only a peculiar movement of the external mouth-parts. Picrotoxin, similarly, acts on dog and frog like strychnine ; on the crab it induces a powerful contraction which is most characteristic.[2]

[1] Gratacap, *Vitality of Insects in Gases. American Naturalist*, vol. xvi., 1882, p. 1019.

[2] *Cf.* Henry de Varigny, *De l'Action de la Strychnine, de la Brucine et de la Picrotoxine sur le Carcinus maenas. Journal de l'Anatomie et de la Physiologie*, 1889, Paris. Also : H. de Varigny and Paul Langlois,

It may be argued that such instances are not very convincing: they concern very different species and genera; how can it be proved that such important variations occur within the same species, for there is the point? To this the answer is easy to give, and if we turn to any given species, we cannot fail to notice important differences. Take the human species, for instance, and consider the differences between man and woman, then those between the races of man, and finally between the different men of the same race.

As an instance of chemical difference between man and woman, here are the percentages of the principal components of bony structures in man and woman of the same age, after Milne-Edwards:—

	Woman.	Man.
Phosphate of Lime	62·15	58·32
Carbonate "	4·52	9·98
Organic Substances..............	33·33	31·70
Inorganic "	66·67	68·30

And it must be noticed that the differences vary according to the age of the patients, and even to the side of the body. While in the young the proportion of inorganic substances is smaller than in the adult, the bones of the right side of the body contain more

Sur l'Action de quelques Poisons de la Série cinchonique sur le Carcinus maenas (*ibid.* 1891), where similar facts are recorded.

than those of the left side, as H. Milne-Edwards clearly recognized in his investigation on animals, and such differences certainly obtain in man, as direct experiments have shown. And such differences are to be met not only between man and woman, between one side and another, but also between one part and another, lime-salts being more abundant in the thigh-bone than in the arm, &c.

If we turn to the chemistry of the blood, the same facts appear. Quetelet has analyzed the salts which are contained in this fluid, and has seen that the differences are as follows:—

	Man.	Woman
At one year	14·2	13·3
At ten years	37·1	34·4
At thirty years	98·9	78·4

There is more iron in male than in female blood (Boussingault); there are also more salts in male than in female, more in the right thigh of the duck than in the left one; there are more red blood-corpuscles in man than in woman (142 against 127, after Becquerel and Rodier, or 4·5 against 3·5, after Malassez), and so on; and all these minute or important differences in anatomical or chemical structure are accompanied by more or less important variations in physiology. Of these differences I shall give only one instance: it is admitted in forensic medicine that when man and wife are drowned together, the wife is considered as having died the last, because it is known that woman faints sooner, and has therefore more chances to survive than man, as experience has shown.

So much then for variability between the different sexes of the same species. If we now compare two races of the same species—mankind again—similar differences come in.[1]

While man and woman are respectively more liable to certain diseases, each race seems to offer different predispositions to the principal diseases flesh is heir to. Pathologists are well acquainted with this fact, and numerous instances of it are known. The following figures show the death-rate from marsh fever among Europeans (Englishmen) compared with negroes, in different countries :—[2]

	Death-rate per 1,000	
	Englishmen	Negroes.
Jamaica	101·9	8·3
Guiana	59·2	8·5
Trinidad	61·6	3·2
Sierra Leone	410·0	2 4

It has been sometimes said that negroes are entirely refractory to malarial fever: the fact is not accurate, but the figures show, at least, that the black race has,

[1] *Cf.* G. Delaunay's interesting *Etudes de Biologie Comparée basées sur l'Evolution et la Nutrition.* 1878-9. A. Delahaye, Paris.

[2] After Bordier, *Géographie Médicale.* Paris, 1884, p. 475.

for some reason or other, much less to fear from malaria than the "white devils." The same difference is found concerning tuberculosis: while it is more dangerous for Polynesians and negroes than for whites, it is more deadly for the whites than for the Mongolians, among whom it has been said that Thibetans quite escape the disease. The statement may have been exaggerated: at all events, it shows that the yellow race enjoys comparative immunity from tuberculosis. Similar instances are frequent among animals: not only do we meet with instances of diseases which are peculiar to some species only,[1] but within the same species some breeds enjoy immunity while others do not. For instance, Prof. Chauveau has shown that the sheep of Algeria enjoy a much greater immunity in respect to anthrax than those of France, and the same difference obtains among asses. This is a racial character, for foreign breeds living in Algeria do not acquire it; but the Algerian breeds transferred into Europe seem

[1] For instance, anthrax affects sheep especially, while it is scarcer among oxen, hogs, and horses, and is never met with among birds. To glanders the pigeon seems to be the only bird at all susceptible. Syphilis is peculiar to man, though it possibly may be seen in apes and hogs. Rats and mice enjoy an almost perfect immunity from diphtheria, and any number of similar cases may be found in any text-book on Bacteriology. Perfect immunity is rather doubtful, but it is quite certain that many virulent diseases, due to microbes, exist spontaneously only in a limited number of species, but may be conferred experimentally upon some or many others under experimental conditions.

to lose it gradually, so that the influence of environment appears to have something to do with it.

And now, if we consider men of the same race—and the same facts would appear if we were to consider individuals of any species of animals or plants—are we not all acquainted with facts of very notable variability? The same external influence acts quite differently upon them, and of four men standing in a draught, for instance, one will have pneumonia, the other rheumatism, number three a bad cold, and number four nothing at all but a temporary relief from the heat of the day.

The very same morbid influence—typhoid fever as an instance—acts differently, producing in the one patient gastric symptoms, while cerebral trouble is predominant in another. Every physician can furnish any number of similar instances, and can also show that while in every epidemic of every disease there are different forms of the same disease which are doubtless in correspondence with different personal variabilities or idiosyncrasies, these idiosyncrasies vary from one time to another, so that in one epidemic one form predominates, while in another some different form is most frequent. It thus seems that personal variation varies according to seasons and periods under unknown influences. Or else, if no variation is assumed to exist in the patients, there then exists some variation in the pathogenetic organism. For the present purpose this comes to exactly the same thing, our only point being to show that variability does exist in a marked manner.

Later on I shall have something to say concerning the degree of variability among pathogenetic organisms under different modes of culture or treatment; it is enough here to allude to the general fact of the attenuation of many sorts of virus which has led to the humane although as yet unexplained [1] practice of vaccination; but something must now be said concerning the external manifestation of this variability. Many bacteriologists have thought at times that it might be possible to transmute one micro-organism into another under definite circumstances, and we have all heard of Büchner's or other experiments concerning the relationship between the common hay bacillus and the typhoid fever bacillus, as well as of similar investigations. But investigators seem to think much too highly of mere morphological transmutations, and to have too much disregard for other transmutations which are in fact of much greater importance. They seem to be running after shadows while substantial reality lies disregarded at their very feet. Let us take

[1] "Unexplained" refers of course to the process by which a bacillus or bacterium, although in appearance unchanged, becomes incapacitated for the production of disease of a virulent type.

an instance. Here is that much investigated anthrax bacillus. Many bacteriologists have tried to determine morphological variations of the species through various experimental methods, hoping to see it assume quite different characters: but they have utterly failed. Professor Chauveau studying the same general topic of variability, has investigated it not on the morphological side, but on the physiological one. And he asks very appropriately whether a bacillus which has entirely lost its virulence, while retaining its morphological appearance—which is always very simple—has not varied more than a bacillus in which form might have varied while the pathogenetic properties had remained unaltered? The answer seems to me manifest, that variability of virulence is of greater importance than that of form and external appearance, especially in the case of such very simple and undifferentiated organisms, since this testifies to deep modifications in the chemistry and vital properties of the organism. How much more would this be evident if the new characters acquired by the organism were to remain unaltered from one generation to another, without it being necessary to provide permanently the special conditions or the peculiar environment which initiated the production of new characters? And this case is not of hypothetical nature; it really exists, and I have been a daily witness to it. Professor Chauveau, from long, and, as usual very careful experiments on *Bacillus anthracis*, has been able to show that while no known method can as yet, entirely destroy the pathogenetic influence of this micro-organism, nor confer upon it new and different properties, the pathogenetic influence may be destroyed to the extent that it can no longer harm the animals in which it makes itself the most easily felt. Such virus may be inoculated into guinea-pigs and mice without doing the slightest harm. But it has not entirely lost its properties, since it retains its vaccinal influence: while apparently no longer noxious to animals, while producing no disease nor pathological symptoms, it acts like a vaccine lymph, and confers immunity against the inoculation of virulent bacillus, as experiment shows. Again, these devitalised or altered bacilli, which only retain a vaccinal influence, may be made to acquire virulence of the highest type through very simple experimental processes. Lastly, these attenuated bacilli retain their new characters (of non-virulence and of mere vaccinal aptitude) as long as is required, without it being necessary to use particular methods of any sort, and, as M. Chauveau remarks, if one were to consider these bacilli in themselves, apart from their origin, and without knowing what they may be made to become under appropriate experiments, they might certainly be looked upon as a distinct species. Between this ultra-attenuated and the highly virulent breed many intermediate types exist, but they have less fixity, and their nature—as measured through their pathological effects—is less constant. At all events Professor Chauveau has succeeded in obtaining three types of *Bacillus anthracis*:

Firstly, the ultra-attenuated type, which has lost all pathological properties, and produces no disease even in the most delicate and appropriate animals (mouse, guinea-pig), but retains vaccinal influence, and can be used for vaccination of the same animals against the disease;

Secondly, the semi-attenuated type,[1] which kills some species of animals (rabbit and guinea-pig), but acts only as a vaccine in other larger animals;

Thirdly, the less attenuated type, which kills the rabbit, guinea-pig, and sheep, and plays the part of a vaccine only with the horse or oxen.

These different types may exist in Nature, and some facts go to show that some of them probably do exist.

The foregoing facts are of undeniable importance in regard to the question of physiological variability,

[1] This type may be obtained either by attenuation of the highly virulent type, or by partial revivification of the attenuated bacilli. The latter method is certainly preferable.

as they clearly show that while no difference at all can be discovered in the *external* appearance of the different types, considerable variation is present when *physiological* properties are taken notice of.

I have quoted this case at some length, because it is one of the most satisfactory yet obtained ; but similar instances are very numerous in bacteriology, where we perceive that very considerable differences of a physiological nature may exist although not perceptible from the morphological standpoint.[1]

Zoology also provides us with other facts which are of great interest. I refer to those which concern the considerable physiological difference which obtains between two closely related species, the brown and the green frog (*Rana esculenta* and *temporaria*), when subjected to identical experiment.

In 1881 Monnier[2] noticed that brucin and its different compounds act differently on these two species. In *R. esculenta* this alkaloid determines a paralysis of the motor nerves, and at the same time an increase in the excitability of the spinal cord. Of

[1] *Cf.* A. Chauveau : *Sur les Propriétés vaccinales de Microbes ci-devant pathogènes transformés en Microbes d'apparence saprogène. Archives de Médecine Expérimentale*, March, 1889, p. 161. Also, by the same author : *Recherches sur le Transformisme en Microbiologie pathogène. Des Limites, des Conditions et des Conséquences de la Variabilité du Bacillus Anthracis. Ibid.* November, 1889, p. 757.

[2] *Archives des Sciences Physiques et Naturelles*, Geneva, 1881.

course, the influence on motor nerves prevents the spinal influence from being detected, unless the experiment is performed in a particular manner. In *R. temporaria* the symptoms are quite different; tetanic convulsions appear, and if the dose is considerable, motor paralysis ensues later. The case is the same with the common toad.

Similar facts had been witnessed years before by many physiologists. As early as 1864 my much regretted master Vulpian[1] found that the same poisons operate differently on the circulatory system of the two species. Two years afterwards J. L. Prévost[2] witnessed facts confirmatory of the preceding, concerning the same animals when subjected to the influence of veratrin; the heart being arrested in one case, while it is merely slackened in the other. Then Schmiedeberg, in 1874,[3] took up the question, studying the influence of caffein, and saw that in *R. temporaria* caffein operates in determining a local action which gradually spreads a sort of muscular rigor, accompanied by a decrease in excitability; in *R. esculenta*

[1] *Sur les Différences entre les Grenouilles rousses et les Grenouilles vertes sous le Rapport des Effets produits par les Substances Toxiques et spécialement par les Poisons du cœur. Bull. Soc. Philomatique*, 1864, p. 94.

[2] J. L. Prévost, *Recherches Expérimentales relatives à l'Action de la Vératrine.* Thesis, Paris, 1886.

[3] *Ueber die Verschiedenheit der Caffeinwirkung an Rana temporaria und R. esculenta. Arch. f. Exp. Path. und Pharm.* 1874.

there appear, on the contrary, an increase in excitability and tetanic convulsions. But after two or three days the symptoms become similar in both species.

Pilocarpin, also, acts differently on the two above-mentioned species, as Harnack and Meyer have shown.[1] In *Rana temporaria*, pilocarpin induces paralysis; in *R. esculenta*, tetanus. Nicotin induces convulsions, followed by paralysis, in *esculenta*, while paralysis is the immediate result in *temporaria*. Similarly, pyridin induces tetanus in *esculenta*, and in *temporaria* the symptoms resemble those of picrotoxin poisoning. L. Wintzenried has confirmed Monnier's results on the different influences of brucin,[2] and Vulpian,[3] in a later paper, investigated the accuracy of the statements of both to his complete satisfaction. Lastly I may be allowed to quote a few lines from a paper by Messrs. Lauder Brunton and Cash,[4] which bears very exactly on the topic: "Johannsen [who was working under Schmiedeberg's direction] observed in the frogs with which he

[1] Harnack and Meyer, *Untersuchungen u. d. Wirkungen des Jaborandialkaloide, nebst Bemerkungen u. d. Gruppe des Nicotins. Arch. f. Exp. Path. und Pharm.*

[2] *Recherches Expérimentales relatives à l'Action Physiologique de la Brucine.* Thesis, Geneva, 1882.

[3] *Leçons sur l'Action Physiologique des Substances Toxiques et Médicamenteuses.* 1882.

[4] Lauder Brunton and J. Th. Cash, *On the Circumstances which modify the Action of Caffeine and Theine upon Voluntary Muscle. Journal of Physiology*, vol. ix. p. 112, 1888

experimented that the muscles became rigid at the place where caffein was injected, and this rigidity gradually extended to the rest of the body, but he failed to observe any tetanus. About three years afterwards, Aubert arrived at results entirely opposed to those of Johannsen, finding that caffein in the frogs with which he experimented produced marked tetanus, but very slight rigor. These contradictory results induced Schmiedeberg again to take up the subject, and he found that the discrepancy between the statements of Johannsen and Aubert was, to a great extent, due to the kind of frog employed by each observer in his experiments, the former having used specimens of *R. temporaria*, and Aubert of *R. esculenta*. According to Schmiedeberg, in *R. temporaria* caffein produces muscular rigor, without tetanus, the rigor beginning at the place where the poison is applied, and extending over the body so gradually that the muscles first attacked may be completely contracted and rigid, while others may be still slightly irritable. On the other hand, in *R. esculenta*, caffein frequently produces a violent and continuous reflex tetanus, without any rigidity of muscle other than that dependent on the tetanic contraction. It is only at a late stage of the poisoning, two or three days after the caffein has been given, that these differences between these two kinds of frogs become equalised, increased reflex action and even tetanic convulsions occurring in *R. temporaria*, and distinct stiffness of the muscles becoming observable in *R. esculenta*, although this stiffness never becomes so great as in *R. temporaria*."

The foregoing differences in the nervous system of these two very closely related species are again exemplified in other experiments, for Lautenbach[1] has shown that while the nerves of *Rana temporaria* are never excited by heat lower than 49° centigrade, those of *R. esculenta* are excited as soon as the temperature attains or exceeds 20° centigrade; and, on the other hand, a friend of mine, M. C. Contejean, a distinguished young physiologist, informs me that, according to his own experiments, considerable differences are noticeable in individuals of the same species which differ in colour. While frogs whose skin contains numerous pigment granules withstand for some time the effects of having part of their blood replaced by salt solution, frogs whose skin is sparsely coloured resist during a much shorter period. Again, the same physiologist informs me that while *Rana esculenta* and *temporaria* possess digestive glands in the lining of their œsophagus, the toad has none. Also, while the green and brown frog are provided

[1] *The Physiological Action of Heat. Journal of Physiology*, vol. ii. p. 1

with gastric glands which are exactly similar, the brown certainly produces a much larger amount of pepsin. These are differences which may be of great importance in the life of the animals—or may go with others yet unknown to make considerable differences—yet nothing in the external character of the animals would lead us to suppose that they were present.

Among the facts which illustrate this physiological variability I shall quote a few more. We all are acquainted with the fact that while many varieties of grape-vine are killed by some fungus or insect—*Phylloxera*, for instance—others do not suffer at all, or at least, as a rule, withstand the unfavourable effects. We know also that while the venom of a snake is deadly for most other snakes, it is not so for the same species, as Surgeon Waddell[1] has recently shown with great care; we have all heard of cases when the same plant is toxic for some animals and is not so for others. Willoughby in his *Ornithologia*[2] says that the common quail eats hellebore and water-dropwort (*Cicuta*) without danger; Daniel Duncan[3] says the same of the same animal; water-dropwort is not dangerous for goats, nor tobacco-leaves for oxen,

[1] *Are Venomous Snakes Autotoxic?* Calcutta, 1889.

[2] Francisci Willughbeii *Ornithologiæ, Libri iii.* 1676.

[3] *La Chymie Naturelle, ou l'Explication Chymique et Méchanique de la Nourriture de l'Animal.* Paris, 1681.

according to Cornevin; common valerian, when growing in marshes, is half or three-quarters less toxic than when it grows in dry soil, according to Pierlot;[1] and *Coffæa arabica* is killed by insect parasites (*Cemiostoma caffeolum*) which do not attack *C. liberica.*[2] Physiological variability is displayed every day among the individuals of the same progeny or of the same brood. Human twins are often very dissimilar in regard to physical and mental abilities. Among animals, the same cluster of eggs gives birth to tadpoles which differ considerably as to the epoch at which the tail is cast off and the limbs put in their appearance, although all external conditions of temperature, food, etc., are exactly identical. Among hogs, the last born of the brood is generally much weaker than the others; of the four or five dogs which are generated by the same parents, no two are alike in regard to the acuteness of the smell, swiftness, etc. Young turkeys of the same brood differ much, while there is but little difference between young ducks or geese. In my experiments on *Lymnaea stagnalis* I have often noticed very marked differences in the length and weight of animals from the same cluster of eggs, although they lived together, under exactly identical conditions, in the same aquarium. For in-

[1] *Note sur la Valériane. Bull. Soc. Bot. France*, vol. ix. p. 189.
[2] *Nature*, vol. xxiii. 1881, p. 541.

stance, in one case I have had animals of four or five millimetres in length only, while some of their brethren were over ten millimetres long, at the same age. In another case the difference between lengths was from three to ten, and in width from two to six. More detailed facts concerning this point I intend to publish shortly in a special paper, and I shall try to establish a comparison especially between the different individuals of the same brood parthenogenetically produced, or at least produced without previous fecundation by a different animal of the same species, for it is a positive fact that *Lymnæa stagnalis* and *auricularia* can yield fertile eggs without fertilisation by another individual having taken place. Similar facts are to be noticed among plants, as every one knows. Cornevin has provided a new demonstration of the fact by subjecting seeds of the same plant to the same conditions. He took thirty seeds from the same pea-plant, and steeped them some thirty hours in a solution of colchicin, and then planted them. Out of the thirty, twenty-five were entirely killed, and out of the five which survived, only three were able to develop a normal plant. Some years ago I had noticed similar facts, in a different manner. Four species of seeds were subjected to the influence of heat in the following manner: In one series the temperature was 80° Cent., and the seeds remained two minutes in the heated water; in the second, it was 90° Cent., and the time for heating was ten minutes; in the third, the seeds remained two minutes only in the water at 90° Cent. The final result was that in the first series only one sort of seed was able to withstand the effects of the heat: two out of ten seeds germinated. In the second series one seed only germinated (*Lepidium sativum*), and in the third series two species germinated (cress and radish).

The same result was obtained when I planted seeds of different sorts which had been subjected to the influence of a solution of sulphate of copper in water, during a period varying from one to sixteen days. Some species were very sensitive, and did not germinate at all. Such was the case with radish and mustard seed. Others were less sensitive, and *Lepidium sativum* germinated even after ten days' immersion, but then only some of the seeds were able to germinate, and the proportion of those that were killed increased when the duration of the submersion was greater. Lastly, flax seed withstood sixteen days' contact with the copper solution, but the number of germinations was larger among the seeds which had been immersed four or eight days only, than among those which had remained the whole period. More recent experiments of the same sort with copper and strychnine have yielded the same results, and show that: 1stly, great differences occur in the sensitiveness of different species in regard to the same reagent; 2ndly, marked differences occur also in regard to the sensitiveness of different individuals ot the same species in regard to the same reagent.

I am at present engaged in repeating and developing this series of experiments, as they help to illustrate physiological variability, and can be used also for investigations in selection. Of course, in many cases the result may be interpreted as due to variation in the thickness of the seed-envelopes, or in the bulk of the seed itself; but whatever the cause of the differences in resistance may be, the main point is to demonstrate palpable variations, that is, a firm basis for the operation of selection; and, on the other hand, the differences in thickness of the seed-envelopes is not a mere anatomical fact, it is also a physiological character of the plant.

Observation has, however, already provided very interesting facts bearing upon this question of physiological variability in regard to the influence of poison. We know that this influence varies according to many conditions, among which are the following:—

First, *the mode of introduction*.—Many poisons are devoid of danger when introduced through the alimentary tract, while they are very dangerous when introduced into the blood or under the skin, because in the first case they are slowly absorbed or are destroyed by their passage through the liver for instance.

Secondly, *the age of the animal under experiment.*—This is easily ascertained, and the fact is recognised in medical practice, where the dose of the same drug varies from one-sixteenth to one, according as it is given to a young child or to an adult.

Thirdly, *sex.*—Females are more sensitive than males, and accordingly are unable to withstand doses of drugs or poisons which males resist.

Fourthly, *species.*—Belladonna is very active on man, cat, dog, and birds; less so on horses and hogs; nearly without effect upon sheep and goats, and especially on rabbits. Within the same class the same differences obtain; the rat is, generally speaking, more sensitive than the guinea-pig, and the rabbit is less so than the latter; and the ass is more sensitive than the mule and the horse. Hemlock (*Conium maculatum*) is not injurious for the lark and quail; although they may eat so much of it that their flesh may be poisonous for Carnivora, they themselves sustain no injury. The red corn poppy is not injurious for rabbits, and it is said that Euphorbia may be eaten without inconvenience by goats, although it confers toxic properties on their milk. Laburnum is not toxic for goats, rabbits, and hares. Within the same species, marked differences obtain between varieties. The Pyrenean sheep feed without injurious effects on the leaves of *Quercus tosa*, while imported Southdowns are killed by this plant. *Lychnis Githago* is very unequally toxic for different species; calves require 2 g. 50 centigr. per kilogram of weight of flour, while the hog requires only one gramme, and the dog ninety centigrammes, to be killed. Similarly, while the horse is killed by two kilograms of fresh hemlock, the ox requires at least double this amount. *Cyclamen europæum* is very dangerous for man, and hardly at all for hogs; it kills fishes and some other aquatic animals, while many Crustacea and different insect larvae do not suffer at all.

Of course, there are many reasons for these differences in the influence of poisons, and in some cases we perceive them easily. But in others there is some intimate unknown reason which would require investigation, such as the case of the Pyrenean and Southdown sheep. Even if there is merely here a case of habitual adaptation—a mere verbal explanation—such as is very often met with in experiments on the influence of toxic media on animals or plants, or on the influence of pathogenetic organisms on higher beings, there must be some difference between the organisms which perfected methods will be able to detect. There may be here some acquired character, some peculiar variation which has become inherited.

On the other hand, turning to plants, similar facts of physiological or chemical variation are noticeable. The same plant is more or less toxic, according to its age or period of life, and to the various parts considered. While some plants are toxic in all their parts, such as meadow saffron (*Colchicum autumnale*) hellebore, squill (*Scilla maritima*), Pride of India (*Melia azedarach*), others are dangerous only in their roots, such as *Atractylis gemmifera*, or in their roots and stem, such as the common dog violet; or in their aerial parts only, such as many Solanaceae; or in their bark and leaves only, as is the case with the yew; or in their flowers, as buckwheat (*Polygonum fagopyrum*); or in their fruits only, as, for instance, in the castor oil plant; or even in one part only of their fruits. Other plants are dangerous in all their parts save one, the fruit for instance in sumach (*Rhus coriaria*).

Young leaves of the yew are much less toxic than aged ones, which is the reverse of the case generally observed. *Clematis vitalba* displays the same fact. *Ranunculus ficaria*, while at first feebly toxic (through its leaves), becomes afterwards more so, but after flowering it loses much of its poisonousness, and the same is the case with *Caltha palustris*. *Aconitum napellus* is very toxic in warm climates, while in the northern or cold regions it is harmless; cultivation also diminishes its injurious properties in a marked manner. Of course, these differences in the physiological influence of the same plant can only be ascribed to variations in the protoplasmic or physiological processes of the plant, so that they afford very good instances of physiological variation. These are a few instances among many ; but they are enough.

We may then draw the inference that between different species there are not only external differences which may seem more or less unimportant —although we must believe them to have some usefulness—there are also other differences of a physiological nature. As yet we are acquainted with but a few of them, the matter having been but very slightly investigated, but we may rest assured that in fact they are numerous, and often very considerable. While they may seem, and perhaps are, in many circumstances of small importance, they may, in others, become of the highest interest, and determine life or death. This is the main fact I wish to illustrate, and I entertain no doubt whatever concerning the novel, and certainly startling, character of the results, which will be put forward when some competent physiologist and chemist shall have devoted some time to the comparative investigation of two species, or better of two varieties of the same species, from this point of view.

With chemical investigation such as Armand Gautier's, and physiological experiments such as those which Vulpian originated, a great deal must certainly be discovered, and herein lies a new way to the investigation of the much debated question of variability. Even if it does not lead us any further, this line of study must bring us to this much needed requisite: an answer to the oft-repeated question, What is a species? Species, we are always answered, are endowed with such and such characters, and when we come to look at matters, we find out that two forms—say of Crustaceans or Rotifers—are considered as specifically distinct because the one has a few hairs more than the other on this or that appendage, or because the form of such or such part is more elongated in one group and rather square in the other. This answer is doubtless very satisfactory, since so many are amply satisfied—or seem to be so—with it; but in future we shall certainly come to define species not only by means of their external anatomical characters, but also in terms of a large number of physiological and chemical differences which have hitherto been entirely disregarded, which are but slightly apparent, and can be made quite clear only by means of careful and methodical investigation with appropriate methods: to the morphological diagnosis, a chemico-physiological diagnosis shall be added, whose importance shall often, if not always, be much greater than that of the former.

As I have dwelt at some length on the topic, I might be expected now to dismiss it, after having shown that variation is to be found in every part of the organism, even in its deepest and most secluded nooks; but there are still some important matters to be discussed. Concerning physiological variability, many experiments might be suggested, and some have been made which show that physiological variations may be experimentally induced. Professor Jean de Tarchanoff,[1] investigating the physiological condition of the brain of new-born animals, has shown that this part of the nervous system does not at birth answer with the normal activity to external stimulations. While Fritsch and Hitzig, afterwards followed by Ferrier and a number of physiologists, established the fact that stimulation of certain parts of the mid-brain is followed by motor reactions in various parts of the body, Tarchanoff has shown that during the first days of life, and especially among the animals which enter upon life with closed eyelids, no motor reaction at all is produced. This is an important feature from the

[1] *Sur les Centres Psycho-moteurs des Animaux nouveau-nés et leur Développement dans différentes Conditions. Rev. Mensuelle de Médecine et de Chirurgie*, 1878.

stand-point of physiological variability ; but there is something more that Professor de Tarchanoff has seen and proved. The brain of the new-born guinea-pig, for instance, does not answer to electrical stimulation during the first days doubtless because it is not yet developed enough. If it could be artificially developed or retarded in its growth and progress, it is likely that the moment at which electrical excitability exists could be hastened or retarded, and that, similarly, the moment at which the eyes open could be at will hastened or retarded. Methods are not wanting to allow the performance of the experiment, and Professor de Tarchanoff has artificially hastened the development by means of some phosphorus mixed with the food, and by means of cerebral hyperæmia induced by occasionally hanging the animals head downwards, while the reverse position and a slight degree of alcoholism have been enough to render cerebral development much slower. The result was exactly what might have been expected ; when the development is retarded, excitability and sight are retarded, and another fact is noticeable : locomotion is also more difficult and is attained later, because the causes which retard the cerebral development inhibit more or less all functions which are dependent upon the brain. Professor de Tarchanoff thinks that it might be possible, by repeating the same experiments on the successive generations, to hasten or to retard in a marked manner the nervous development. This may be: at all events the facts are interesting, and show that methods may be devised through which an influence may be exerted on constitutional peculiarities. Such facts give us hopes of seeing where the cause of physiological variability may be sought.

To all that has been said concerning variability, morphological and physiological, we must add that this variability is not limited to any group of plants or animals, to any time of life, nor to any geological epoch. From the very beginning of life variation is apparent. Take a number of eggs of the same frog, in conditions apparently identical: some are early and some late in developing into tadpoles. Tadpoles acquire their limbs and lose their tail at very different intervals, although all live in the same aquarium, are born from the same parents, and enjoy the same food; and if you put some of them into the same toxic solution, some are certain to die much earlier than the others, and if the time during which they remain in the dangerous medium is not too long, some certainly will recover, while the others die. Of the whole progeny of the same parents, whether two or two hundred young are produced at one time, no two are exactly alike: noticeable differences exist, if not in the external, certainly at least in the physiological characters. Of the same brood of pigs the last born is generally weaker; of twin lambs one is always heavier, etc. And if any two animals of the same brood are selected and observed in after-life in order to estimate the differences between them in physiological respects, although they live under exactly similar conditions, considerable differences appear in their weight, height, etc. It is enough to note these facts, without dwelling any longer on the topic. But while variability exists everywhere, in every species, in every individual, it must be also admitted that its sum total is very variable. Some species are much more liable to variation than others, as all horticulturists can testify. But, in fact, truth seems to require another statement. All species are probably equally variable, but all are not at the same time in conditions which are equally suitable for the production of variation. Take the case of any imported species of plants which is cultivated in our European gardens. At first no variability is apparent, and many horticulturists give up the hope of improving or of modifying it in any way. But those who have more experience go on with their plant, knowing very well that while at first no marked variability appears, very important variations may suddenly appear at a later stage, after cultivation has been continued for some time. Such has been the case with the common pansy, as Weismann has noticed, and it often happens that the foreign species, after keeping very constant for some years, begins all of a sudden to vary considerably and in many directions, thus giving birth to an unexpected number of varieties. These facts must be kept in mind, and they go to show that one must not be hasty in deciding whether any species is or is not liable to vary more or less than another: variability is doubtless itself variable, according to influences of which we are more or less ignorant. Palaeontological facts are known which are of value in that they show that variability has existed in the past as well as in present times, and that it has been more apparent in some species or genera than in others. Mr. Hyatt provides a good instance of such facts, in his paper on the Steinheim fauna,[1] where a large number of fossil varieties of *Planorbis* are met, all of which most probably descend from four principal varieties of one single species, *P. laevis*, unless we prefer to believe that every single one of these principal or secondary varieties has been called into existence by an equal number of special creations. H. Filhol has also given valuable facts bearing on this matter, in his investiga-

[1] See *The Genesis of the Tertiary Species of Planorbis at Steinheim* (*Bost. Soc. Nat. Hist.* 1880), and *Transformations of Planorbis at Steinheim* (*Am. Naturalist*, 1882, p. 441.) *Cf.* also Stearns, *Proc. Acad. Nat. Sc.*, Philadelphia, 1881.

tions concerning the fauna of the Quercy phosphorites. For instance, he shows that at St. Gérand le Puy, *Amphicyon*, that presumed ancestor of our dog, has a great tendency to vary in height, strength, form of the head, and in the teeth, while a number of other genera exhibit the same tendency in all the parts of the body: such are *Palaeotherium*, *Eurytherium*, *Acerotherium*, *Palaeochaerus*, *Cynodictis*, *Hyaenodon*, *Cynohyaenodon*, *Achirogalus*, *Necrolemur*, *Adapis*, *Pseudelurus*, etc.; and so far as dimensions are concerned adult fossil remains are often met which indicate that the differences were often from one to two. Nor can all these differences be explained as of mere age or sex.

As I have already noticed, we know nothing of the causes of variability. We can measure and appreciate it to some extent, but we do not know how it is determined in most cases, save in those where it is consequent upon some modification in external environment, and to these we shall have to revert further on. Must we believe in some innate "tendency towards variation"? Buffon thought that every species tends towards degeneracy, while Lucas believed in some innate tendency towards the production of new forms, which is, of course, in constant warfare with conservative heredity. Naudin and Naegeli similarly believe in internal forces, and Delbœuf, the eminent

Belgian thinker, clearly expresses himself in favour of the belief in a "tendency to betterment," some permanent cause with an unlimited aim, which will operate as long as our planet lasts, and as long as something may be imagined which is more perfect than that which exists. Clearly such a *tendency* is undemonstrable. For even if things could be seen to progress as they ought if this tendency exists, this is no proof of the existence of the tendency.

On the other hand, many naturalists appeal to external influences in order to explain variation, and they certainly can point to many facts which are of weight, as we shall show further on. Conditions which seem, at first sight, to have no importance, often exert considerable influence on individuals or groups of individuals. But a discussion of this matter would require more time than can be afforded here, and I must dismiss the subject after having briefly stated the state of opinion concerning the cause of variation. I only wish to add that while, in the earlier period of Darwinism, much stress was laid on the slowness with which variation appears and operates, there is at present a tendency to a change of opinion. This change has been well expressed in a recent paper by W. H. Dall, *On a Provisional Hypothesis of Saltatory Evolution*.[1] The writer bases his argument on the oft-

[1] In this connection Mivart's *Genesis of Species* may also be cited.

repeated fact that in many cases while two forms of life, fossil and extant for instance, seem to be directly or genetically related to each other, some link seems to be missing through which the transition from the one to the other could be effected. In Mr. Dall's opinion no links are really missing in such cases, because none have existed ; the much asked for transitional forms have never lived. He supposes that sudden and considerable variation may take place under the cumulative influence of a number of small causes which remain a long time inoperative, but become efficient when at last one more slight influence is added, which acts like the straw that breaks the camel's back. A good deal may be said in favour of this view, and many facts from zoology and palaeontology go to support it, and it may be that there is more truth in this manner of explaining some facts than in the former opinion. At all events abrupt or sudden variation is not an unknown fact. Some years ago, in France, a variety which stands between *Begonia Schmidtii* and *B. semperflorens* made its appearance quite suddenly in different points at the same time ; at Paris, at Poitiers, at Lyons, at Marseilles, etc. At Lyons the fact was very striking, as 500 specimens of the plant suddenly exhibited the new characters, and these characters were found in a plant which had been a year previously isolated in order to prevent hybridisation.[1] A year later, a similar fact was observed by another horticulturist when all of a sudden a large number of plants of *Narcissus*,[2] which had met with adverse circumstances and had required some quantity of chemical manure, began to bear double flowers.

Among animals, sudden variation is not uncommon, mainly through the disappearance of some external characters. In Paraguay, during the last century (1770), a bull was born without horns, although his ancestry was well provided with these appendages, and his progeny was also hornless although at first he was mated with horned cows. If the horned and the hornless forms were met in fossil state, we would certainly wonder at not finding specimens provided with semi-degenerated horns, and representing the link between both, and if we were told that the hornless variety may have arisen suddenly we should not believe it, and we should be wrong. In South America also, between the sixteenth and eighteenth centuries, the Niata[3] breed of oxen sprang into life, and this

[1] *Cf.* A. Carrière, *Spontanéité simultanée. Rev. Horticole*, 1882, p. 534.

[2] M. Poulin, *Particularités Végétales. Ibid.* 1883, p. 342.

[3] Niatas are occasionally met with in Europe, and many are living at present. They are descendants of common cattle and sheep, and no particular reason for their peculiarity is offered. This sort of variation thus seems to be common.

breed of bull-dog oxen has thriven and become a new race. So in the San Paulo province of Brazil a new breed of oxen suddenly appeared which is provided with truly enormous horns, the breed of Franqueiros, as they are called. The Mauchamp breed of sheep owes its origin to a single lamb which was born in 1828 from merino parents, but whose wool, instead of being curly like that of its parents, remained quite smooth. This sudden variation is often met with, and in France has been noticed in different herds.

Other cases of the same sort might be quoted, showing that, as some naturalists feel inclined to believe, sudden variation is much less uncommon than has been generally supposed.

Having dealt at some length with the facts concerning variability, we may briefly sum them up in the statement that a certain degree of variability is met with in every species, fossil or existing, animal or vegetable, and that this variability is not found merely in external characters, and does not affect only the colour, dimensions, or integumentary appendages, but that it extends also to the most deeply seated parts such as the viscera and bones of animals, and reaches even further, since physiological variability is merely the external expression of physical and chemical variations in the most intricate recesses of the various parts which make up the organism.

Here ends the first group of facts which lie at the basis of experimental transformism, the facts of variability in the state of nature. We may now proceed to examine the second group, that of the facts of variability under domestication and culture.

LECTURE IV

Summary.—Second Group of Facts supporting Experimental Evolution ; Facts of Domestication of Animals ; their Departure from the original Wild Type as seen in the cases where the latter still exists ; Much more might be done in this way, and many new Resources discovered ; Domestication has caused Animals to vary in all parts of their Organism, from Weight of Brain to Length of Digestive Tract. Third Group of Facts : Cultivation of Plants ; its Influence ; the Departure from the original Wild Type ; Variation in all parts of the Plants from Roots to Flowers ; Numerous Varieties of the commonly-cultivated Vegetables. Fourth Group of Facts : Influence of Environment on Structure ; Closeness of Agreement between Environment and Organism ; Beudant and Raulin's Experiments ; the Author's Experiments ; Dareste and Teratogeny ; Pouchet, Yung ; Facts and Experiments ; Pierre Lesage ; Schmankevitsch ; Weismann's Criticisms.

We positively know that some of our domestic animals and cultivated plants were companions of prehistoric man. It even seems that among the older nations, now dead and gone, some animals were subjected to domestication which have since been totally abandoned and allowed to run wild. The beech-marten and other Mustelidae were in part domesticated by the ancient Greeks in order to destroy rats, and in ancient Egypt *Lycaon pictus*, a wolf, was tamed, and during a long period made use of as a dog. Similarly the jackal and lion were tamed and used for hunting purposes, and many species of antelopes were domesticated, being shut up at night in stables, while during the day shepherds led them about like ours with cows or sheep. But the domestication of these various species was soon abandoned, for while we notice these pictures in many sepulchral monuments of the earlier periods, they are no more to be seen in those of the more recent epochs, these animals having doubtless been superseded by others more useful and more easily tamed.

If we call *domestic* those animals which remain voluntarily in man's dependence, while being of special use to him, we perceive that their number is very small. Excluding, of course, animals such as oysters, clams, bees, silkworms, trout, salmon, and the fishes which are the object of the pisciculturist's attention, domestic animals are wholly comprised in two classes, those of mammals and birds. Among the birds we have the ostrich, swan, goose, duck, turkey, pheasant, peacock, guinea-fowl, common fowl, and pigeon. Among mammals: guinea-pig, rabbit, cat, dog, hog, horse, llama, camel, reindeer, sheep, buffalo, and ox, and partly also the elephant. If to these twenty-three species we add some fifteen or eighteen species which are more or less domesticated by the inhabitants of other parts of the world, we obtain a sum total of some forty, let us say fifty species.

The wild forms of these domestic species are nearly all known and living. The two genera of ostriches, found in Africa and America, are among the most recently domesticated forms, and in fact have been domesticated only to a slight extent. Wild swans are yet found in Sweden and Norway; geese are met in Asia and Europe; ducks are found in the greater part of Europe; the turkey is an inhabitant of the New World, whence it was imported but a few centuries ago; pheasants—but yet partly domesticated—live wild in Central Asia; the guinea-fowl has been found wild in Africa by De Brazza; the peacock inhabits India, Java, and Sumatra; the common fowl is the descendant of the Indian *Gallus Bankiva;* the rock-pigeon is the ancestor of all our varieties of domestic pigeon.

However, wild guinea-pigs are no longer found in South America, where they existed some centuries ago; and of our hog only doubtful ancestors exist in Asia and the Sunda Islands (*Sus vittatus* and *papuensis*). Wild horses most probably still exist. Pliny, Varro, Strabo mention them, and Erasmus Stella and Rosslin speak of wild horses in 1518 and

1593 in Prussia and Alsace, while Gaspard de Saunier mentions their existence in 1711 in the forests between Dusseldorf and Wosel, where he had hunted this animal. And a few years ago the celebrated Russian explorer, Prjevalsky, discovered in Dzungaria a wild species, which has been named *Equus Prjevalskii*, and which seems to occupy a place between the horse and the ass. Wild asses have been seen by Prjevalsky in Kokonor, and by others in Africa, near Obock. In Egypt Marco Polo and Pallas found the wild camel, which, nearer us, Prjevalsky discovered near Lake Lob-nor. The wild form of the llama seems to be the guanaco; the American caribou seems to be the wild form of the reindeer, and the Asiatic moufflon (*Ovibos musimon*) is considered as the wild form of our sheep, while wild goats are found in the Himalaya, buffaloes in Asia and Africa, and the yak in Asia, and oxen in Africa only, as they have now disappeared in the wild state from Europe and Asia, where they formerly existed. Some points are, however, doubtful. Concerning the sheep, we cannot exactly tell whether our domestic form is derived from any one of the present wild forms or from the crossing of two or more, or from an extinct species; and as to horses, while they were very abundant in the wild state during the neolithic period, when they were used as food by prehistoric man, yet we cannot tell exactly whether our horses are derived from these wild ancestors, nor whether the descendants of the neolithic horse really persisted till some two or three centuries ago.

Of our present domestic animals, most have been domesticated for a very long time, and while we feel assured that domestication is merely a matter of time and patience, we wonder at the fact that civilised man has been content to accept the legacy of his savage ancestors, and has done so little to increase it. At the present time man, in the civilised state, does not possess more than some twenty species of birds and some twenty of mammals in the domesticated state. It is true these species meet most of his requirements; but who would venture to say that there is not much profit in store for him if he were to increase the number of his domestic friends? While man in all parts of the earth is—or ought to be—eager to discover new vegetables, or at least new fruits, and to cultivate and export them, how is it that our animal resources remain so very few in number? I do not explain the fact, but merely call attention to it, and many, I hope, will concur with me, and think that much might be done in the way of acclimatising useful animals—useful as meat or milk-givers or wool-producers—of domesticating them, and of discovering new resources from which mankind may derive many benefits. Our acclimatisation societies do not yet understand the real aim they ought to pursue, and they are much more occupied with the pursuit of new or rare animals, which are seldom of any use, than in that of the useful species which certainly exist. We cannot believe that our present domestic animals are the only ones which can be of use to us; there are many others which may be profitable, but instead of trying to domesticate these, we often wantonly destroy them. Such is the case at present with the American bison, which competent writers state to be an excellent flesh-producer. While it roamed in millions some years ago, not 300 could be found to-day on the whole surface of the earth. I do not wish to open here a chapter recording the foolish and cruel dealings of so-called civilised man—the real civilised man occurs in infinitesimal proportion among the featherless bipeds—with animals, for a whole book would not suffice, but whoever is acquainted with the facts cannot help regretting this stupid and sinful waste.[1]

Revenons à nos moutons. Domesticated animals are few in number. There are good reasons, however, apart from that which has been already given, for this fact.

[1] For interesting details as to domestication of animals in India, see J. Lockwood Kiplings's recent volume, *Man and Beast in India.* Lond. 1891.

As Cornevin[1] remarks, some essential conditions must be fulfilled before any species can be domesticated. The principal requirements are the following:—

First, *Sociability*.—All domestic animals, as observation at once shows, live in societies, in herds—all, save one, the cat; the much-abused cat, the much-praised cat, the most independent and self-centred of our domestic friends. This gregariousness of all our domestic mammals and birds, pigeon or fowl, hog or ox, shows that if we are to attempt new domestications we must direct our attention towards gregarious animals.

Secondly, *Tameness*.—Only docile animals can be domesticated, and all animals cannot be tamed with the same facility. Some retain a very considerable element of savageness, whatever care may be taken to tame them. Tameness is the result of kind treatment, and of the habit of being constantly with man and while some species seem untamable, the greater proportion may be more or less tamed.

Thirdly, *Preservation of Fertility*.—Many animals, even in their own country, become sterile under domestication or captivity; they have no progeny, or but a very small one. It also often happens that the transfer to a new country induces temporary decrease of fertility. The common fowl when intro-

[1] *Traité de Zootechnie générale*, 1891.

duced into Colombia was at first nearly sterile; but this condition does not last in most cases. It is useless to try to domesticate animals which do not multiply under domestication or captivity; for even if acquired characters are in general not inherited, it is certainly true that the progeny of a wild species is much more difficult to tame and domesticate than that of the same species in the domestic state. And if this hereditary transmission of the acquired character of tameness did not exist, domestication would prove impossible. In fact, domestication may be more or less complete; while some animals can hardly revert to the wild condition, in many others domestication has not existed long enough to dispel all natural tendencies towards wild life. Such is the case with the llama, the reindeer, the rabbit, goose, duck, and some others, and many instances might be given. Cornevin, for instance, has seen a flock of geese, which for over thirty years had lived in the same yard, arise in the air on seeing a flock of wild geese overhead, join them, and fly away for ever.

The foregoing conditions must be fulfilled when the domestication of any species is contemplated, and we know enough of many animals to be assured that the number of our domesticated species might be easily increased if we only took some care.

As I have already said, the domestication of animals has been the work of man. The study of the prehistoric vestiges of our ancestors shows that domestication began at different times in different localities. Up to the palaeolithic epoch, man possessed no domestic animals. He hunted and killed the reindeer, aurochs, and horse, but ate them instead of domesticating them. In the Solutré encampment vestiges have been found of over 40,000 horses; but all the bones are broken and shattered, the animals have been killed by the hunters and immediately eaten, and the flint arrow-heads have been found embedded in the bones, showing beyond doubt what use was made of the horse by palaeolithic man. It is only during the neolithic period that domestication began. The dog was one of the animals first domesticated, and he was already in use during the Kitchenmidden period. The domestication of other animals followed at very irregular intervals and in very different countries, and it is nearly impossible as yet to ascertain exactly at what period or epoch our domesticated animals entered upon their present condition. In fact it matters little, the principal point of interest for us being that the domestication of animals has been the work of man. This is sufficiently proved by all ascertained facts.

The next and most important point is that domestication has been a means of transmutation. The species which have been domesticated have gradually departed from the feral ancestral type, and among the domestic descendants man has, by his industry and through the use of methods which he has discovered, established different types. It matters little from our point of view whether the ancestral form of our horse was one or many, for if many, they at all events had doubtless one common ancestral form, and we may consider all our present types and varieties of any one species as having, in each case, one single ancestral form from which, as we see, they have considerably and in manifold ways departed. Compare, for instance, the Shetland pony, the racehorse, and the heavy drayhorse of the north of France; compare the numerous and very different breeds of sheep, of oxen, of hogs, of dogs, so useful in different ways. In some cases the wild form has disappeared, and in those where it yet exists we perceive to what extent domestication has modified and transformed the descendants of the original stock. Whatever may have been the ancestor of our dogs, the differences the existing breeds exhibit are marked enough to illustrate our thesis.

The modifying influence of domestication makes itself felt in all parts of the organism, even in its depths and in its less external characters. For instance, if we measure the skull-capacity of wild and domestic forms, we find, as Cornevin has well shown, a marked difference in favour of the former.

Animals.	Skull-capacity.	Difference in favour of Wild Form.
Wild Ass of Persia	521	cubic centimetres 71
Domestic "	450	
Abyssinian Ox	479	" " 47
Domestic African Ox	432	
Moufflon	240	" " 118
Sheep	122	
Boar (Europe)	190	" " 13
Hog	177	
Cochin China Boar	162	" " 12
Sus vittatus	181	" " 31
Domestic Chinese Pig	150	
Wolf	142	" " 26
Dog	116	
Jackal	82	" " 6
Italian Greyhound	76	
Wild Rabbit	9·4	" " 1·9
Tame Rabbit	7·5	

At the same time wild animals have heavier brains than the domestic forms. Domestic animals are also heavier and more fleshy, and the tendency towards flesh-production increases with domestication. At the present day our oxen are three times heavier than the same animals, at the same age, four or five centuries ago. In France, since the beginning of the century, oxen have more than doubled their weight; in 1808 the average was 300 kilograms, now it is 700. Domestication also reacts on the length of gestation, and even among domesticated animals, compared with one another, there are marked differences.

Cornevin shows that this period in the cow varies in length from 288 to 277 days, a difference of eleven days. The following table gives details :—

Cows.	Length of Gestation, in days.
Schwytz	288·75
Freiburg	287·50
Auvergne	286
Tarentese	282
Flemish	280
Durham	280
Valais	279·65
Jersey	279·40
Ayr	279
Dutch	279
Breton	277

The same is observed among sheep, according to Nathusius and others.

Merino	150·3
Southdown	144·2
Southdown Merino	146·3
¾ Southdown	145·5
⅞ Southdown	144·2

It seems useless to do more than point to the modifying influence of domestication. While we must assume that domestic animals were not specially created for man, and that, as palaeontology, zoology, and anthropology show, they have been evolved by man himself out of wild existing species which in some cases are yet living in the feral state, the mere comparison between the different types which have been evolved out of the original stock, at different times—not always very remote—is enough to show the influence of domestication. I may add that the divergence might have been even more marked. For we must remember that man has always had in view his own profit, excluding all other considerations; the selection which he has consciously or unconsciously performed has always been directed along the same lines; and all variations which were of no direct and positive use to him have been swamped. This is well displayed by the cases in which man has been less subservient to utility; in pigeons, for instance, where the production of new types or varieties has been the business of amateurs and *dilettanti* who pursued no particular useful object, but merely wished to obtain the greatest number of variations, there are more different types than among other species of which only a small number of useful characters—useful to man, not to the animal itself—are required, all other characters being considered useless and uninteresting, and therefore neglected, the result being that the variations are swamped by intercrossing.

As Geoffroy Saint Hilaire[1] observed, there is a

[1] *Histoire Naturelle générale des Règnes Organiques*, vol. iii., p. 133

definite relation between the degree of civilisation in man and the condition of his domestic animals. Many savages at the present time are yet devoid of domestic animals, although they possess hunting and fishing implements, and have even some cultivated plants. "Where man is much civilised, domestic animals are varied, either as species or as races of the same species; and among races many exist which differ greatly from one another and depart greatly from the original type. On the contrary where man is himself not far removed from the wild condition, his animals are also very near to the feral state; his woolless sheep is nearly a moufflon, his hog resembles a boar, his dog itself is no more than a tame jackal, and so on with the others if he possesses any more."

I think none will dispute the accuracy of this statement.

Some animals have, from the Cambrian to the Quaternary epoch, varied but in a very slight degree, such as *Nautilus*, and some fossil forms which lived in cretaceous times, are yet living in the depths of the present seas. Generally speaking, according to Gaudry and Lyell, the group of molluscs is less variable than that of mammals, and among molluscs Gasteropods vary more than Lamellibranchs, and among Gasteropods, Siphonostomata have varied more than Holostomata. Among mammals, Artiodactyls and Perissodactyls afford good illustrations of the variability of the group, and the number of fossil forms discovered in the Tertiary strata is very considerable. Among domestic animals there are very different degrees of variability; Cornevin gives the following list of domestic birds and mammals, where the animals are arranged according to the order of decreasing variability :—

Pigeon.	Hogs.
Fowl.	Dogs.
Duck.	Oxen.
Pheasant.	Sheep.
Goose.	Rabbits.
Guinea-fowl.	Hares.
Peacock.	Donkeys.
Swan.	Camels.
Turkey.	Goats.
Barbary Ducks.	Guinea-pigs.

Of these species, three which vary but very slightly (turkey, Barbary ducks, and guinea-pigs) are recent importations and will perhaps vary more in later periods. At all events guinea-pigs have already sufficiently varied to have become infertile with their original stock.

The modifying influence of domestication on animals has a parallel in the influence of cultivation on plants, and to these we must now turn, to call attention to the important transformations which wild plants have undergone under cultivation at the hands of mankind.

Man seems to have cultivated plants before he domesticated animals, in some cases at least. In the lake-dwellings of Lagozza, in Italy, belonging to the neolithic epoch, while flints, terra-cotta ware, and even crude linen are found, no bones are seen at all; it would seem that the inhabitants were strict vegetarians, and wheat, acorns, nuts, apples, and other vegetable products have been discovered, carefully stored away. The people who lived there, ages ago, were already well advanced—comparatively speaking—in agriculture, before they began domesticating, or perhaps even eating animals. But in other cases man seems to have begun with animal domestication before he cultivated plants. He began with the cultivation of the species which were abundant in his familiar haunts, and were of considerable use to him in one way or another, especially as food for himself or for his animals. Has he paid more attention to vegetable food, or is some other reason to be called for? At all events, the number of plants which are cultivated in the different parts of the earth is much larger than that of domesticated animals. Truly, cultivation is an easier process than domestication, and while the latter is always attended with many difficulties, the former requires less care. In all parts of the world some vegetable species are predominantly cultivated: in India rice, in Europe wheat, in Oceania the taro-plant, and so on. And while little more than nothing is done at the present time to increase our animal resources, much is being done every day to cultivate new plants, for food, pleasure, or drugs. But more remains also to be performed, and I doubt whether the hundred or more species which Sir Joseph Hooker pointed out in his *Flora Tasmaniæ* as being suitable for cultivation, have all been added to those which have been known to mankind since the long-past ages when agriculture began to be evolved. Four centuries have now elapsed since the American continents were discovered: how many species have been added to those which were already under cultivation? Some forty species, among which, it is true, we must include the potato, arrow-root, cinchona, tobacco, tomato, pine-apple, indian-corn; but are there not many more which it would prove beneficial to cultivate? But this is the business of the future, and ours is with the past.[1] Many of our cultivated plants differ but

[1] While thus advocating the necessity of turning to a better account the numerous plants which exist and may be, by cultivation, made very profitable to man in one way or another, I perceive that Prof. G. L. Goodale was addressing the American Association for the Advancement of Science on the same topic at the same time. I will merely refer the reader to his very interesting paper published in the *American Journal of Science*, under the heading: *Useful Plants of the Future. Some of the Possibilities of Economic Botany* (pp. 271–303), where

slightly from their wild congeners, especially when we consider plants which have been cultivated only within recent times. De Candolle[1] shows that out of 247 cultivated species, 169 have enough resemblance to some wild species to allow us to trace with exactness their origin in the latter. In five cases there is room for doubt ; in four, the cultivated species although unlike the wild one is not different enough to prevent us from tracing its origin ; in fifteen the differences are greater, and the question remains open whether there are here distinct species or mere varieties ; in twenty-four cases wild forms are met which may be cultivated plants which have been dispersed and have become naturalised ; in three cases the wild and cultivated forms differ to the extent of being considered as distinct species ; in three cases the species are distinct ; in twenty-four the wild form is unknown, but some may yet be recognised after more careful investigation. Upon the whole, then, out of 247 cultivated species of plants 113 exist in the wild as well as in the cultivated state, identical or more or less modified ; twenty-seven are doubtful ; and twenty-seven have not been found growing wild. Such is the result obtained by De Candolle in his valuable

a large amount of useful information and valuable suggestions are embodied (April, 1892).

[1] *Origine des Plantes Cultivées*, 1883.

investigations. Among these 247 species there are seven which are rapidly becoming extinct.[1] We thus see

[1] The following is De Candolle's list :—

I. Species spontaneously growing in the wild state, with all the appearance of indigenous species, and identical with the cultivated species... 169

Of these 169 species 31 are of very remote origin ; 56 have been cultivated for more than 2000 years ; the others are of unknown date.

II. Species of the same category as I., but which have been found in a wild condition in one locality only, and only by one observer 3

Cucurbita maxima. *Faba vulgaris. Nicotiana tabacum.*[1]

III. Species seen and noticed, but not gathered, by non-botanical observers who may have been mistaken (old authors) 2

Carthamus tinctorius, *Triticum vulgare.*

IV. Species found in a wild state, by botanists, under forms which differ slightly from those which are cultivated, but not enough to prevent most botanists from recognising that both are of the same species 4

Olea europea, *Oryza sativa*, Solanum tuberosum, *Vitis vinifera.*

V. Species found in a wild state, but considered by some authors of different species, by others, of different variety, when compared to the cultivated forms.......................... 15

Allium ampeloprasum porrum, Cichorium endivia var.*, *Crocus sativus var.*, Cucumis melo*, Cucurbita pepo, Helianthus tuberosus, Lactuca scariola sativa, *Linum usitatissimum annuum*, Lycopersicum esculentum, Papaver somniferum, Pyrus nivalis var., Ribes Grossularia*, Solanum melongena, Spinacia oleracea var.*, Triticum monococcum.

[1] Italicised names are those of species which have been cultivated for a very long time ; names with an * are those of plants cultivated for less than 2000 years.

that the greater number of cultivated plants are known also in their wild condition. This result may seem surprising and we may wonder at it. As De Candolle

VI. Species found in a sub-spontaneous condition, similar to any one of the cultivated forms, but which are perhaps cultivated species having run wild 24

Agave americana, Amarantus gangeticus, *Amygdalus persica*, Areca catechu, Avena orientalis*, Avena sativa, Cajanus indicus*, *Cicer arietinum*, Citrus decumana, Cucurbita moschata, Dioscorea japonica, Ervum ervilia, *Ervum lens*, Fagopyrum emarginatum, Gossypium barbadense, Holcus saccharatus, *Holcus sorghum*, Indigofera tinctoria, Lepidium sativum, Maranta arundinacea, Nicotiana rustica, *Panicum miliaceum*, Raphanus sativus, Spergula arvensis.

VII. Species found in a sub-spontaneous condition, but different enough from the cultivated varieties to allow most botanists to consider them as distinct species 3

Allium ascalonicum* (form of *A. cepa*?), Allium scorodoprasum* (form of A. sativum?), Secale cereale (form of some other Secale?).

VIII. Species not found in a wild condition, nor in a sub-spontaneous state, having perhaps originated in cultivated forms, but too widely different not to be commonly called species.. 3

Hordeum hexastichum (derived from *H. distichum?*), *Hordeum vulgare* (derived from *H. distichon*?), *Triticum spelta* (derived from T. vulgare?).

IX. Species not found in a wild or sub-spontaneous condition, having originated in countries whose indigenous flora is not yet sufficiently known, but where wild species exist which are perhaps the same.................................... .. 6

Arachis hypogaea, Caryophyllus aromaticus, *Convolvulus batatus*, Dolichos lablab*, Manihot utilissima, Phaseolus vulgaris.

says, "we should have believed *a priori* that a much larger number of species which have been cultivated for more than 4,000 years, would have departed from the original type to such a degree that the latter could not be recognised. It appears, on the contrary, that the wild forms have generally persisted." And he goes on to explain this fact in two ways. First of all, the period of 4,000 years is comparatively short, when we consider the duration of most phanerogamous species; and on the other hand, intercrossing between the wild and the cultivated forms may have prevented the production of considerable differences between them in all cases where the wild form persisted. This last view is very important and goes far to explain why the cases where the wild progenitor is not recognisable are not more frequent; and if it is correct, we should find the largest departure from the original

X. Species not found in a wild or sub-spontaneous condition, having originated in countries whose indigenous flora is yet incompletely known, but more different from the wild species of these countries than in the preceding case 18

Amorphophallus konjak, Aracacha esculenta, Brassica chinensis, Capsicum annuum, Chenopodium quinoa, Citrus nobilis, Cucurbita ficifolia, Dioscorea alata, Dioscorea batatas, Dioscorea sativa, Eleusine coracana, Lucuma mammosa, Nephelium Litchi, Pisum sativum*, Saccharum officinarum, Sechium edule, Trichosanthes anguina*, *Zea maïs*.

Total 247

type in species cultivated in countries where the wild form does not exist.

The foregoing facts show that some modification is due to cultivation, in cultivated plants, since some species exist which are not recognised in the wild state, such as indian-corn, sugar-cane, wheat, etc. But numerous modifications are met with, when we consider our cultivated species themselves, and investigate the orgin of the varieties they exhibit. Here, cultivation shows itself as having played an important part. Let us consider, for instance, the cabbage, *Brassica oleracea*, which is most probably a European species. While Theophrastus recognised three varieties, Pliny was acquainted with six, Tournefort with twenty, and De Candolle enumerates more than thirty. These varieties are probably all due to cultivation, and in some cases the differences between them are very considerable, and the differences between the varieties and the parent form, which still exists in France and England, are greater still. Almost every part has varied in this species, from the root to the tip of the leaves and the peduncles of the flowers. Compare Brussels sprouts and Hungarian turnips, cauliflowers and common cabbage, for instance, or let us turn to the common kidney bean (*Phaseolus vulgaris*); here also varieties are numerous. The potato has also a large number of varieties although its cultivation is of recent origin; among the radishes considerable differences obtain, whatever their original form may have been; and the same is true of carrots, lettuce, strawberries, peaches, pears, apples, oranges—in fact, of every cultivated species we are acquainted with. In all these species, and also in all plants which are cultivated for the sake of their flowers, or because they provide drugs which are of use to man, in all vegetable species, in short, which mankind cultivates for some reason or other, numerous varieties exist, and in many cases we meet with twenty, thirty, or forty varieties, if not even more, in the same species. These varieties man is responsible for: he has made them, he has evolved them out of the species, and some are of very recent origin, such as the Brussels sprouts, for instance—some were made yesterday, and others will appear to-morrow. The process may be indefinitely varied, and so long as man cultivates plants he has the right to expect to create new varieties. The method used in such creations is nowise mysterious, and all breeders, horticulturists, and gardeners are acquainted with its application.

The mere enumeration of our garden vegetables, fruit trees, commercial and industrial plants, garden trees and flowers, and even their names show that variability exists amongst cultivated plants as well as among the wild species, and in plants generally as well as in animals. It is because plants and animals vary naturally or spontaneously—here spontaneously merely means from unknown causes—that man has been able to select among the variations and to make them become permanent. And when we see how very different are the lines along which the same species has varied—take the cabbage, for instance, or the dog—we are warranted in drawing the conclusion that variability is very considerable among cultivated or domesticated organisms.[1]

We must now consider the last of the three groups of facts which lie at the basis of experimental transformism. We have considered variability in the state of nature, and shown that it is to be met with in all parts of the organism; we have also shown that the same variability obtains among cultivated plants and domestic animals whose numerous varieties are the result of man's selection. We must now refer to the facts which show how natural variability may be determined or facilitated. These facts may be arranged under the general heading of the influence of environment in the production of variations.[2] Their

[1] See De Candolle's *Origine des Plantes Cultivées*, and Sturtevant's important series of articles on *Origin of Cultivated Plants*, in *American Naturalist*, 1887–89.

[2] Besides Semper's well-known *Animal Life* (published in the *International Science Series*), the reader will find a large body of subsequent literature condensed in J. Arthur Thomson's *Synthetic Summary of the*

interest lies in the fact that they can help to show us how—and how far—we can produce variation instead of having to wait for its spontaneous appearance. It must be said that up to the present date but little has been done in this line. Such investigations are only of speculative interest—at least they seem so to most people—and they require much time and patience as well as favourable conditions which are seldom offered by our city laboratories. On the other hand, we are allowed to use many facts of observation in this demonstration of the influence of environment upon parts or the whole of living organisms: they are of as much value as direct experiments when we can really ascertain what are the influences which have been in action. Between observation and experimentation there is not as much difference as is commonly said, and when the conditions under which any phenomenon is observed can be exactly recognised, the result of the observation has all the value of that of an experiment.[1]

The investigation of the direct influence of environ-

Influence of the Environment upon the Organism. Proc. Roy. Phys. Soc. Edin. 1887.

[1] An observation made under circumstances which allow all the elements which co-operate to be well ascertained has as much worth as an experiment; the only difference being that in the latter case the experimenter's will has determined the conditions while in the former he has had no control upon them. But if he is exactly acquainted with these, the result is quite as valuable, and the difference lies only in the mental process which precedes the recording of the result.

ment illustrates from the very first a fact which is more or less familiar to all, the fact that living organisms can withstand, generally speaking, but a small amount of environmental modification. They are in so many ways, and by so many parts, dependent upon the external medium, their adaptation to it is so very close, and the slightest change in environment is apt to react on such a large proportion of the vital functions, that we cannot wonder at the enormous influence which external modifications can exert on life. Suppose for instance the very small percentage 0,030–0,034°/₀ of carbonic acid which always exists in our atmosphere, were to disappear, life would soon be extinct on the whole earth, because plants cannot do without it, nor animals without plants. Thus a very small change, which would be perceived only through the use of precise methods and instruments, and could not be detected by our unaided senses, would suffice to ruin all life. This instance shows how very close is the adaptation between organisms and their environment, and teaches the need of care in all our experiments on the action of the latter on the former. A great number of instances are known which show the considerable influence of minute external variations, but none, I think, is more cogent than that which I gather from the excellent *Etudes chimiques sur la Végétation* of Jules Raulin (Paris, 1870). This writer, while investigating the influence of the different elements which go to make up that complex whole which we call environment, has studied the influence of many chemical substances upon the growth of *Aspergillus niger.* After having ascertained the exact nature and proportions of the chemical substances which are required to provide for the plant the best suitable medium, he has investigated the influence of some chemicals which do not contribute to the making of that medium. Some of them exert a most unfavourable influence; thus bichloride of platinum for instance prevents all vital manifestations of *Aspergillus;* even when added in the very minute dose of $\frac{1}{8000}$, it kills the plant. With bichloride of mercury these results are still more striking, as the dose of $\frac{1}{512000}$ is enough to kill *Aspergillus*; and with nitrate of silver the results are still more surprising: add only $\frac{1}{1600000}$ and death ensues.

It even happens that when *Aspergillus* is made to grow in a silver cup, the plant soon dies, because some of the silver dissolves in the liquid medium, and although there is not enough of the metal in the liquid to allow its detection through chemical analysis, the plant detects it immediately, and shows that it feels the influence of the poison. Similar facts are very abundant now, since bacteriology has sprung into existence, and we all know of the considerable influence exerted on micro-organisms by very dilute reagents. Very minute doses may kill, in more or less time, most bacteria, and this provides a basis for the prevention and treatment of many diseases due to pathogenetic organisms, and for the disinfection of places where pathogenetic organisms are supposed to exist. It is needless here to quote instances. But this investigation has led to some interesting results, in showing also that different organisms require different quantities or proportions of the same substance to cause death, and that even the same organisms, under different forms, require different proportions. Here again, in these very minute and elementary organisms, we find physiological variability or variation in play; here again, in these minute cells, where function would at first glance appear very elementary, great differences exist in reaction towards external agents, and hence in intimate physiology. We know, for instance, that while bacteria are killed, in one species, at 80° or 90° centigrade, the spores of the same species require 100° or 120°; that one species is killed at 40° or 50°, another at 70°, 80°, or 100°; that the one thrives well in such and such a culture, while the other requires very different media. Here also, once more, physiological variability is in action; the bacillus of tuberculosis, for instance, thrives in *bouillon* of herring with glycerine, or of clams, or of monkey, hen, or goose, while in rat broth it becomes very feeble. Each micro-organism has its very marked preferences as regards temperature and chemical conditions, and the susceptibility to variation in these conditions is so very marked, that while the fowl is too warm-blooded to allow anthrax to develop, it is enough to cool the animal artificially to render it inoculable.

These facts show that the correspondence between the environment and the organism is very close, and that very slight alterations are enough to cause death, and the result is that in the experimental investigation of the influence of environment upon variability, we must be careful to handle our methods with great prudence.

But, on the other hand, while in all cases very slight modifications in environment are apt to destroy life—especially as concerns chemical environment—we often justly wonder at the extent of the modifications which may be introduced into the latter without impairing the vital functions. Micro-organisms afford numerous instances of this fact, but I prefer giving some examples from the higher organisms. It is a familiar fact that while most aquatic animals or plants die very quickly when transferred from sea to fresh water or from fresh to sea water, a number of them withstand the change perfectly well, and many fishes are known to spend part of their life in each of these media. Direct experiments on this matter were made in the beginning of the century by Beudant; and the results were fully recorded by him in a paper read to the Academy of Sciences.[1] While sudden passage from one medium to another was in most cases conducive to death, he has shown that a gradual passage may be attended with success. A number of *Lymnaea, Planorbis, Physa, Ancylus, Paludina*, etc. were divided into two sets; the one lived in fresh water, the other were put into fresh water to which, every day, a small quantity of salt was added. After a few months, when in the latter case the saltness was 4 °/₀ the number of the foregoing individuals which were yet living in the salted water was nearly exactly the same as that of the individuals yet alive in the fresh-water aquarium: in both aquaria the same number of animals had been originally introduced, and out of 400 there remained 170 in salt water, 184 in fresh water.

Other species suffered more; while only 40 °/₀ *Paludina* died in fresh water, 71·54 °/₀ died in salt water, and while *Unio* and *Anodonta* all throve well in fresh water, they all died in salt. It is an

[1] F. S. Beudant, *Mémoire sur la Possibilité de faire vivre des Mollusques fluviatiles dans les Eaux salées, et des Mollusques marins dans les Eaux douces, considérée sous le rapport de la Géologie.* Paris, 1816, *Journal de Physique*, vol. lxxxiii. p. 268.

interesting fact that some of the animals accustomed to salt water did very well when suddenly transferred into fresh water, and also when, a month later, they were again suddenly replaced in the salt solution, although, in the latter case, animals of the same species die immediately when they are placed in salt water without having been gradually accustomed to it by small doses of salt.

In another series of experiments Beudant tried to accustom marine animals to fresh water. These animals, of the genera *Haliotis*, *Cerithium*, *Buccinum*, *Tellina*, *Venus*, *Ostrea*, *Pecten*, *Mytilus*, when suddenly immersed in fresh water soon died, although some of the littoral species certainly seemed to resist longer. After this experiment Beudant tried to accustom the same species gradually. He had thirty-eight species at his disposal, in great numbers, and performed the experiment as in the converse case, adding fresh water every day, so that after five months the animals were living in pure fresh water. Out of thirty-eight species, twenty withstood the change perfectly well; 370 out of 610 being alive in fresh water, while in salt water there were 401; while the eighteen others were unable to exist.

Lastly, in a third series of investigations, Beudant established the fact that marine molluscs are able to live in sea water containing 30 °/ₒ of common salt, a much larger proportion than that which is commonly found in the sea.

These experiments show that animals may be accustomed to live in media which are very different from those which they normally inhabit, provided the change is a gradual one. I performed similar investigations some years ago with different animals,[1] with the view of ascertaining whether it may not be generally said that the animals which live close to the sea shore where the fresh water of river and rain must certainly somewhat sweeten the sea water, are more liable than others, inhabiting the sea at greater distances from the coast, to get accustomed to life in fresh water. I first compared the resistance to fresh water of three species of Actinozoa, which were *Actinia mesembryanthemum*, *Anthea Cereus*, and *Sagartia parasitica*, putting them at first into an aquarium containing six litres of salt water and one and a half of fresh water. All went well. After a few days I increased the proportion of fresh water, and no change was apparent, so that on the seventh day I mixed four and a half litres of sea water with three of fresh water. On the ninth day death came, carrying off one *Sagartia*. A second died on the eleventh day, and on the thirteenth and fifteenth days the two last of this species

[1] Henry de Varigny, *Beitrag zum Studium des Einflusses süssen Wassers auf die Seethiere*. *Centralblatt f. Physiologie*, 21 January, 1888.

died. The other species were doing quite well; both are shore-inhabiting animals, while *Sagartia* lives at some depth in the sea.

In another experiment, I used animals of very different groups—*Carcinus maenas*, *Pagurus Prideauxii*, *Dromia vulgaris*, *Anthea Cereus*, *Sagartia parasitica*, *Portunus puber*, *Doris tuberculata*, *Venus* (sp. ?), *Actinia mesembryanthemum*, *Holothuria cucumaria*, *Grapsus* (sp. ?)—beginning with fresh water 20, and sea water 70.

I gradually altered the proportions, so that on the thirty-fifth day I had 80 fresh water and 10 salt water. I cannot go into details of this experiment, or record the obituary of the different animals as they one by one died, dreaming doubtless of shores where experimenting bipeds are not admitted; but the final result was, that on the thirty-eighth day I had nine animals living, of which eight were *Actinia mesembryanthemum*, and one *Carcinus maenas*. While all my *Anthea* died, as also my *Sagartia* (the latter opening the march), I did not lose one single *Actinia mesembryanthemum*. As all are aware, this species, as well as the *Carcinus maenas*, lives as close as possible to the shore and to the surface, so that, in fact, both must certainly in their normal habitat have become accustomed to a decrease in the saltness of the water. I have witnessed facts which are exactly parallel[1] while comparing the influence of increased temperature on Crustaceans of different species, some dwellers on the shore, and others at some distance; the former withstand temperatures which the latter are unable to resist, and the reason may be found in the fact that the shore-inhabiting animals are liable (my experiments were made in Banyuls on the Mediterranean coast) to be much warmed in the pools or even in the surface water, by the heat of the sun.

In the case of heat, as well as that of salt, there seems to exist a marked adaptation of the organisms, and this adaptation I have also investigated in regard quite different animals and conditions.[2] For instance while tadpoles soon die when introduced into water containing some amount of common salt, it is easy to enable them to survive by using at first very dilute solutions to which a small quantity of salt is added every day or every second day. But even when the pro-

[1] Henry de Varigny, *Ueber die Wirkung der Temperaturerhöhungen auf einige Crustaceen. Centralblatt f. Physiologie*, 1887.

[2] *Cf.* Henry de Varigny: *Influence exercée par les Principes contenus dans l'Eau de Mer sur le Développement d'Animaux d'Eau douce. Comptes Rendus*, 1883, vol. xcvii. p. 94. But many of the results here recorded have been obtained at a later date, and the paper in which they have been embodied, read before the meeting of the French Association for the Advancement of Science, at the meeting held in Rouen, 1883, under the title: *Sur l'Action des Variations de Milieu sur les Animaux d'Eau douce*, has remained unpublished.

cess is carefully and slowly accomplished, the age of the tadpoles experimented upon must be taken into consideration, for tadpoles of three weeks' age withstand conditions which younger larvae cannot resist. For instance, young tadpoles, aged two or three days at the beginning of the experiment, all died when the water contained eleven grammes of salt per litre, while those which were three weeks old died only when the percentage was fifteen grammes per litre. Rather strangely, tadpoles of four or five weeks which had been accustomed to live in water containing fourteen grammes of salt per litre, died rapidly when I transferred them to their normal element, fresh water : they had become so well adapted to the new environment that their normal medium had become deadly for them. Young eels may also be accustomed to live in salt water, although they are very sensitive to the influence of salt, even if there is less than two grammes of salt per litre of water: at the beginning they feel the change, and during some ten or twelve hours remain sluggish, seemingly half paralysed ; but they become quite well after twelve or fifteen hours, and recover their usual activity, and may be by gradual additions of salt accustomed to live in water containing at least five grammes of salt per litre. P. Bert, Plateau, Hugo Eisig, and many others have performed similar experiments with similar results. They all show that even inconsiderable differences in the environment, when sudden, are accompanied with great danger to the life of animals, while slow change may do no harm at all ; it even happens that the adaptation of the animal to the new environment becomes so close that sudden transfer to the formerly normal one is dangerous and even deadly. Instances of sudden changes of any sort where the result is death are to be met with in nature ; so that in all experiments bearing on the influence of environment upon organisms, care must be taken to alter it very gradually. Of course there are great differences in this influence of environment on life ; any amount of carbonate or of sulphate of lime, for instance may be added to water in which tadpoles are living, without any injury ; other reagents are not equally dangerous, for a number of reasons which it is needless to enumerate here.

Generally speaking, then, care must be taken to alter environment slowly, if we wish to appreciate its influence on organic forms and life. In some cases it happens that, while not totally impairing life, a change of environment is conducive to considerable change in the vital functions. Such [1] Regnard has seen to be the case in the animals subjected to pressures much greater than that under which they commonly live.

[1] *Cf.* P. Regnard, *Recherches expérimentales sur les Conditions Physiques de la Vie sous les Eaux.* Paris, 1891.

By means of instruments which he devised, he has been able to compel animals to live under enormous pressures, 500, 600, 800, and even 1000 atmospheres, which corresponds to a depth of over 30,000 feet in the sea, and he has seen that most animals, although they withstand 200 or 300 atmospheres without giving any signs of alteration in their vital functions, pass into a state of *latent life* when the pressure becomes higher. For instance, Vorticellæ, under a pressure of 600 atmospheres, become motionless; the cilia stop entirely, and the animal does not move at all: the same phenomenon is observed even at lower pressures (300), while other animals require higher ones. The same phenomenon appears with different species at different pressures. While some are killed if they remain some time in this condition, others revive when the pressure is diminished and becomes normal; they revive in more or less time. *Actinia plumosa* may live a whole hour under 1000 atmospheres pressure: the animal seems to be dead, and when taken out of the apparatus strikes the spectator as being larger than before; when weighed it is seen to be twice as heavy as before the experiment, water having doubtless been forced into its tissues. But after some hours the animal is itself again, has regained its normal weight and dimensions, and is quite well. The same obtains with star-fishes, ascidians, molluscs, worms, crustaceans, some aquatic insects, while frogs and fishes die when subjected to high pressures which the former easily withstand. I cannot enter here upon the details of the experiments or their results, nor upon the reason of these differences between different species, but the fact of *latent life* must be recorded. In experiments on the influence of environment such circumstances contrary to the continuation of life must be avoided; they may be induced by other factors, such as *change of chemical medium*, for instance (in the case of micro-organisms), and it suffices to notice the fact.

In some cases external influences may make themselves felt by creating animals or plants which are abnormal and monstrous. Although in most cases life is impossible, through the importance of the disorders thus introduced, these facts are interesting, as they go to show that very slight influences may make themselves felt in a marked manner, when they are allowed to operate at certain periods of life, especially during that of early development, which is the only period during which real monsters may be produced.

Among the physiologists who have investigated the question of experimental teratogeny, of the artificial production of monsters, none deserve mention more than Camille Dareste, the author of that celebrated work *Recherches sur la Production Artificielle des Monstruosités ; ou, Essais de Tératogénie Expérimentale.*[1] While Swammerdam [2] seems to have been the first to try to produce monstrosities in animals through experimental means, M. C. Dareste has been the first to investigate the subject in a precise manner, and he may indeed be called the founder of experimental teratogeny. All his methods, which are of a very simple sort, are described in his work ; they all have reference, as well as most of his investigations, to experiments on the eggs of birds, and they consist: (*a*) in placing the eggs in a vertical instead of a horizontal position ; (*b*) in covering part of the shell with some substance which resists the penetration of air ; (*c*) in keeping the eggs at temperatures which are slightly below or above that of normal incubation ; (*d*) in heating at different temperatures any two parts of the same egg. These methods have enabled M. Dareste to produce a large number of teratological specimens, many varieties of which were previously unknown, and the results obtained are very important in the discussion of many of the higher problems of natural science. But much remains to be done on the lines of investigation thus opened, and the main problem to be solved is that of

[1] Paris, 1877 ; 2nd edition in 1891. Reinwald.
[2] Unfortunately, no record has been found of his experiments.

producing monstrosities which are not of such a sort as to prevent the continuation of life after the end of the developmental period. We may expect that many methods will be discovered through which the embryo of most groups of animals and plants can be modified in some degree, without impairing vital functions, and this discovery will certainly prove most beneficial to experimental transformism. Other investigators have, in most parts of the scientific world, followed M. C. Dareste's experiments, and the results are valuable. For instance, M. J. Fallou [1] has shown that when cocoons of *Attacus Pernyi* are hung vertically, the butterflies are normal, while if resting horizontally the same cocoons yield abnormal butterflies. He adds that a very slight disturbance in the internal conditions of the mode of suspension is enough to induce important variations. This point should be investigated anew. Again, G. Pouchet and Chabry have recently investigated the influence of some chemical conditions on the development of young sea-urchins, by depriving the water in which the eggs are hatched of one of its normal components, carbonate of lime,[2] and they have seen that under such conditions

[1] *Etude sur la Production Artificielle des Lépidoptères anormaux. Bull. Soc. Acclimatation*, 1887, p. 499.

[2] *Sur le Développement des Larves d'Oursin dans l'Eau de mer privée de Chaux. Comptes Rendus de la Société de Biologie*, 1889, p. 17.

considerable differences are noticeable in the evolution of individuals. The less carbonate there remains, the slower is the development; and in no case does the larva go any further than the Pluteus stage. But the most interesting fact is that these embryos do not acquire the spicules which are so characteristic of their structure. The problem now consists in obtaining such abnormal embryos with sufficient vitality to keep alive, for the *Pluteus* obtained by Pouchet and Chabry have always proved unable to outlive the stage known by the same name. Recently also M. Marcacci has shown that when eggs—of the common fowl—are subjected to motion during the period of incubation, abnormal chicks are obtained and this is a new method of producing monsters.

The foregoing general facts are enough to entitle us to draw the conclusion that environmental changes, while in many cases liable to cause rapid death, or considerable disturbance of vital functions, may in others, and especially when care is taken not to produce too important modifications, result in organisms which diverge widely from the common form. As yet, however, the divergences have been too great, involving changes which make life impossible. But new investigations may show that this influence of environment can be reduced in degree, so that the organisms thus modified may be able to live to the adult condition. At all events, it is certainly more interesting for us to see that slight external causes may determine considerable effects than it would be to meet with sheer impossibility of any action or influence. The facts that we are now acquainted with go to show that the influence of environment upon the development of the egg is considerable—from the very first stages of the building of the embryo, from the segmentation process itself. This has been ascertained by H. W. Conn in reference to the eggs of *Thalassema mellita*, where segmentation is regular or irregular according to the mode of life—free or protected; by Pflüger, in reference to amphibian eggs, by O. and R. Hertwig, and by Marcacci; while Tichomiroff has witnessed parthenogenesis artificially induced in insects by chemical stimuli.

Many facts also go to prove that external influences are very efficient in accelerating or retarding the developmental processes, as every naturalist has certainly noticed when comparing the development of eggs from the same *Lymnaea* or *Planorbis* for instance, under different conditions of heat or light; and other facts show that environment also exerts a marked influence on the general metabolism of the body.

This influence is illustrated by experiments on the growth of animals or plants, and it may be well to dwell upon this. As a general fact, growth and dimensions are correlated with the quantity and quality of food. All breeders know what is the most suitable food for their animals, according to the result they wish to obtain; all agriculturists know exactly what are the manures they must use, according to the soil on which they grow their plants, and the species, the varieties even, that they put under cultivation. These facts have been put beyond doubt by the numerous experiments which have been performed in the English, French, and German agricultural stations, and all have seen illustrations showing the influence of the principal manures, if not the plants themselves. Although these sorts of experiments originated over a century ago—although Tillet began in 1770 to perform investigations with artificial soils made with different proportions of natural earth, with the view of ascertaining the influence of the components on the crop obtained, the study of manures, conducted in a scientific manner, has been only during the present century made to yield the precise and valuable results we are all acquainted with.[1] But

[1] Tillet's experiments were published under the following title:—*Expériences et Observations sur la Végétation du Blé dans chacune des Matières simples dont les terres labourables sont ordinairement composées, et dans différents mélanges de ces matières, par lesquels on s'est rapproché de ceux qui constituent ces mêmes terres à labour. Histoire et*

many other external conditions exert a considerable influence on growth and weight, such as light, heat etc. In 1709 De Vallemont[1] noticed—others may have done the same before him—that in trees the wood is thicker on the southern than on the northern aspect, and he ascribed this fact to the greater heat received by the southern side of plants; and Kraus has shown that fruits grow much more during the night than during daytime, the difference being between eighty or ninety and twenty or ten per cent.; eighty per cent. of the growth of an apple, for instance, occurring at night-time, while twenty per cent. occurs during the day.[2] On these points further information is to be found in any text-book of plant physiology, such as those of Sachs or of Vines.

Concerning animal growth and its dependence upon external conditions, Yung,[3] of Geneva, has performed many useful experiments, showing how considerable variations are induced in the length and dimensions of tadpoles through the use of different foods, animal foods, such as egg and flesh especially, being much

Mémoires de l'Académie Royale des Sciences, 1772, p. 99 of Part I. of the *Mémoires*.

[1] *Curiosités de la Nature et de l'Art*, 1709, p. 57.

[2] *Cf.* Naudin, *Rythme de la Croissance. Revue Horticole*, 1872, p. 408.

[3] *Cf.* his recent *Propos Scientifiques*, in which are abstracted most of his investigations.

more suitable than vegetable food. He has shown also that growth is certainly correlated with other influences, since, even when food is superabundant, tadpoles become sooner transformed into the adult form when they are few than when they are numerous in the same space. This had already been noticed by Semper in his experiments on *Lymnaea stagnalis*,[1] to which reference has already been made, and I have also spoken of my own experiments on the same subject.[2] To these facts I shall merely add the general fact, recorded by Herbert Spencer, that trout and other fishes living in small streams are generally small; that pond-snails grow larger in ponds than in rivers, and that under natural conditions, as Balaschewa has noticed, the smaller the mass of water in which molluscs live, the smaller they remain. And R. P. Whitfield,[3] who has performed experiments similar to those of Semper, has obtained the same results, with this additional interesting circumstance, that when the dwarfing process is made to operate

[1] C. Semper, *Ueber die Wachsthums-Bedingungen der Lymnaeus stagnalis. Arbeiten aus dem Zool. Zoot. Institut zu Würzburg*, vol. i., 1874.

[2] H. de Varigny, *Contribution Expérimentale à l'Etude de la Croissance. Comptes Rendus*, June 15, 1891.

[3] *Description of the Animal of Lymnaea megasoma* (Say), *with some Account of the Changes produced by Confinement in Aquaria and under Unnatural Conditions. Am. Naturalist*, 1880, p. 51 (abstract).

on successive generations, important sexual changes occur which are correlated with the changes in growth and size; instead of being as usual hermaphrodite the individuals (of *Lymnaea megasoma*) become unisexual and are exclusively female. At the same time also the hepatic gland undergoes a process of partial atrophy. These influences of external agents on internal viscera, such as the sexual glands and liver, whether directly or indirectly exerted, are very interesting, and more cases of a similar nature may be discovered if more attention is paid to the process. It is merely a matter of historical interest to recall that the father of natural science, Aristotle, many centuries ago noticed the variations in growth and dimensions while comparing animals of the same species in Egypt and in Greece.

The preceding facts show that external influences may react on the general growth of organisms, and perhaps on their internal constitution.

In some cases, however, where variation in environment might be expected on *a priori* grounds to interfere greatly with internal constitution, we perceive no such interference, and we are compelled to draw the conclusion that the organism admits of a greater amount of physiological elasticity than we expected. I find a good instance of such elasticity in the result of recent experiments which have been conducted by

Messrs. Irvine and Woodhead.[1] These experiments show that when laying hens are deprived of carbonate of lime by being shut into a room lined throughout with wood, without sand or soil, they are yet able to lay normal eggs, provided with the usual shell, if sulphate of lime is given to them in their food. It follows that when the hen's organism does not receive carbonate of lime as is usually the case, it is able to transform sulphate into carbonate. We do not know exactly how things go on in this case, nor even in the normal case.[2] Are carbonate and sulphate of lime transformed into phosphate for instance, which is carried under this form to all parts of the organism, and is in the oviduct transformed by nascent carbonic acid—the result of the respiration of tissues—into carbonate of lime? In such case the thing would be less surprising than in any other hypothesis; at all events, whatever the case may be, we have here a very precise instance of external variation being met by the organism, whether its physiology varies or not. But other animals offer the reverse instance, as Irvine and

[1] R. Irvine and G. Sims Woodhead: *On the Secretion of Lime by Animals*, *Proc. Roy. Soc. Edin.* May 7, 1889, and *Secretion of Carbonate of Lime by Animals* (Part II.), *ibid.*

[2] I would refer the reader, for a recent investigation of this matter, to Moynier de Villepoise: *Note sur le Mode de Production des Formations calcaires du Teste des Mollusques. Comptes Rendus Société de Biologie*, 1892

Woodhead have shown. They have seen that crabs living in sea-water from which chloride of calcium is excluded, cannot, after shedding their exoskeleton, form a new one even when sulphate of lime and chloride of sodium are present in the water : chloride of calcium is quite necessary for the formation of the crab shell.

Considering now external form, we see that it may be influenced by external influences, as well as internal constitution or physiology. Here are some instances out of many. M. Languet de Sivry, some fifty years ago,[1] noticed that seeds of short-rooted carrot, when sown in a particular soil, in the alluvial deposits formed by a small river in France, yielded immediately, during the first generation, a number of long-rooted plants, either white or yellow, whose roots were very much larger than those in the parent plants. The seeds of the best, or less deformed plants, were selected, and sown in the same soil. The result was that in the second generation hardly any roots were found of the short type, and most were exactly similar to the common wild form. Again, Petermann (*Contribution à la Chimie et à la Physiologie de la Betterave à sucre*, Brussels, 1889) notices the fact, familiar to all horticulturists, that when a plant—beet for instance—is grown in poor soil, its root becomes much longer

[1] Cf. *Société Royale et Centrale d'Agriculture*, 2nd series, vol. ii., 1846–7, p. 539.

than is usually the case because it lengthens in pursuit of richer soil ; and when thick-rooted plants, such as beet, are experimented upon, a very striking difference is produced in the length and also the thickness of the root by the chemical constitution of the soil the plants inhabit.

Many facts of this sort might be quoted. Concerning animals, we may note that Bourguignat thinks *left-handed* shells of molluscs are due to some electrical influences in operation during the period when the embryo is rotating in the egg (experiment might settle this) ; and Brot[1] has noticed a fact which, although very singular, can be merely mentioned here. One year he remarked a small pond near Geneva which contained many pond-snails, and found that nine-tenths of them were in various respects abnormal. Next year he visited the place again, but found only normal forms. The fact remains unexplained—but not unparalleled, as other observers have met with similar instances—as our writer only notices that in the first visit he found a large amount of common *Hydra*, while none at all were to be found at the second. It would seem rather audacious to ascribe the deformations of the molluscs to the presence of *Hydra*, even in large quantities ; but, on the other

[1] *Cf.* Locard, *Variations Malacologiques*, vol. ii., where these facts are noted.

hand, I think no naturalist, knowing anything about the mutual interaction of living organisms, would dismiss the case entirely, and say that any influence of the one on the other is impossible and incredible.

Alterations in the forms of animals, especially molluscs, are very frequent, and in some cases a suggestion as to the causes may be gained, when the deformed animals live under circumstances where a departure from the normal conditions is obvious. It is well known that many warm springs contain a large number of living plants and animals. *Physa acuta*, for instance, has been found in waters at 30° Cent. (Fischer), and even at 33° and 35° Cent. (at Dax, according to Dubalen, in *Soc. Linnéenne de Bordeaux*, vol. xxix. p. 4), while *Turbo thermalis* lives at 50° Cent. at Albano,[1] and *Neritina thermophila* between 50° and 60° Cent. in New Brittany Island, according to Studer, etc. But it would seem that when individuals which have not been gradually accustomed to such conditions, through their ancestors, live for the first time in such unnatural media, many deformations are apt to appear. M. G. Regelsperger[2] has observed the following case. The waters of an artesian well, sunk

[1] De Blainville's article *Mollusques*, in the *Dictionnaire des Sciences Naturelles*, 1816–30.

[2] *Déformations remarquables de Physa acuta observées à Rochefort sur Mer. Actes de la Société Linnéenne de Bordeaux*, vol. xxxix. (vol. ix. of 4th series), 1885, p. 117.

many years before, were made to run into a garden. The water was ferruginous, and its temperature was 32° or 33° Cent. in 1881, when the writer first began to notice the facts. At that time individuals of *Physa acuta* were seen in the water, and it was remarked that they were generally small, and in many cases much deformed, as one may perceive by a glance at the illustrations which accompany M. Regelsperger's paper. In 1882 the temperature of the water had fallen off considerably; instead of being at 32° or 33° it had decreased to 26°·5. M. Regelsperger again examined the animals, and saw that deformed individuals were very scarce. Since then the flow of the water has entirely ceased, the pipes having become impermeable, and the part where the animals are seen receives simply rain-water. Now the animals seem larger than they were when they lived in warm water, and none are deformed. In this case it seems quite certain that the deformations were the result of the heated water the animals lived in, and experiments can easily be made to prove the fact, or to disprove it, as the case may be. Ritzema Bos[1] has observed other deformations, or form-alterations, in very different animals and circumstances. Tylenchi (*Tylenchus devastatrix*), which have been accustomed for some generations to

[1] *Untersuchungen über Tylenchus devastatrix. Biol. Centralblatt*, vii., 1887, pp. 232-243.

feed on one single sort of plant, acquire form and size which differ from those of the same species fed on other plants, and it even seems that here physiological variation also comes into play, since prolonged life on one plant makes them less dangerous for other sorts of plants.

Deformations may be determined by other natural causes. M. Piré[1] has seen *Planorbis complanatus* much deformed in Belgium through the influence of a thick layer of aquatic plants on the surface of the pond, preventing easy access to the air, and S. Clessin[2] explains the numerous deformations of the *Lymnaea tumida* in the Bodensee and Starnbergsee as due to the constant motion of the surface of these two lakes.

Form may vary in plants also, according to motion for instance, many plants being much smaller and much deformed in windy localities. Behrens has noticed the influence of currents on the forms of aquatic plants, which E. Mer and others have also done; and this leads us to consider the variations in internal structure, and in functions which are correlated with those modifications of external characters. E. Mer is among those who have investigated the matter in the most precise manner, and the results

[1] *Annales Soc. Malacologique de Belgique*, vol. vi., 1871, and xiv., 1879.

[2] *Deutsche-Excursions Mollusken Fauna*, Nuremberg, 1876, p. 368.

obtained are interesting. His investigations were made upon amphibious plants.[1] In *Ranunculus aquatilis* the external differences are well known between the plants which grow in water, and those which live on land, in the length and thickness of the stem, the length of internodes, etc., and if direct experiment and observation had not shown that the two forms belong to the same species, they would certainly be considered as different. Important structural differences are also present. If we consider the leaves only, we see that in the aquatic leaf the dichotomies are from eight to ten in number; the cells are cylindrical with two or three hairs; the epidermic cells are regularly shaped and contain chlorophyll, and bear but few stomata; while in the air-inhabiting leaf, dichotomies are from two to six in number; cells are flattened, without hairs; epidermic cells are irregular and contain no chlorophyll, while they bear a large number of stomata. The same differences obtain in the leaves of *Carex ampullacea* as shown here:

Submerged leaves.	Leaves not submerged.
Epidermic cells long, wide, without stomata.	Very thick cuticle. Numerous stomata.
2–3 rows of cells with chlorophyll.	4–5 rows of cells with abundant chlorophyll.

[1] E. Mer, *Des Modifications de Forme et de Structure que subissent les Plantes suivant qu'elles végètent à l'Air ou sous l'Eau. Bull. Soc. Botanique*, 1880, p. 50.

In a more recent paper Costantin has investigated the same subject, and given special attention to the influence of environment on the production of stomata.[1] In *Hippuris vulgaris* the leaves which live in water are long, thin, and sinuous, while in the air they are short and thick ; in the water the epidermis consists of short regular cells, bearing a number of stomata, while in the water the cells are long, thin, narrow, and bear no stomata. The writer has from the same rhizome of *Polygonum amphibium* grown two plants, one in water, the other in air, and while the latter was provided with numerous stomata, the former had none at all. This illustrates well the influence of environment on the production of stomata, and in *Stratiotes aloides* stomata are seen to appear on the leaves as they gradually emerge above the surface of the water. But the most minute and valuable investigations in reference to this matter have been conducted by M. Pierre Lesage, and were described last year[2] in his inaugural thesis. His researches bear upon the question of the influence of

[1] Costantin, *Influence du Milieu Aquatique sur les Stomates. Bull. Soc. Botanique*, 1885, p. 259.

[2] Pierre Lesage, *Influence du Bord de la Mer sur la Structure des Feuilles.* Rennes, Oberthur, 1890. See also his *Contributions à la Biologie des Plantes du Littoral et des Halophytes ; Influence de la Salure sur l'Anatomie des Végétaux*, *ibid.* 1891, which contains an abstract of many experiments performed after the publication of this thesis.

shore and inland life on different individuals of the same species, the number of species investigated being eighty-five. The results have been that in fifty-four species leaves are thicker in the vicinity of the sea than in inland individuals; in twenty-seven species no difference is apparent; and in four, leaves are thicker inland than near the sea. If we consider that of the fifty-four species above mentioned, seventeen are shore-inhabiting plants, while thirty-seven live preferably inland, it is obvious that, generally speaking, inland plants acquire thicker leaves when living in the vicinity of salt water. So much for the external characters. Now if we consider internal characters, many differences are detected which explain the presence of the external differences, and these differences are observable in all parts of the leaves. Epidermic cells are larger in twenty-three shore plants, but in thirty-one there is no difference, while in four the difference turns to the advantage of inland plants. Here, then, the influence makes itself but little felt. The case is quite altered when the mesophyll is considered, for while in eleven species there is no difference, in all others the palisade cells are either more numerous or attain greater thickness, or exhibit both characters at once, and at the same time the interspaces which underlie the stratum of palisade-cells are much reduced. The principal cause of dimensional differences is then to be found here. And lastly chlorophyll is less abundant in shore-inhabiting individuals. These facts of observation have been confirmed by experimental facts. M. Lesage has cultivated plants of the same species under precisely similar conditions save one, and has found that the results concur with his observations. The one condition which differs between the two sets of cultivations is the presence or absence of common salt in the water used for watering the plants. Of two individuals of the same species the one which has grown in soil watered with salted water has thicker leaves, and in these leaves the palisade cells are seen to be larger and more numerous. The experiments show that the influence exerted by sea-shore life on plants is principally due to the salt which is always contained in lesser or greater quantities in the soil, and is brought there by the winds carrying small drops of sea-water from the crests of the waves; they show at the same time that external variations are accompanied by important structural differences.

Sea-shore life or the presence of some common salt in the soil where plants grow exerts even more important variations; variations not in morphology and anatomical features, but in the physiological processes. In experiments concerning radishes M. P. Lesage has seen that while in radishes watered

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with common pure water starch is never or but rarely present in the root at the period when it is generally eaten, plants of the same age watered with solutions of common salt (from 1 to 20 per thousand) do contain a great deal of starch under certain conditions. For instance, if the water contains 20 per *mille* of salt, or 1 or 2 per mille, there is no starch ; at 3 or 5 °/₀₀ there is little of it ; at 10 °/₀₀ a little more, but at 4 °/₀₀ there is a large amount of starch. Thus, the presence of common salt, in definite proportions, exerts a considerable influence on the chemical structure and physiology of the plant.[1] With *Lepidium sativum* things are somewhat different : while the plant contains normally a large amount of starch, watering with a solution containing 10 °/₀₀ or more of common salt causes the starch to diminish in a large measure and even to disappear totally, while weak solutions do not interfere with the proportions of this substance.[2]

[1] *Cf.* Lesage : *Sur la Quantité d'Amidon contenue dans les Tubercules du Radis. Comptes Rendus*, September 7, 1891.

[2] *Cf. Influence de la Salure sur la quantité de l'Amidon contenue dans les Organes végétatifs du Lepidium sativum* (*Comptes Rendus*, April 20, 1891), and two other notes in the *Comptes Rendus*, January 18, 1892, and March 31, 1891. I would also refer the reader, on this general subject of the influence of salt on structure and physiology, to A. Batalin : *Wirkung des Chlornatriums auf die Entwickelung von Salicornia herbacea* (International Meeting of Botanists and Horticulturists in St. Petersburg, 1884) ; and to C. Brick : *Beiträge zur Biologie und vergleichende Anatomie der baltischen Strandpflanzen*

But external differences seem in some cases to have much greater moment than has hitherto been recognised, and in this respect no facts are of higher interest than those which were some years ago made known by the investigations of a Russian naturalist, M. Schmankewitsch,[1] to whose work I must call attention, although most have certainly heard more or less about it. *Daphnia* (or *Moina*) *rectirostris* is a small Crustacean which lives indifferently in fresh water, in brine ditches, and in salt lakes when the concentration varies from five to eight degrees of Baumé's areometer. But this difference in life-conditions is accompanied by noticeable variations in the physiology and structure of the animal. The mean temperature of the salt lake being lower than that of the fresh waters, *Daphnia* while being a summer form in the latter becomes an autumn form in the former, and thus has acquired the custom of living and even multiplying in salt water at temperatures at which the fresh-water form cannot live. So much for the physiological variation. M. Schmankewitsch goes on to say, as

(*Schriften der Naturforschenden Gesellschaft zu Dantzig*, 1888). Both authors obtain results which are entirely confirmed by M. Lesage's investigations.

[1] *Cf. Zeitschrift f. Wiss. Zoologie*, 1877, vol. xxix., p. 429, and also the *Transactions of the Neo-Russian Society of Naturalists* for 1875. The original papers have been abstracted in Packard's *Monograph of North American Phyllopod Crustacea*, 1883, Washington (Geological Survey).

concerns anatomical characters: "In the females of the Chadschibai Lake the penicilli or fascicles of knobbed setae (*Tast-Borsten*) are but little developed, being nearly fifty times shorter than the antennae themselves; while in the females of fresh water the same sensitive penicilli are moderately long and only six times shorter than the entire antennae. In the males the sensitive rods are also shorter than in those males inhabiting fresh water. The small hooks situated near the sensitive rods on the tips of the male antennae of fresh-water forms are strongly curved with pointed tips, while in the males of Chadschibai Lake those hooks are shorter, less curved, and with blunt tips. Of the two pointed pale sensory threads situated on geniculated protuberances of the first posterior third section of the male antennae, the posterior one is a little shorter than the anterior thread, the latter coming out a little more in front. These threads are, in the males of *Daphnia rectirostris* of the Chadschibai Lake, not in a straight but in a screw-like line. The distance between the threads is considerable, which character in the fresh-water males is much less prominent." I should scarcely have ventured to quote the foregoing lines, and to enter into such very minute details, if it was not a fact familiar to all that many zoologists are to be found in all countries who spend their life in establishing new species which are often based on characters such as those which Schmankewitsch refers to. These are specific characters, or at least we are told so every day by any number of systematic zoologists, and they are supposed to know "all about species," so we may go on with the quotation. "Besides the differences observed in the antennae of the salt-water generations of *Moina rectirostris*, our attention is called to the number of slender finely-toothed spines which occur on the lateral surface of the post-abdomen of *Daphnia rectirostris*, running in lateral series and nearly parallel with the direction of the rectum. Leydig called them finely-feathered spines, which I would have called triangular laterally dentate plates. However this may be, we observe in our fresh-water forms of *D. rectirostris* on each side from eleven to thirteen of these spines or plates, and only from seven to nine in the salt-lake form, meaning here, as a matter of course, mature individuals only. In younger specimens there are fewer spines than in the adults of the same surroundings, and therefore the young fresh-water forms have the same number of spines at a certain age as the adult forms of the salt lake, which demonstrates the retarded development of the latter." And further on: "We now find, in comparing the fresh-water generations with the salt-water generations of *Daphnia rectirostris*, that the latter generations are not only changed in consequence of the immediate effect of the surrounding elements, but also in consequence of retarded development under their influence; and, furthermore, that sexual maturity shows itself in the salt-water generations earlier than the complete typical development of the body-parts. The termination of the sensory antennae, the colour of the body, the lesser pinnulation of the bristles in the salt-water generations are principally dependent upon the immediate effect of the surrounding elements. The smaller number of the above-mentioned spines on the post-abdomen principally depends upon the retarded development under the influence of changed surroundings."

We thus see that the change of environment makes itself felt in various changes in the anatomy and physiology of the species investigated. The same conclusion holds good in the case of *Branchipus ferox*, another Crustacean which inhabits both salt and fresh waters. The differences relate to the length which the egg-sac attains, the segments of the animal, its shape, the length of the furcal lobes, and the bristles of the latter. "The most important difference," says Schmankewitsch, "consists in that while in *Branchipus ferox* of our salt ditches the furcal lobes have both edges bristled, in the fresh-water form only the inner edges of the lobes are bristled, etc." And he again says, "Had I not found all possible transitory forms between salt-ditch and fresh-water specimens, had I not convinced myself of the variability by domestication of this form, I should have regarded the salt-lake specimen as a new form."

Other investigations have shown M. Schmankewitsch that *Daphnia degenerata* is merely a changed and degraded variety of *D. magna*, while the latter is an intermediate form between the typical *D. magna* and *D. pulex*. But among Phyllopods no species seem more sensitive to the influence of the surrounding element than the genera *Artemia* and *Branchipus*. Changes in environment are apt to produce such variations in the same generation that two closely allied forms hardly admit of distinction. For instance, *Artemia salina*, which lives in water whose concentration varies from 5° to 12° of saltness, exhibits at high concentrations (12° or 15°) strong tendencies towards the form of *Artemia milhausenii*, which is a form able to live in water at 24° or 25°, in water where the self-deposition of salt is imminent, and between both forms all transitional types are found, which show that both are really of the same species, since *A. milhausenii* may be obtained from *A. salina* through the increase of the proportion of salt in the water. And we cannot escape the conclusion that either our species may be transformed into each other, or that the characters upon which they are based are worthless and require a severe revision.

To end with external characters, I shall merely remind you of the facts which illustrate the influence of environment on integuments; merino sheep lose their wool in warm climates and recover it under cold skies; Iceland cattle have short and small horns which develop well under mild temperature; the silky covering of the common hen, in Guinea, becomes transformed in Europe into the feathers we are accustomed to see on our domestic fowl, etc. Such instances are very numerous, and since the anatomical change follows immediately upon the change of conditions, we are at liberty to ascribe the former to the latter.

But we must now turn to more important changes in internal structure or functions, which may be experimentally produced at will through environmental changes.

Schübeler has shown that seeds may, through a change in climate and environment, be made to become larger than usual, and also to exhibit greater rapidity in the germinating process. At the same time the plants grown from the seeds are more coloured than in their accustomed climate. This fact may be ascribed to the influence of the greater light in northern regions, and light, it is known, is apt to make up for temperature, within some limits, in plant growth, as De Candolle and others have shown. At all events the fact that climate or environment reacts on the germinative faculty is a positive one.

It is well known that the same animals or plants, in different climates or conditions, exhibit marked differences in their physiology and intimate functions. Grape-vines transferred from the Rhine valley to Madeira require but a few years to yield Madeira wine, very different from the Rhine wines. It seems, from the experience of vine-growers, that the taste of the grapes and wine depends largely upon the chemical composition of the soil, some soils being very unfavourable and always yielding wine or grapes of inferior taste. Of course, differences in taste are due to variations in the chemistry and physiological processes of the plant and fruit. The same fact may be observed with most vegetables; all possess a pleasanter flavour when grown in one sort of soil than in another. In some cases these internal differences are accompanied by external differences, and M. Saint Lager has thus been led to consider *Ulex major*, *Trifolium Molineri*, *Cirsium anglicum*, and *Rhododendron ferrugineum* as forms of *U. parviflorus*, *T. incarnatum*, *C. bulbosum*, and *Rh. hirsutum*, due to the influence of soil containing much silicon, while the latter inhabit soils containing much lime.

Some mutilations react on the process of fructification, if it be true that by cutting through the medulla of a grape-vine stem, grapes are obtained which contain no seeds. The fact has been often spoken of for at least 1,900 years, as ancient Romans practised it, according to trustworthy witnesses, such as Columella, Camulogen, and Pliny.[1] At all events the experiment is worth trying, and some interesting facts might be derived from it.

Colour variations may be experimentally determined through changes in environment. Moleschott has observed that in pure oxygen no black pigment is generated in the skin of frogs, and the coloration of birds' eggs sometimes varies with the amount of light to which they are exposed. These facts are enough to point the way to many interesting experiments.

We can also produce changes in sexuality, since we know that the want of suitable depositing ground for trout, is, according to Barfurth, followed by degeneration and permanent sterility, or at least by the production of weak forms. In many cases also, deficient nutrition of parents determines predominant maleness in the progeny, and Giard has shown that *castration parasitaire*, as he calls it, that is the presence

[1] *Cf. Revue Horticole*, 1884, pp. 6 and 219; Couverchel, *Traité des Fruits*, 1839; Columella, *De Arboribus*, and also Olivier de Serres, who knew of the process, but denied its efficacy.

of parasites, affects in a marked degree some sexual characters, males being made thus to resemble more or less females in external secondary characters, while complete sterility may be induced through action on the essential sexual parts.

Fertility may thus be affected in many manners, by want of space (Semper), by nutrition (Maupas), etc., and many facts also go to show that external influences have a good deal to do with the nature of segmentation and even with the occurrence of parthenogenesis.

As concerns physiological characters, we are also able to induce much modification, by changes in pressure for instance, or by altered food, etc. Through gradual increase of heat we may, as Dallinger has done, accustom certain Monads to live at temperatures which are deadly to them in other circumstances; Chauveau has shown us how we can profoundly alter the physiological characters of micro-organisms, etc.

At least it seems to us that this conclusion fairly follows from the foregoing facts. But we cannot proceed without saying a word of Weismann's position in regard to these facts. In one of his essays, *On the Supposed Botanical Proofs of the Transmission of Acquired Characters*, written a few years ago, Weismann has discussed some cases which are similar to some of those I have related. One of these cases, brought forward by Detmer, is that of the shoots of *Thuja occidentalis.* These shoots contain in their upper side green palisade cells, while the under sides possess green spheroidal cells. And when the branches are turned upside down, and are fixed in this position, the anatomical structure of the shoots which put in their appearance later is reversed ; the side which was destined to become the upper side, and which a change in the branch's position has made the lower side, assumes the structure of the lower side, and *vice versâ.* A similar case occurs with the climbing shoots of the ivy, and in *Tropaeolum* leaves important structural changes have been noticed by Detmer in response to differences in external influences, or change of environment. Now Weismann says, "Such differences [in structure] do not by any means afford proof of the direct production of structural changes by means of external influences. How would such an explanation be consistent with the fact that the leaves are, in all these cases, changed in a highly purposeful manner? Or is it assumed that these organs were so constituted from the beginning that they are compelled to respond to external conditions by the production of useful changes? Any one who made such an assertion nowadays, or who even thought of such a thing as a possibility, would prove that he is entirely ignorant of the facts of organic nature, and that he has no claim to be heard upon the question of the transformation of species." These are rather big words, and Professor Weismann has perhaps written somewhat hastily. It may be answered that all evolutionists, and more especially "Natural Selectionists," to whom Professor Weismann belongs, assume the production of "useful changes" with or without change of external conditions, since those only survive in the struggle for life who offer beneficial modifications or adaptations. And of course the production of such "useful changes" is of much higher importance when the environment changes than when it remains unmodified. Professor Weismann denies the importance and transmissibility of variations due to external modifications. But then how does he explain the fact—now repeatedly ascertained in all bacteriological laboratories—that all micro-organisms, bacilli, bacteria, etc., undergo under cultivation in different external conditions—whether of light, heat, or food, it matters little—such important modifications that they may be made to lose their essential characters, and that these characters are lost as long as the external modification persists? Take *Bacillus anthracis*, for instance. Compared with many other bacilli, it differs very slightly in external characters; the principal and all-important difference is that it determines in many animals a disease of a very precise character which can be mistaken for no other. But this bacillus, if you alter even very slightly some of the external conditions it lives in, gradually loses its most important character; it is deeply modified for the time being—that is for the time you compel it to live under particular conditions. and perhaps even for some time the normal conditions are restored. The usefulness or unusefulness of the modification are not in the slightest degree apparent, and to evolutionists it matters little, since modifications may be useful, useless, or indifferent, or even injurious, and natural selection destroys all that is not useful, or at least indifferent; but the main fact is that here is an important modification which puts in its appearance when some external conditions are changed, and disappears when the normal condition reappears. Is environment operative here, or must we assume that when *Bacillus anthracis* is cultivated in some particular manner, it loses its most important character without any reference to the change of environment? I dare say no bacteriologist or physiologist could be met who would venture to assert that the change of character is not in direct relation with the change of external conditions, whether the former is beneficial or not.

It seems, then, that Prof. Weismann goes certainly too far when he asserts that we have no proof of the direct production of transmissible changes by means of external influences. It may be said that he restricts his denial to the Metazoa; but this is assuming a physiological contrast to the unicellular forms of life which it will not be easy to justify. On the other hand, we possess, in the facts of domestication and cultivation, a large number of cases of variation—which occurs in every part—due to environment, and transmitted by inheritance in various degrees. Psychology affords similar instances: a kitten which has never seen a dog is afraid from the first moment it perceives one; young birds of many species instinctively fear the hawk and other birds of prey, while remaining unaffected by the presence of other birds. Are these not psychological "attitudes" due to environment (acting on the *mens* of ancestors) which have been transmitted by inheritance; are these not *acquired characters?* I would recommend, in regard to this discussion, two recent papers: Mr. J. A. Thomson's *History and Theory of Heredity* (*Proc. Roy. Soc. Edin.*, 1889), where the writer gives his reasons for not accepting Weismann's extreme views, and E. B. Poulton's *Theories of Heredity* (Midland Union of Natural History Societies, 1889). The latter seems more favourably disposed to Weismann's theory, which he has greatly contributed to spread in England through his excellent translation of the *Essays on Heredity*.

LECTURE V

Summary:—Experimental Evolution based on the four preceding Groups of Facts. These Facts illustrate at the same time its Methods, which are: Change of Environment; Use and Disuse; Natural Selection; Sexual Selection; and Physiological Selection. These Factors of Evolution must all be subjected to Experimental Test in order to show what they can Effect. What is wanted: A Direct Proof, which all may Perceive and Touch, of one Species (or Form) giving Birth to another more or less Different, and Permanent. Numerous Accessory Problems to be Investigated at the same time. Scientific and Practical Import of this Line of Investigation. Requirements: Farm and Laboratory; Animals and Plants; Time; Experiments must be able to last 20, 50, 100 Years or more. This Experimental Investigation must and shall be performed. But who is to begin?

We have thus shown that a high degree of variability exists among animals and plants in the natural state as well as under domestication, and that through the modification of environment, in part or in whole, we are able to determine some changes in organisms. This variability is met with in all parts of the body, even in those which seem to be the most permanent.

What does this demonstrate? it may be asked. And the objection arises: What does it matter that variability occurs in all animals and plants, even to a large extent? Of course, if all parts of any animal or plant were to vary to the extent which has been shown, variability might explain the production of new species; but do they really vary to this extent in any one individual or group of individuals? Certainly not, and we must admit that variability is limited, and that it is not, so far as we know, sufficient to create new species. The differences are not numerous enough.

What, then, is the use of the series of facts which have been examined, and what do they show?

They provide a basis for the study of variation, specific or otherwise, in showing that no species are so very permanent in their structure or functions that no departure from their type is possible. They form the solid ground on which evolutionists stand, and if this ground were missing the evolution hypothesis would have no support at all, and would be nothing more than an aerial and unfounded structure. In the second place, they provide the basis and suggest the methods for experimental transformism.

Believing as we do that transmutation or evolution must have taken place under the action of natural causes and influences, we consider that these causes have been natural selection and environment, in proportions which we cannot determine. But these causes cannot have had any influence if the species do not admit of some variability; and this very variability itself we consider as being in some degree the measure of their operativeness and influence.

We are thus impelled to conclude that transformism admits of experimental investigation, and this is the main point we wish to establish. If the present species have really originated from the more or less closely allied species which have lived in the past, if the present has really been evolved out of the past through natural agencies, without any special intervention of any force, we do not see why there might not be in the future forms evolved out of the present, and why we could not evolve them ourselves, in part, and help towards their production, through the use of the methods which we believe to have been used by Nature herself. If we do not succeed, we are either mistaken in our general idea, or mistaken in regard to the methods through which evolution is supposed to have taken place; but we can really draw no conclusion at all as concerns these methods, as long as we have not subjected them to experimental test.

I do not propose to show all that can be done in this line; the matter would require more time than I can devote to it, and on the other hand I am firmly convinced that much is to be done of which we have at present no idea at all. As Dareste rightly says:

"We may rest assured that the execution of experiments will cause a great number of questions to arise, of which we can at present have no idea. This is one of the great advantages of experiment. If we do not always find what we are seeking after, while starting from hypotheses which the facts do not support, we often find what we were not looking for, and light is thus cast on regions which till then seemed buried in complete darkness."

This I consider as exactly true, and many unexpected questions will certainly turn up of which we have no idea, while answers to others may also be found. But, if it is impossible to state exactly what will be done, we may at least gather some idea of the principal methods and hints of experimental transformism.

The methods first.

What can the methods of experimental transformism be? The only answer to this question is based on the consideration of what the Factors of Evolution are or are supposed to be. At the present moment five are usually recognised.[1] I quote from Le Conte's able paper of recent date :

"First. Presence of a changing *environment* affecting functions, and functions affecting structure, and the changed structure and function inherited and integrated through successive generations indefinitely.

[1] *Cf.* Herbert Spencer's *Factors of Organic Evolution*, and Le Conte's *The Factors of Evolution* in *The Monist*, April, 1891.

"Secondly. *Use and disuse* of organs reacting on growth-force, and producing change in form, structure, and relative size of parts, and such change inherited and integrated through successive generations.

"Thirdly. *Natural selection* among individuals of a varying progeny, of those most in accord with an ever-changing environment, or, as it has been otherwise called, *survival of the fittest* in each successive generation.

"Fourthly. *Sexual selection.* The selection of females among varying male individuals, all competing for the possession of the strongest or the most attractive. Among mammals the selection is mainly of the strongest as decided by battle; among birds, of the most attractive, as determined by splendour of colour or beauty of song.

"Fifthly. *Physiological selection*, or selection of those varieties, the individuals of which are fertile among themselves, but sterile or less fertile with other varieties and with the parent stock. This has also been called *segregate fecundity* by Gulick, and homogamy by Romanes."

These five factors are not all recognised by the same group of evolutionists. The two first factors are Lamarckian; the third and fourth are Darwinian; the two first are supposed to operate during individual life; the third and fourth operate on the offspring, which has more or less departed from the parental type. For some time an important discussion has been carried on, especially in England, Germany, and the United States, concerning these factors between Lamarckians and Darwinians, as to their efficiency and frequence.

I cannot enter upon the discussion, which would require much time, and in fact it might seem rather early yet to discuss the *quomodo* of a fact whose existence is not proved to the satisfaction of all whose opinion is of any weight. Lamarckian views are held especially by American and some French evolutionists, while in England and in Germany strict Darwinian theory prevails. It must be said, however, as concerns the Lamarckian theory, that, as Le Conte has well remarked, the Lamarckian factors of environment and use and disuse, are the most fundamental in importance, and first in order of appearance. Selective factors are conditioned by reproduction, and on sexual reproduction particularly in which the characters of two diverse individuals are blended in different proportions in the individuals of the same progeny; sexual generation thus provides material for selection to operate upon. But where no sexual generation occurs—and this is the case with the lower forms of life which were first evolved, and out of which the higher forms are supposed to have developed—Weismann urges that selection is impossible, since the progeny is nothing more than a part of the parent form, an outgrowth thereof, the two being so very similar that in most cases none can tell which is the parent, and which the progeny. It then results—if really no selection can occur when sexual reproduction is wanting, and this is a matter which is not settled by Weismann's very sweeping assertion, with which I cannot concur, as there is no reason why some degree of variability should not exist in unicellular organisms as well as among multicellular plants or animals—that Lamarckian factors must have operated, and must operate even now, if evolution has existed from the beginning, and has been carried on through natural agencies. Weismann strongly argues that at present, at all events, or as concerns higher animals, the Lamarckian factors are possessed of no influence, and his essays on heredity are all against the heredity of acquired characters.

Such is the present state of the question. I have no intention to discuss the matter, as I have already said; but I think it may be in the future discussed in a much more profitable manner than has been done till now. And this may be effected through experimental transformism. If we are to subject the evolution theory to the test of experiment, we can only do so by the investigation of the efficiency of the factors of evolution, and we must subject them to the said test.

This method cannot fail to be highly profitable to the discussion ; I do not think of any other that, at present, can settle the matter ; and if we are to know something some day about the general fact of evolution, and the methods through which it has been going on, and may in future go on, it is through experiment that our knowledge will be acquired.

Whatever our opinions may be as to the real value of the factors of evolution which have been suggested on different sides, all must be subjected to the same test, that of experiment: the results will allow us to decide upon the theory itself, and upon the details of the process.

Such is the general view. As to details, now, I must confess that we are rather in the dark as yet. At first we shall have to grope about somewhat, searching for the ways in which the experiments may be performed, and for suitable organisms upon which we may experiment. Every one of the recognised factors must be investigated.

Concerning environment, we may operate on many sorts of animals and plants. A first and simple method will consist in transferring animals and plants from one country to another, or from mountains to plains, or *vice-versa*, from dry to moist soil, from cold to warm, from calcareous to siliceous soil, from one pond in one sort of soil to another pond in another soil, from light to semi-darkness, from land to water, &c. The experiment may be performed in a thousand ways, and all external differences, all changes in environment which seem to operate on organisms may be successively tested. Care must be taken, however, to watch the experiment without interfering, and the animal or plant must be left wild, without domestication or cultivation, so that they are exactly in the condition of a species transferred from one set of conditions to another without being particularly helped to support the change. Other experiments may be performed in a different manner. Instead of altering slightly all conditions of environment as in the preceding case, we may alter only one: for instance, while keeping the animal or plant in its native climate, alter the proportion of chemical components of the soil, alter the nature of food, add some new compound to the one or the other, increase or decrease motion around it, &c. All the facts mentioned as illustrating the influence of environment show how numerous and varied are the experiments which may be performed, and it is needless therefore to repeat what has already been said on the matter. If the variations of environment have really effected the result noticed, such result must be again obtained by direct experiments.

The fact is however, that in such experiments, the difficulty lies not in the experiment itself, but in appreciating the results. Such results are not always external and obvious; many are internal and require chemical and microscopical investigation in minute details, and such differences in chemical constitution or in structure may have a great influence in the struggle for life and operation of natural selection.

In reference to use and disuse, experiments may be made to diminish or suppress the activity and use of some organ, by keeping plants or animals in such conditions as to render some character useless. For instance, one might try to obtain unscented or plain flowers through artificial fecundation of all the flowers of the same plant which require insect-intervention, as it is supposed to have developed scent or colour in view of attracting insects. Or again, place any animal in such conditions as to render any one function useless; or also, such as to render the development of some organ or function of great use and necessity. Experiments on the inheritance of mutilations may be repeated at the same time: those which have been already performed have not been successful nor sufficient, although *a priori* it seems most likely that the result will be exactly what it has been. For such and other experiments bearing on the question, intending experimenters may be referred to the essays of Weismann, and W. P. Ball's *Effects of Use and*

Disuse, where they will see what has been done and what may be attempted. Care should be taken to operate preferably on useless characters. But are there any useless characters?

It is a rather curious fact that while the operation of selection is recognised by most evolutionists, even if holding Lamarckian *versus* Darwinian views, but few experiments, as such, have been yet performed, although observations are plentiful. Among the best which have yet been made, I must refer to those of Vilmorin,[1] performed many years ago, and published for the first time in the *Transactions of the Horticultural Society* in 1840 (2nd Series, vol. ii., p. 348). M. de Vilmorin, considering that most of our food-vegetables are derived from species which have been altered by man, and that the most interesting point to investigate is the methods through which the alteration has been obtained, notices the fact familiar to all, that while species which have been a long time under cultivation vary easily and in many directions, those which have been less cultivated, or have not been cultivated at all hardly exhibit any variation. Such has been the case with this writer in his experiments on *Lactuca perennis*, on *Tetragonia*, on *Solanum stoloniferum*, on *Brassica orientalis*. But

[1] See his *Notice sur l'Amélioration de la Carotte Sauvage* in *Notices sur l'Amélioration des Plantes par la Culture*, Paris, 1886.

in the case of the wild carrot circumstances have been quite different, and through *selection*, artificial of course, he has obtained very precise and interesting results which it may be useful to quote here.

In 1832, M. de Vilmorin, wishing to obtain from the wild carrot plants with thick and edible roots, planted some seeds of the wild plant. All the plants thus obtained grew quickly and yielded seed, while no root was any better than that of the common wild carrot. He began again in 1833, and among the seeds planted, many were late in germinating and no seed was produced, while some roots were somewhat larger and thicker than usual. These roots he selected and put apart so as to plant them in the following spring, and they yielded seeds in 1834. The seeds were again planted in 1835. Many gave plants with the ordinary wild carrot root, but a rather large proportion ($\frac{1}{5}$th) yielded plants with thicker roots. The seed of these plants was selected, and planted in 1836. Selection again was performed, so that in 1837 many good roots were obtained. In 1838 and 1839 the process was continued, with the result of yielding a large proportion of satisfactory carrots ($\frac{9}{10}$ths). While acquiring different dimensions, the roots acquired also unusual colour: yellow, lilac, and even red.

Here we have a good instance of the selective process and of its influence and operation. Another is yielded by experiments on the beetroot, performed some years later by the same writer, with the view of obtaining a variety of this plant containing more sugar than is commonly the case.[1] It is worth recording, as it shows that through selection it is possible to influence physiological variability, and the result has been to increase the proportion of sugar from 10 on an average to 12, 14 and even 16 per cent. M. de Vilmorin notices a fact which it is well to state here, when he says that it is better to select seeds from plants belonging to a group with high average than from plants yielding high maxima but also low minima.

A large number of facts from observation confirm these results of experiment. Our domesticated animals, our cultivated plants have been made to yield so many varieties simply through selection. While cultivation or domestication increases the tendency towards variation as we all know, selection of variations has led us to produce quite a number of varieties of which we make use in very different manners, because different variations have in turn been selected according to the particular wish of the selectors, or to the peculiarly interesting nature

[1] *Note sur un Projet d'Expérience ayant pour but d'augmenter la Richesse saccharine de la Betterave. Loc. cit.* (1890), and *Note sur la Création d'une nouvelle Race de Betterave à Sucre. Loc. cit.* and *Comptes Rendus*, Nov. 1856.

of the variation. On this point I shall merely refer those who desire more information to Darwin's book on variation: instances are there most numerous and convincing, as concerns animals and plants, and are enough to show the power of selection. I may also refer to some recently-noticed cases. One concerns the production of a new variety of hornless oxen. In 1874 a Sicilian farmer noticed among his herd a young bull which had no horns at all. This young bull was allowed to mate, and the result has been the production of other hornless animals, so that, by selecting at each generation the progenitors of the following this farmer has obtained a hornless variety. A similar fact occurred in 1861 in a village of the Meuse department. A cow gave birth to twins, male and female, deprived of horns: they were mated together, and thus, by constant selection of hornless animals as progenitors, a hornless variety has been also created. The breed of Mauchamp sheep has similarly been evolved out of a ram which was born in 1828, in the Mauchamp farm, with the peculiarity of bearing an even wool, instead of having it frizzled, merino-like. And M. Cornevin, from whose work I abstract these facts remarks that any day any breeder in the south of France can, if he chooses, produce by simple selection a variety of sheep with four udders, for these animals often bear four of these organs. From what we have seen, and that which has been done, purposely or unconsciously by man, we may infer that every possible variation may be permanently consolidated as a normal and fixed character, through selection, through the choice of progenitors based on the similarity of the character which it is required to render permanent.

Some points concerning selection require a passing notice.

In the first place, scientific investigation being the only aim, the only point in view, it seems advisable to undertake the study of the influence of selection—be it on animal or on plant—without any particular forethought at all. I mean by this that such investigations should be begun without any view of obtaining a variation and variety in any particular direction. For instance suppose *Lysimachia nummularia*—I quote the first name which occurs to my memory, without the slightest choice among those which come with it—is made the subject of an investigation in selection. Well, it would not do to decide beforehand to seek for a new variety having such or such a peculiarity in the roots, or stems, or leaves: one should merely cultivate the plants, and if among them some offered any interesting or curious variation in *any* part whatever, one ought to begin the process of selection, and try to consolidate in the progeny this particular variation. This method offers the advantage of opening a wider field to investigation, and on the other hand, animals or plants which, through culture have been brought to vary in *any* direction, are more apt to vary in the one particular direction which may be desired. Even in cases where one particular variation is desired in order to create a special variety, de Vilmorin rightly advises as follows :—" In order to obtain from a yet unmodified plant, varieties of a determined character, I would strive at first to make it vary in any manner, and would choose, as progenitors, not the accidental variation which would most nearly approach that I am seeking for, but merely the one which offers the largest departure from the type. In the second generation, I would again select as progenitors the plants which are most different from the normal type, and, at the same time, from the progenitors selected in the previous generation."[1] In fact, as Vilmorin says, we must try to *craze* the plant, to make it vary as much as possible, in all possible directions ; and it is only when this result has been obtained that it becomes advisable to select the variations in the desired direction.

We should always remember that plants and

[1] *Note sur un Projet d'Expérience ayant pour but de créer une Variété d'Ajonc sans Epines. Loc. cit.* p. 35.

animals may vary in a large number of directions, that variability increases with variation, and that any desired variation is more apt to occur among plants whose tendency to variability generally has been considerably increased through the process which has been mentioned. And then also, it is well to remember that while proceeding in this manner, though we may not meet what we desire, we may meet with very unexpected variations which may prove of even greater interest than that which we are seeking.

In the second place one must not forget that in experiments of this kind, especially with wild or uncultivated plants, a long time is sometimes required before any important variations occur; the species seems for a long period to resist all inducements to variation, and then, all of a sudden, it begins to vary considerably and in many different directions.

So much for experiments on selection. While speaking of selection, a word may also be said of the method which is in some sense exactly the reverse of selection; I refer to crossing. While in the course of selection progenitors are chosen among the animals which are the most similar to each other, in that of crossing, on the contrary, the progenitors are of different type, and crossing is performed in order to obtain animals or plants—which combine the character of both parental forms. In natural conditions, crossing occurs both among animals and plants, although, generally speaking, among animals at least, there is a marked tendency against the mating of unlike, and towards that of similar animals. It has even been noticed that in the same town—I have heard of the fact in Florence—the pigeons congregate in flocks according to their colour, and keep together, and mate together, while they seldom do either with the pigeons of a different flock. In many cases, crossing gives origin to hybrids which are unable to reproduce their own form, so that the process is not of much use if new varieties are required. In other cases, however, the varieties produced by crossing are fertile *inter se*, and it must be noticed, that when crossing does not seriously impair or totally destroy fertility, it increases it in a marked manner. It is on account of this increase of fertility that crossing is often resorted to by breeders or agriculturists, and this increase has been demonstrated by Darwin for plants, and observed by many persons in animals. Cornevin quotes an instance when a flock of sheep yielding 6°/ₒ twin births had its percentage increased to 13 through the introduction of a ram of different breed. The same is true of hogs, of pigeons, and other animals.

If crossing is to be used in experiments on Evolution some points must be particularly attended to.

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Observation has shown that crossing is beneficial in some cases only, and many persons have spoken unfavourably of it. This is doubtless due to the fact that we are yet ignorant of some conditions which are of high importance in the matter, and it would seem that facts of apparently little significance have a great deal to do with the ultimate result. Fertility and sterility may be induced by very feeble external agencies, and at the beginning results may be obtained in crossing, which are quite unfavourable, and quite unlike those which are obtained later on. On the other hand some conformity must exist between the plants or animals between whom the crossing occurs. When it does exist—and, in fact we hardly know when and where it does or does not—the results may be unfavourable in that the characters of the parents, though apparent in the progeny, are not blended together, as was expected ; they are separate, and may be seen side by side instead of being combined. Such is the case, for instance, when white-seeded peas are crossed with green-seeded varieties ; in the progeny green peas are met amidst white seeds, some seeds are green striped with white ; none are between green and white. Similar instances are met with among animals when among the progeny of the same parents, some exhibit the character of the one, and others those of the other parent. Another point which requires consideration is the fact that of the two progenitors in an intended crossing the one is generally—as a variety—possessed with stronger hereditary tendencies than the other, and the result is that the progeny takes more after one of the parents than after the other. For instance, in the crossing of the Angus bull with the Dutch cow, the progeny has more characters of the former than of the latter, and among plants, according to Cornevin, *Vitis rupestris* is said to transmit its characters very markedly when crossed with other varieties.

Such varieties or individuals display what may be called *predominant* heredity ; by this is meant that in crossing the hereditary tendencies of one parent lord it over those of their mate. This is true of females as well as of males ; it does not seem to belong to the one sex more than to the other, and among horses and oxen *Eclipse* and *Duchess* have been renowned for the predominance of their own qualities over that of their mates. Among plants, instances also occur, and are well known, although in many cases horticulturists vainly strive to find individuals thus endowed. Many have sought to discover *Ulex europaeus* devoid of spines, able to transmit this peculiarity to their progeny—this would be a very valuable conquest, as *Ulex europaeus* is good fodder but requires to be crushed so that the cattle do not injure themselves—but none have obtained it yet. And this is but one instance among a thousand. Why, and how it is that some individuals are thus endowed, we know not. It must be noticed that the pre-eminence of one of the progenitors, as a race or variety is not constant; while variety *a* for instance, possesses stronger hereditary tendencies when crossed with *b* and *c*, its tendencies are overpowered by those of *d* in a crossing with this variety; and in order to ascertain exactly or more accurately the relative energy of these tendencies, many systematic crossings would have to be made. This pre-eminence of one of the progenitors over the other, in regard to the character of the progeny may in some cases be so considerable that no difference is apparent; it would seem that no crossing has been made and that both progenitors belong to the same variety.[1]

As M. C. Dareste[2] rightly observes, "in the present

[1] It may happen, however, that the difference, as concerns external characters, is well marked, but then one marked physiological feature may be transmitted from one of the progenitors to the hybrid; for instance, M. Millardet, in his *Essai sur l'Hybridation de la Vigne* (Paris, Masson, 1891), shows that in some hybrids between two varieties ot grape-vine, the marked immunity of one of the progenitors—the male especially—towards phylloxera, is transferred to the progeny, notwithstanding the reverse tendency in the other progenitor. The same writer notices that when the American variety is used as male, immunity towards phylloxera is considerable, while the amount of fruit produced is smaller; the reverse obtains when the American variety is used as female. Similar facts have been observed by Th. Niebner, *Die Rose*, Berlin, 1880.

[2] *Nouvelle Exposition d'un Plan d'Expériences sur la Variabilité des Animaux*, p. 16 (*Bull. Soc. Zool. Acclimatation*, 1888).

condition of science it is nearly impossible to foresee what will be the results of a crossing . . . . . But we may assert that this method will yield very interesting results, and all the more so that the crossed varieties will be more dissimilar."

This is quite true, and we cannot wonder at it when we consider how many different forms heredity is able to assume. Accepting Cornevin's recent classification, we recognise the following modes:

1. *Predominant or Unilateral Heredity*, where the hereditary tendencies of one of the two mates appear predominantly or exclusively in the progeny.

2. *Bilateral Heredity.* Here the progeny has characters of both progenitors; and it seldom, if ever, happens that the progeny has both sorts of characters to the same degree, or in the same proportion; one of the progenitors has more influence than the other. Both sorts of characters may fuse and combine together; white and black begetting gray for instance; or may coexist, remaining separate, the progeny having some traits of the father, and others of the mother. In this sort of heredity four cases are possible: Heredity is *Direct* or *Crossed*, *Equal* or *Unequal*. These terms require no explanation.

3. *Atavistic Heredity*, or *Retrogression*, which may also be *direct*, *crossed*, or *collateral*. In this case the predominant characters in the progeny are charac-

ters belonging, not to the progenitors but to their ancestry, near or remote. Many anomalies in organisation are but reversions to former ancestral types; and reversions exist to a less degree in the many cases when a child, for instance, resembles his grandfather or great-grandmother by some marked peculiarity instead of possessing the traits of one of his direct progenitors.

4. *Indirect atavistic Heredity*, or *Heredity through Influence.* An instance will explain this form. Lord Morton caused a mare to be mated with a quagga; she gave birth to a striped hybrid. Mated the following year with a thoroughbred, her progeny was again striped, although not hybrid; and this occurred three following years, although she was but once mated with the Quagga. The influence of the latter—and we do not know what we exactly mean by *influence* in this case—had incorporated itself as it were, in the mare's organism so deeply as to make itself felt even three years after. Such cases are met in human marriage. They are, as yet, unexplained, but they are occasionally met with, and positively ascertained.

5. *Homochronous Heredity.* This term applies to cases where psychical or physical peculiarities put in their appearance among the progeny, at the age when they appeared among the progenitors. Many pathological tendencies are transmitted after this mode, in

man as well as in animals, and this form of heredity may be direct as well as crossed.

6. *Reinverted Heredity.* The progeny may, at first, and during some time, closely resemble one of the progenitors, and later on, resemble the other.

7. *Homotopic Heredity.* In this case, a given peculiarity of one of the progenitors is met with in the progeny exactly in the same part of the body as in the former; for instance, a differently coloured lock of hair, or a *naevus maternus* similarly placed in progenitors and progeny. This form may be also direct or crossed.

8. *Heterotopic*, or *Homohist Heredity.* Peculiarities in one part of a progenitor may be transmitted to other parts (made of similar tissue) in the progeny; the same disease, for instance, may assume one form in progenitors, and another in the descendants.

The natural sequel to experiments in crossing will be experiments in hybridisation, that is, crossing between distinct species. New hybrids must be obtained, and the number of those which are fertile *inter se* must be largely increased. And then, will the latter hybrids form permanent varieties or species? Experiment only can decide. And while experiment and artificial impregnation may go a long way in creating entirely new and unexpected forms of life, which may be of great interest, particular attention must be paid to the production of hybrids under natural conditions. This study of hybridisation, scientifically conducted, will certainly yield very useful results in being conducive to experiments concerning that very marked variability in the reproductive function which in so many cases determines sterility where fertility might have been expected, while fertility occurs where sterility would have seemed natural. It seems that the process of impregnation is of the most delicate sort, and very slight circumstances—slight they seem at least—determine its success or failure. Investigation of these circumstances cannot but prove profitable.

Many experiments may also be performed in regard to sexual and physiological selection, which are connected with those on crossing and hybridisation, and Mr. Romanes has already suggested some.

Such are, briefly stated, the methods of experimental transformism. I sincerely hope and expect that some years hence, perhaps when all of us shall have become things of the past, the lecturer who shall have assumed the pleasant task which I here fulfil, will have much more to say on this topic, and especially will be able to say : "this and that have been done," instead of the "this should be done" which is the somewhat monotonous conclusion of each of these lectures.

And now, what are the aims of experimental transformism?

As stated, from the beginning, we wish to test the theory of evolution, as applied to living beings. This is the main object: but I wish to call your attention also to the practical and utilitarian results which may be expected and attained. As you all know, most, if not all, of our garden vegetables and plants are the result of man's industry, and in the cases where the original wild form still exists, we can well measure the distance between Nature's product and the perfected form shaped by man, and best adapted to his needs. And if man had not undertaken the task of bettering—bettering meaning here simply adapting more adequately to his needs—Nature's work, many of our important foods would not have existed.[1] The same is true of animals. Compared with our domestic animals the wild forms sink in insignificancy as concerns usefulness, and here again man has been a most potent factor in creating out of the raw material those useful

[1] *Cf.* Asa Gray: *Were the Fruits made for Man or did Man make the Fruits?* (*American Natural.* vol. viii., p. 116.) The veteran botanist here showed that while some fruits and vegetables have hardly departed from their original wild type (huckleberries, cranberries, persimmons, etc.), others have been bettered and rendered more useful by cultivation (currants, gooseberries, raspberries, blackberries, chestnuts, strawberries), while others have been so much perfected as to represent new fruits (apple, pear, peach, etc.). He adds a list of fruits which man should endeavour to render more perfect for his own needs.

allies which provide him with meat, milk, wool, or energy. But, since man has been able to do what he has done, while devoid of knowledge and unconscious of methods, what may he not expect to perform now, with larger means, more varied resources, many centuries of accumulated science, and the certainty of success if he is persevering enough? The past is a promise for the future, and the results already obtained well show what we may attain to; the whole thing rests in our hands. Are we to believe that among the unnumbered species of animals and plants which are yet living in the wild state, none remain which may be cultivated or domesticated? Has all been done that was possible? No naturalist would venture to answer yes, and to say that man has reached his Pillars of Hercules. We are then entitled to expect many useful discoveries if we only set to work, and the field which lies open to us is infinite. Both organic kingdoms may be made to yield any number of new forms whose use we cannot even foresee, so short-sighted are we; and even if we were only to increase the number of the useful animals or plants, without varying the use to which either may be put, a great deal would doubtless have been done for the benefit of mankind.

But this cannot happen if we do not purposely set to work. In former times, when man lived in small or large tribes, widely separated, without easy means of intercourse, it was necessary for him to attend at all costs to his cultures and herds; it was a matter of life or death. Now, our most civilised nations are unable to produce all they need; some work in one line, some in others, and by cooperation and exchange every one is provided with the things he needs, and none feels the necessity of creating new resources. But civilised man must be made to understand that he can considerably increase the latter if he chooses. Such is the practical interest of experimental transformism, and it can in no possible manner interfere with its scientific aim; both are bound together.

As to the latter purpose, I must be content with a few words on the principal lines of investigation. Our main aim must be the study of evolution, that is of the derivation of living forms from each other, and the study of natural influences on the process of this derivation. If the evolution hypothesis is true, we must find that new forms of life may be evolved out of pre-existing forms, by means of influences actually operative in Nature, without man's or any other agent's interference. Of course, in experiments on evolution, man must interfere, but he does so only in order to test the real efficiency of what he considers to be the factors of evolution.

This general line of investigation is not a simple one; a large number of questions are intimately connected with it, and cannot be separated, concerning which information will be most valuable, from the practical as well as from the scientific point of view. Some of these questions may be briefly indicated.

First the question of variability in species, or other groups, and how they are established. External and internal or physiological variability must both be investigated anew, with the most delicate tests and methods, and particular attention must be directed to their causes. Of course, this study implicates that of species. What is a species; what are specific characters? If we consider many of the recognised species, we see that these characters are often of the most insignificant sort, and that many of the so-called specific characters are even of less value than those which are used to distinguish varieties. It may be predicted that terrific discussions will arise concerning this *vexatam questionem*, which seems to become more intricate every day, and that much sorrow will befall that numerous and well-disposed class of systematists, whose self-assumed task in life seems to be to increase the list of specific forms. Perhaps we shall thus understand what really makes a species; for while we talk much about them, we really do not understand what they are, and no thorough definition has yet been given.

The problem of heredity will also be investigated.

What is heredity? how does it operate? what is transmitted? We hardly know anything on the matter; we all have read a number of anecdotes of more or less unscientific character, and remain in the dark. Weismann's solid and heavy essays are certainly valuable, but facts must be added to reasonings, and the facts we need are mainly experimental. The whole problem requires a thorough investigation, although many facts are already ascertained, a great deal remains to be done to explain heredity, and to ascertain its limits and power. One important question is that of the heredity of Abnormalities and Mutilations. Many abnormalities, when not opposed to the continuation of life, are hereditary.[1] A good instance is provided in the case of the cats, mentioned by E. B. Poulton (*Nature*, 1883-6), and in the case of the Fodli tribe, in Arabia, where all the individuals, since very ancient times, have been born with twenty-four digits instead of twenty, and where they all marry within the tribe.[2] Many diseases are hereditary, under

[1] Paul Bert must be included among those who have somewhat investigated the subject, after Philipeaux, Bronn, and many others. He observed that if the eyes of young, newly-born rats are removed, death always ensues when the experiment attains the fourth generation, doubtless, says he, through some impairment of the optic lobes. *Cf.* his *Essais d'Expérience sur la Transmission héréditaire de certaines Lésions chirurgicales ; Relations trophiques entre les Yeux et les Lobes optiques. Comptes Rendus Soc. de Biologie*, 1870.

[2] Aira, *Bull. Soc. Anthropologie*, 1886.

the same or a different form. And finally comes that much-discussed question of the heredity of mutilations, negatively settled by Weismann, but which certainly requires much new investigation. On hybridism, sexuality, and many other points, useful facts will be discovered ; in fact, as has been said, we cannot exactly foresee the subjects which will naturally offer themselves to our investigation.

But there is enough to be done, even if experiment were to suggest nothing new, and the field which is opened to experiment, in the lines briefly indicated above, in the line of the investigation of organic evolution in its general sense, and in its details, is simply unlimited.

All these experiments can be made on any animals and plants, and in any country. What is required for their execution is an institution of some sort specially devoted to this line of investigation. It appears to me that this institution should comprise the following essential elements: rather extensive grounds, a farm with men experienced in breeding, agriculture, and horticulture, some greenhouses, and a laboratory with the common appliances of chemistry, physiology, and histology. Of course this must be located in the country. It is very important to have experienced farm-hands, and a good chemist and histologist are necessary in the staff of the institution. As to the general management, it seems advisable to have a director with a board of competent men, whose function would be to decide, after careful investigation and exchange of views, what are the fundamental experiments to be performed. These experiments, when once decided upon, should be pursued during a long period of years, and nothing should be altered in their execution, unless considered advisable by the board, or unless the experiment should be found useless or devoid of chances of success. The main thing should be to provide for the duration of this experiment, whether the originators were living or dead, and to follow it out for a long time. Time is an indispensable element in such investigations, and experiments of this sort will surely exceed the normal duration of human lifetime. But, as old Pierre Belon writes in his *Remontrances sur le Défaut de Labour et Culture des Plantes*, 1558 : "Il ne se fault pas excuser sur la longueur dù temps pour entreprendre choses séantes au bien public." Into the details of the work of the chemists, histologists, or physiologists, it is useless to enter ; the mere enumeration of the varied facts which have been quoted shows that their services are of the utmost usefulness, and are quite necessary for the investigation of the results. Any number of experiments of minor importance may be carried on at the same time, and surely the fact that they will be performed under good conditions, in a laboratory specially prepared for such investigations, will contribute greatly to the final success. The co-operation of many outsiders might be of great use. Young men might spend some time—some three, four, or five years, or more—in attending specially to some of the experiments in course of execution, in the investigation of some special points. Many friends of science could also do good work and help greatly by agreeing, for instance, to cultivate in various localities the same species of plant, or to co-operate in breeding special varieties of animals and reporting the results. In fact, all natural history societies, all laboratories, and all individuals could undertake a share of work, and among the individuals, naturalists, horticulturists, breeders, and pisciculturists would occupy a prominent part. The institution for the experimental investigation of evolution would thus be the headquarters for all that concerns evolution, and its influence would make itself felt in all departments of natural history, and thus create a strong current in the line which, sooner or later, must be opened.

I do not entertain the slightest doubt as to the fact that it will be opened. The thing must be done. It is a matter of money—as usual. But in civilized countries individuals or corporations are occasionally met who understand that mankind's glory lies not entirely in the invention of instruments of war and death, and that there are aims in life higher than mere money-making or enjoyment. There are two main aims in life—the benefiting of mankind, which may be performed in a thousand manners, and the pursuit of truth. Much money has already been given towards the accomplishment of these two purposes, and this allows me to hope that some charitable and enlightened persons may be found who will be able and willing to help towards the experimental study of evolution.

The matter is of sufficient importance when we consider that, in fact, nothing less is proposed than an application of experiment to the solution of one of the highest problems of science, and the one in which thinking mankind is most interested.

POSTSCRIPT.—Since the above lectures were delivered, and even in type, I have had the pleasure of learning that Dr. Romanes has circulated an appeal for an experimental institute essentially on the lines above suggested, which he wishes to see established in connection with the University of Oxford. There is also a prospect that the Granton Marine Station at Edinburgh may be more fully adapted to some department of this line of research.

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D.

THE END

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