PROBLEMS OF MODERN SCIENCE

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PROBLEMS OF MODERN SCIENCE

A SERIES OF LECTURES DELIVERED AT KING'S COLLEGE (UNIVERSITY OF LONDON)

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PREFACE

THE addresses printed in this volume were delivered as public lectures at King's College in the spring term of the present year. The intention of the organisers of the course was to place before the public the present position with regard to the advancement of science in some of its main branches, and to point out the directions in which progress is actually being made or may be hoped for in the near future. No uniformity of treatment was expected, and each lecturer was left to follow his own inspiration. Some have attempted a more or less comprehensive survey of the field allotted to them, while others have concentrated their attention upon smaller areas.

It is hoped that many who were unable to be present at the lectures may be interested in this brief review of the situation at a time when, in spite of the disturbances due to the Great War and the political and social unrest which have followed it, the activity of scientific workers is perhaps greater than at any previous period in the world's history.

ARTHUR DENDY

King's College, London
July 1921

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MATHEMATICS

By J. W. NICHOLSON M.A. D.Sc. F.R.S.

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I

MATHEMATICS

A COURSE of lectures on the Problems of Modern Science, designed on the present lines, may well raise the question of the exact position which the problems of Mathematics should occupy. Is Mathematics to be regarded as an Art or a Science? This old question has never been solved by any individual or corporate body, and for the sufficient reason, perhaps, that Mathematics is neither Art nor Science exclusively. Our University shelves the question entirely by placing the subject in three distinct Faculties—Arts, Science, and Engineering—this is perhaps the solution, and not a mere shelving. For it is necessary to realise that the subject is not like, for instance, any other scientific subject—it is a class of subjects of very different types, connected only by the one dominating characteristic of being logical accounts of some set of conceptions or of phenomena which can be stated in quantitative, and not merely general or qualitative terms. These conceptions or phenomena form the subject-matter on which it operates, and the subject-matter determines, in any case, whether an investigation is to be classed, though always roughly, as Science or not.

It is not a practical problem to select the branches of Mathematics to which I should refer as constituting its scientific side in accordance with the scheme of these lectures. The so-called 'useless' Mathematics merely means, at any point of time, those portions of the logical development of the subject which, for one of two reasons—either because they deal with mental conceptions not yet adopted by the more practical physicist or engineer, or because, while dealing with physical phenomena, they are concerned with deductions which have not been tested by practical experience in a laboratory—have not hitherto played any part in the elucidation of anything which makes a direct appeal to one of our senses. Such are the parts of the subject which are usually referred to as an Art, and they include a very large number of the most progressive developments of the subject at the present day, and of the most interesting and fundamental problems which mathematicians are endeavouring to solve.

But we have to face the fact that many of the most important branches of Pure Mathematics, worked out in the first instance on some purely æsthetic ground for their own interest in themselves, without any ulterior motive such as their application to anything concrete, have often, perhaps fifty or more years later, turned out to be just the material necessary for clarifying some group of natural phenomena and welding them into a compact whole. We have a striking instance at the present day in regard to Einstein's Theory, which I select as an illustration, not because many more equally striking, or even more so, could not readily be given, but only because Einstein's Theory is so prominent everywhere that I do not think a single one of my readers can have failed to pick up some information about it. Many years ago Riemann and Christoffel worked out what, until only just before the War, seemed to be perhaps one of the most 'useless' developments of Mathematics which the mind could conceive, from the point of view of ultimate application to anything in Nature—I mean the Theory of Tensors, whose only recommendation was then that it was very general and involved some interesting analysis of a new type, fundamental only as a contribution to mathematical logic. But this analysis has nevertheless turned out to be just what Einstein wanted for the development of his theory, and may well be described as its backbone. I shall quote at least one other similar instance later.

Such considerations are in no sense a 'justification' of Pure Mathematics—it does not need any, like all other logical creations of the mind, which have a beauty of their own. But I offer them as a justification for my own action in including some of the outstanding problems of the 'purest' Mathematics in my lecture, even when I can give no evidence directly that they will ever come strictly into the purview of Pure or Applied Science. In fact, I should like to define Mathematics as Quantitative Science, either of the present or perhaps of the future—excluding the actual registering of a formula which describes a phenomenon, but including any logical deduction of that formula from a general principle. In this way it plays a part in unexpected sciences. It has, for instance, succeeded in giving an account of the development of deposits of silica on sponge-spicules, following a suggestion of Professor Dendy, and it has made its way strongly into Physiology in regard to the various actions associated with muscles in the body. These instances could be multiplied, though I propose to say no more regarding its applications to sciences other than Physics or Chemistry, beyond giving this mere indication that such applications exist. If the special branch of Mathematics which deals with statistical questions is considered even casually, a host of problems from every branch of Science come into its scope—in fact, all problems regarding which information may be obtained from a consideration of the probabilities of occurrence of the various members of a set of alternative events. The value and continuous use of such investigations as the foundation of all actuarial work is familiar to everybody. Less familiar, perhaps, are the contributions which investigations of the same kind have made to the consideration of such matters as heredity and the variations in the generations of a species from its mean type. I wish to say at the outset that of all the modern developments of Mathematics which are now taking place, there is none more fertile and more rapidly expanding in scope than the so-called 'Theory of Probability.' In Physics we have the whole kinetic theory of gases, and the newer Quantum Theory built, in the first instance, entirely upon this as a secure foundation. In the hands of Planck, it has recently come to absorb the whole body of thought which, with the engineer, the physicist, and the physical chemist, previously clustered round the word 'Entropy.' The Entropy of a system is merely determined by the probability of the setting-up of that system as against all others which are formally possible.

The Theory of Probability is, of course, built up by the gradual solution of more and more complex problems, whose humble starting-point deals with such things as the chance of throwing four sixes in, say, ten throws with dice, or in fact any problem of a simple nature in which we require the odds against the happening of some event. I now direct your attention to a very similar type of subject—the Theory of Partitions—which, especially through the work of Major Macmahon, is also one of the most rapidly developing, and at the same time most fundamental, branches of Pure Mathematics properly so called. Let us take one of the simplest possible instances—the partitions of the small number 5, or the various ways in which we can make up 5 by the addition of smaller numbers. They are as follows:

1, 1, 1, 1, 1	1, 2, 2, 0, 0
1, 1, 1, 2, 0	1, 4, 0, 0, 0
1, 1, 3, 0, 0	5, 0, 0, 0, 0
3, 2, 0, 0, 0	

where the order is not regarded as relevant, and the total is 7. In a case like this it is easy to enumerate the partitions by writing them down and counting, but it is the work of a lifetime to find the number of partitions of, let us say, the number 20,000 by any such procedure. We need a theory of partitions, and Major Macmahon has supplied us with a very comprehensive one. But my readers may well, at this point, ask me what is the practical value, and what can ever be the practical value, of such an investigation into the number of partitions of a very large integer number. It might be compared to the practical value of a solution of a very specialised chess problem, in which we are informed that "Black mates in three." But such a comparison would be very unjust. I will give one illustration only, in which this very comprehensive enumeration has been of service. Let us turn our thoughts, by an apparently violent transition, to an opalescent liquid, consisting of water with particles of some substance in suspension, the substance being capable of forming, with equal likelihood, agglomerations of one, two, three, or any number of molecules consistent with a state of suspension in the liquid. If the maximum number under these conditions is N, the proportion which will form, say, an agglomeration of *m* particles is, on the whole, from considerations of probability, directly proportional to the number of partitions of the integer *m*, if the total number of molecules is extremely large. Thus the constitution of the suspended particles can be calculated, and so also, in consequence, as we know the number of any specified size, can such phenomena as the effect of the liquid on light passing through it, which is different for light of different colours or wave-lengths. In fact, a very large number of physical phenomena which such a liquid would shew can be actually predicted as a consequence of the formulæ belonging to this apparently quite useless branch of Mathematics. He would be a bold man, therefore, who would say that any imaginable branch of Pure Mathematics, however pure, will not at some time become the foundation of much that can only be classed, without any reservation, as Science. And this is true even of geometry. The geometry of four dimensions, worked out very completely, at least on its analytical side, very much in the spirit of a mathematical curiosity, came into its own in Science, in the hands of Minkowski, as a fine framework for the restricted Principle of Relativity. Elliptic and hyperbolic geometries, associated with the names of Lobatchewsky and Bolyai and others, were mathematical curiosities until one of them was found almost infinitely simpler than Euclidean geometry in giving an account of the ultimate laws of Physics in space. But geometry at present is not a subject which may be said to be expanding rapidly, or to contain serious fundamental problems which many mathematicians are trying to solve—attention, as for instance in the notable work of Dr Robb, being mainly confined to its ultimate postulates and foundations generally—so that I propose to leave it at this point.

Let us turn our attention to some of the matters on which our own leading school of Pure Mathematicians is at present mainly engaged. These matters lie in a region in which progress is very difficult, which bristles with fundamental problems of types which can be stated quite simply—some of them more than a hundred years old—and yet in which fundamental advances are being made. One of the outposts is what is known as Fermat's last theorem, which asserts that the equation

$$x^n + y^n = z^n$$

has no solutions whatever in which x, y, z, and n are whole numbers, provided n is greater than 2. It appears simple enough to understand the nature of the theorem—when $n = 2$ we have the right-angled triangle whose sides can be $x = 3$, $y = 4$, $z = 5$, or $x = 12$, $y = 5$, $z = 13$, and so on—but, though all tests made upon it when n is greater than 2 indicate that it is correct, no strict proof of it can be found. Fermat left many such theorems, due probably to conjecture or repeated trial, but this is now the sole survivor which resists all attacks. A prize of 100,000 marks for a proof or disproof of it was once founded at Vienna, and has accumulated since. Hundreds of attempted solutions are sent in, but all contain a fallacy. May I call the attention of everybody to this theorem, which is one towards the solution of which an extensive mathematical training is of little help—a curious characteristic of all Fermat's theorems and of many other of the more difficult problems in the theory of numbers.

There are many other such theorems—for example, one due to Euler, and more than 150 years old, to the effect that no whole numbers can be found to satisfy

$$x^4 + y^4 + z^4 = n^4.$$

Euler stated that though he could not prove this, there was no reason to doubt its truth. Nobody has proved it yet, and still there is no reason to doubt it, and much less reason even than in the time of Euler.

All such things are really centred round what is called Waring's problem, enunciated in 1770, which is essentially a problem of partitions. A number N may be portioned off into a set of whole numbers which add up to N—the ordinary partition to which I have referred above—or it may be partitioned into squares, cubes, fourth powers, and so on, of whole numbers. We may seek to write, for instance,

$$N = x_1^n + x_2^n + \ldots + x_r^n$$

—the sum of the nth powers of r integers. What is the minimum number of such integers necessary for any specified value of n, and any value of N? This is essentially Waring's problem, and 1909 was a red-letter year in its history, for in that year much was accomplished by Landau, Hilbert, and others towards its solution. Professor Hardy, of Oxford, and Mr Littlewood, of Cambridge, have done much since, but the general problem remains unsolved. After what I have already said perhaps none of my readers will raise any question as to the ultimate utility, from any point of view whatever, of such a general solution.

In connection with problems of this nature, a point of much interest, generally unappreciated by the non-mathematician, emerges. Pure Mathematics is often regarded—in certain respects truly—as a kind of sublimated arithmetic. But this view must not be carried too far, and I take this occasion to point out a serious pitfall, which is not sufficiently well known. I shall begin with a few remarks upon prime numbers. A prime number is defined as one which has no divisor smaller than itself (except, of course, the number 1, which divides everything). The first few prime numbers are

$$1, 2, 3, 5, 7, 11, 13, 17, 19, 23, \ldots$$

The study of prime numbers is one of the most interesting, and yet difficult, branches of Pure Mathematics. It illustrates what I have just said. A formula has been known for some years, which gives very closely the number of prime numbers less than a given number. It has been tested by arithmeticians, who by direct counting verified it up to given numbers of many thousands, and it was believed to continue indefinitely as a truthful formula. Mr Littlewood has shewn recently that it is not, but that it fails at a point which the arithmeticians could not hope to reach in less than a lifetime. This illustrates the fact that the success of any finite number of tests of a mathematical theorem, with actual numbers in place of symbols, can never demonstrate the truth of the theorem, which may always fail at a point beyond which, for sheer lack of time, the purely arithmetical verification cannot proceed. This is very significant for all problems in the theory of numbers, and especially of prime numbers—and, as a consequence, for any theorem suggested in a more restricted branch, such as the Theory of Partitions.

But perhaps I have already said enough regarding Pure Mathematics. It is difficult not to say more, but perhaps it may be disguised as Applied Mathematics, for the rest of what I would like to say here belongs now equally to either. Let us turn to the Einstein Theory quite definitely. It is at least the most sensational thing, in the popular sense, which has happened recently in relation to Mathematics. Here, as with the Quantum Theory later, I must not forget that Professor Richardson is to give a lecture also, and I must confine myself very strictly to the mathematical aspect of the subject. It is often said, and very truly, that a comprehension of Einstein's Theory is not in fact possible without some mathematical equipment.

But perhaps an idea of the nature of the very complex mathematics which underlies this theory may be given. It is a question of what the mathematician calls an invariant, and Einstein's general position is that the group of interrelated phenomena which constitute the universe must have something of an invariant nature about them—and it is not at all obvious that the laws we previously built up for their action are in accord with such a specification, though they may be nearly so in accord.

Now what is an invariant? Everybody really knows, though, like M. Jourdain, who talked prose without knowing it, one may use an invariant in ordinary reasoning without being aware of the fact. The Principle of the Conservation of Energy is perhaps the invariant most commonly used. If we have any system of moving bodies, which is self-contained, then, however the configuration of the system may be changing in accordance with dynamical laws, there is always a function, of the positions and motions at any time, which is not changing but preserving its value, and we call it the *total Energy*. It expresses an *intrinsic* property of the system, with a meaning which is in no way dependent on the particular manner in which we choose to measure the positions of the bodies at any instant. As all will know, such measurements depend on the use of co-ordinates, Cartesian, polar, or otherwise, and even when we have decided to use, say, Cartesian co-ordinates, we can choose them in any two perpendicular directions whatever. The expression which represents the total energy can thus take a variety of forms, but it is always numerically the same.

Now all the ordinary conceptions of geometry, for instance, have some intrinsic character (or more than one) which is often expressed by the fact that *something* involved in the conception has the same value whatever axes of co-ordinates are chosen. For instance, an ordinary ellipse, referred to any two perpendicular axes through its centre, has an equation of the form

$$ax^2 + 2hxy + by^2 = 1$$

i.e., any point on it, of co-ordinates x and y, satisfies this equation. If we choose another pair of axes, at any angle to the first, the ellipse has a new equation, say

$$\mathrm{A}x^2 + 2\mathrm{H}xy + \mathrm{B}y^2 = 1$$

where the large letters exhibit no obvious relation to the small ones. But we can prove that, always,

$$\mathrm{A} + \mathrm{B} = a + b$$
$$\mathrm{AB} - \mathrm{H}^2 = ab - h^2$$

So that $(a + b)$ and $(ab - h^2)$ are always the same for the same ellipse, and we call them the *invariants* of the ellipse, just as the total energy is an invariant for a set of bodies in motion.

The Principle of Relativity is ultimately, on its mathematical side, founded on the supposition that the whole universe, considered as one system, likewise has its appropriate invariants, which of course cannot be completely unrestricted in nature, but are subject to many obvious assumptions suggested by known phenomena, which rule out the majority of assumptions which would otherwise be somewhat bewildering. In fact, in their choice, the procedure was to take the formulæ which constituted a shorthand summary of existing knowledge, and to postulate invariants which would not violate these and yet would leave a loophole, by greater generality, to bring in gravitational phenomena. The result was that Einstein found only one possibility—with, as I said, the pure analysis very largely constructed long ago and awaiting application. This possibility, when developed, constituted his theory. I hope that at least the nature of the mathematical side of the theory has been made somewhat clearer by these brief remarks.

If we turn now to another branch of my subject in which significant progress is at present being made rapidly, we cannot perhaps do better than devote a little attention to what are called 'asymptotic expansions.' They are the most frequent type of connecting-link between Pure Mathematics and subjects of mathematical physics such as hydrodynamics, special problems in distribution or motion of electricity, the diffraction of light round obstacles, and acoustic vibrations—to mention only a few of the main subjects concerned, though perhaps those which stand out most prominently in this regard. Further progress in special directions in these subjects is often greatly retarded by the lack of some asymptotic expansion. In a problem of diffraction or bending of light round some obstacle of definite shape, for example, in order to proceed from the quite general laws governing such phenomena to an actual numerical determination of the amount of light received at some point behind the obstacle—a matter which can be quite vital in such a simple case as the interpretation of phenomena occurring at the focus of a lens forming part of an optical instrument—it is often found necessary to use mathematical functions of some complexity, which are not, in their usual form, at all adapted for numerical calculation when some quantity in them becomes large, and alternative forms are needed. These alternative forms are often of a peculiar type, and known as asymptotic expansions. We may say that such an expansion is an expression which, to a certain degree of accuracy, gives the same numerical values as the function which actually occurs in the problem. It is often very difficult to find, though many valuable ones have been found recently, and are still being found, by quite new methods of considerable generality. A period of rapid development in mathematical physics will necessarily follow.

I must perforce, from the mere limitation of space, disregard several lines of present-day progress in Mathematics, and many interesting problems awaiting solution. A majority of these are naturally, in their present form, not very capable of anything like a popular exposition, and I do not pretend to give any general review of the subject as a whole, for its ramifications are very vast, and involve many totally different types of thought—as different in kind at least as, for instance, pure geometry and the analysis of the properties of whole numbers—connected only by the special form of logic, whose nature has been so much discussed, which runs through the whole. Otherwise I should have wished for space to mention, with a certain amount of detail, such matters as the Theory of Transfinite Numbers, with which the names of Whitehead and Russell, after Cantor, are mainly associated—capable of loose description as the Theory of the Infinite—the general advance in decisiveness of our conceptions of such fairly simple processes as integration, due to Lebesgue, and recent rapid progress in such matters as the theory of the tides, of earthquake phenomena, and so forth. Perhaps this mere list may serve to shew the very wide range of work which comes within the province of Mathematics, and the arbitrary character which must belong to any selection which can be made for the purposes of a lecture. I must crave indulgence from my readers for any omission of a subject which would have been found more interesting than those I have selected as types.

But there is one group of subjects, with which I shall conclude, for which a general title is not readily found. They belong as much to Physics as to Mathematics, being essentially what are called 'border-line' subjects, and include the general laws of dynamics in the microscopic field—as distinct from the statistical aggregates of dynamical laws, which are the laws we postulate macroscopically, that is to say, for matter in bulk—the ultimate laws of radiation from the smallest possible system, an atom or an electron, the principles of the Quantum Theory, and the structure of the atom. Perhaps, in view of the nature of the progress now being made rapidly in this domain, we might group all these subjects together as Quantum Theory, for it is the introduction of this theory by Planck which has alone made possible any real advance from the point which the so-called 'older' electrodynamics had reached several years ago. We may describe this older dynamics quite simply, in the historical order of its development. Beginning with Newton's simple laws of motion for 'particles'—the important one being the formula

$$P = mf$$

familiar to all elementary students—the corresponding laws for a rigid body or system of rigid bodies, or for a fluid, which are in their nature a kind of integration or summation of the results of applying this law to all the individual particles composing the body or fluid, have been built up, and, naturally, ultimately take very general forms such as Lagrange's equations, which can be used for any possible way of specifying the positions of parts of the system at any moment, or alternatively, in the case of fluids, the Helmholtz equations. But in spite of their appearance in such a general shape, they contain no more than Newton's laws. Hamilton reduced them to one fundamental principle applicable to any system, from which everything in dynamics can be deduced—the *Law of Least Action.* In the hands of Larmor this was made a foundation for the laws of electrodynamics deduced by Maxwell, and for the mathematical side of our conception of the electron and of its radiation of energy when in a state of accelerated motion. Electromagnetic theory—and thence, light being an electromagnetic phenomenon, the theory of light, and in fact the whole of Physics—thus became virtually a branch of mechanics, and so it always will be so long as we are dealing with what I have called macroscopic phenomena, or the phenomena relating to matter in bulk.

But we have no obvious justification for the assumption that the laws we perceive in matter in bulk are in any way even in close resemblance to those which describe the behaviour of the ultimate indivisible unit of matter, and still more those which are applicable to the separate component parts actually inside that indivisible unit. This unit is, of course, the atom, and the component parts are the positive and negative electrons. An electron is not matter, but special groups forming atoms constitute matter. Now there are many wide branches of Physics in which the existing phenomena shewn by matter are not, as in ordinary mechanics, shewn by it as a bulk, but are shewn by the individual units acting quite independently of one another. Of such is the radiation given out by a body, whether under the influence of heat or any other agency. And it is precisely in such regions that the classical electrodynamics is not only unable to offer explanations of phenomena, but can be shewn to be incompatible with such phenomena if we assume it valid in the microscopic field presented by an atom and its contained electrons. We can, however, shew that something very like, at least, the Quantum Theory is needed for such explanations. According to this theory, radiation is not being emitted continuously in appropriate circumstances from an atom, but in definite jumps of prescribed amounts, these amounts involving a new universal constant of nature, denoted usually by h, to which all are proportional. Many other phenomena of different type involve h, for example, the kinetic energy of an electron which, by striking an atom, causes it to emit its characteristic X-ray, or the energy with which, when an electron is driven from an excited atom, that electron departs. Now h itself is not an energy, but an *action*, and we have a universal constant of action. Perhaps I should explain what *action* means. It is the product of an energy and a time, or we can think of it as an angular momentum of rotation, so that it is not a concept of which the elementary student has no cognisance. One very good method of visualising the significance of h is to suppose that the electrons in every atom can only rotate with speeds corresponding to definite values of their angular momentum determined by h. Then, of course, the expression for the energy or any other property of the atom, which can be determined from a knowledge of these angular momenta and the distribution and number of electrons, will also involve h. In fact, h is so fundamentally the basis of the specification of the atom as a structure that it must appear of necessity in the quantitative expression of any phenomenon in which the atom plays the part of a unit. In these considerations taken alone there is clearly no difficulty. It is when we compare such a specification, known from its success in interpreting spectra, and for a variety of other reasons, to be now practically inevitable, with ordinary dynamics that the difficulties begin. I can illustrate this with the simplest case of all—a hydrogen atom, whose form is now effectively established from a long list of different lines of evidence.

This consists of an electron, with a charge e, rotating in a circle—or other orbit, but consideration of the circular orbit is sufficient—of radius r, round a relatively heavy positive charge, also of magnitude e, at the centre of the circle. The attraction between them is $\frac{e^2}{r^2}$, which supplies the force necessary to keep the electron from flying off at a tangent. If the mass of the electron is m, and its angular velocity is ω,

$$mr\omega^2 = \frac{e^2}{r^2}$$

which is, after all, only Kepler's law for the planetary systems, $\omega^2 \propto \frac{1}{r^3}$ or the squares of the periodic times of the planets are proportional to the cubes of their distances from the Sun. Now these Kepler laws are known to be true in the case of atoms, so that ordinary dynamics is not really superseded in the interior of an atom. In particular, the formula above is certainly true in a hydrogen atom.

In fact, the laws of ordinary dynamics remain valid up to the point at which the emission of radiation by the atom is considered. This emission is a transference of energy from matter —the atom—to the æther, and not directly from matter to matter in the sense we meet, for instance, in ordinary macroscopic mechanics in the impact of two billiard balls or the driving of a nail into a piece of wood. It is this transference of energy between matter and æther which introduces something new, and the Quantum Theory in its successful form states that among all the steady states of an atom which ordinary dynamics allows, only a certain number are in fact possible, during which the atom emits no energy to the æther, and these are the states which it takes up. If energy is emitted, it must be of an amount which enables the atom, after the operation of emission, to fall into one of the other steady states. Thus the emission can only occur in what we may call bundles of energy dependent, like everything else about the atom, on the universal constant *h*.

The quantum view is thus essentially something superposed on the results of ordinary or classical dynamics, rather than something in contradiction with it. It is, in fact, a selective or *restrictive* principle applicable to atoms, which is all-important in any phenomenon involving one atom only, but in wide ranges of phenomena, as in ordinary mechanics, where there are multitudes of such atoms which we treat as a statistical whole, its effects disappear in the average. Let us take an illustration of something similar—the similarity may be far-fetched, but I want to indicate the possibility of an almost infinite number of effects cancelling out. An ordinary electrically-charged conductor is known to produce no force at an internal point. Yet every bit of charge on it produces a force at this point, in some direction, though if we consider the whole charge at once, all these separate forces completely cancel, whatever be the shape of the conductor. Though this problem has no relation whatever to the one we are discussing, it serves to shew that effects shewn by something—whether matter or electricity—in bulk may completely fail even to give us an indication of what may be the fundamental law of the indivisible unit. The ultimate laws of dynamics of an atom can thus only be found by the study of the particular phenomena shewn by that atom behaving as an independent unit.

Before the Quantum Theory arose, it was well understood that some restrictive principle was necessary in the ultimate unit of matter, and the reason for its necessity is not at all difficult to understand. Let us return to the hydrogen atom. Dynamics tells us that the angular velocity and radius in that atom are connected by Kepler's law, but it is incapable of telling us more. With two unknown quantities and only one equation, we cannot determine both—a commonplace of elementary algebra. Thus dynamics allows the atom to have *any* radius it chooses provided that the electron rotates with the proper corresponding speed. In a planetary system like our own round the Sun, the particular radii selected are determined by the motions which existed when the planets were formed as such—or what the mathematician calls initial conditions. But such motions of matter on the large scale involve no resultant transfer of energy to the æther, in the sense that our reception of light from the Sun does. Classical dynamics of the statistical aggregate of immense numbers of atoms is therefore competent to deal with such problems.

But these 'initial conditions' are not operative in atoms. They are of course a restrictive principle in themselves, for they serve to pick out the orbits our planets (and the Earth) are adopting, as a choice from the infinite number which the laws of motion would allow. In an atom, they must be replaced by something quite different, because all hydrogen atoms, wherever found and whatever their earlier history, behave, spectroscopically for instance, in the same way. This is equivalent to saying that some other universal equation connects angular velocity and radius in such cases—if we adopt the angular momentum principle, as we find it necessary to do, we obtain the result that $r^2\omega$ has a constant value. The Quantum Theory replaces initial conditions by this form of condition.

But perhaps I have already said enough on this matter. My aim has not been, as that of a physicist might be, to give any account of such matters as the Theory of Relativity or the Quantum Theory, but only to point out the essential nature of the mathematical questions involved in the roots of the theories, for experience shews that the essential difficulties found in their comprehension are due to a lack of realisation of this essential nature, which is not always difficult even for the non-mathematician to understand. To some extent, in these two problems, I have trespassed on the domain of physics. My excuse must be that by far the most fundamental developments of Applied Mathematics have recently been, and must continue to be for some time, in these two directions. I can only conclude by expressing the hope that I have not given a disproportionate amount of attention either to Pure or to Applied Mathematics, in this endeavour to select what I regard as the most typical lines, in each category, of the recent or probable advances in my subject in the near future.

J. W. Nicholson

II

ASTRONOMY

By J. B. DALE M.A.

ASSISTANT PROFESSOR OF MATHEMATICS IN KING'S COLLEGE

II

ASTRONOMY

IN the present course of lectures, Mathematics, the science of calculation, an essential element in the organisation of all exact knowledge, appropriately occupies the first place. But it is no less appropriate that the second place should be assigned to Astronomy, the earliest product of man's recognition of the uniformities which underlie natural phenomena. It is, moreover, the physical science of widest domain, for it embraces an almost limitless range of time and space.

The data of Astronomy are observations of the positions which the heavenly bodies occupy at particular times, of their apparent forms, dimensions, and of the intensity and peculiar qualities of the light which they emit. Astronomy is therefore primarily an observational science, and a great part of the work of the practical astronomer consists in devising instruments and methods which will enable him to make his observations as complete and accurate as possible. But all that these observations furnish is a series of isolated facts. The relationships in which they stand one to another have to be discovered by mathematical analysis or by reasoning based upon a knowledge of the laws which govern the transformations and varying states of matter. Hence the problems of Astronomy are to a very great extent problems of mathematics, physics, or chemistry. The unravelling of the apparently complex motions of the heavenly bodies, and the determination of the laws which govern their motions, are fields of enquiry upon whose cultivation the mathematician has, during many centuries, expended all the resources of his art, and in which he has reaped much signal success. It is no small achievement to overleap the limitations which confinement to this puny globe and the brevity of human life impose, to measure distances which light itself takes many years to traverse, to weigh the stars against the sun, to determine the configurations of planetary and stellar systems at distant past or future epochs of time.

And in passing it may be noted that if astronomical knowledge has thus been advanced by the aid of mathematics, on the other hand mathematics as a pure science has also benefited by the stimulus and direction to research which the endeavours to solve the problems suggested by Astronomy have supplied.

Physics and chemistry, the sciences which are concerned with the properties and constitution of matter, hardly came into contact with Astronomy till the nineteenth century was well advanced, but out of the close union which has now been established has arisen what is almost a distinct branch of Astronomy bearing the name of Astrophysics.

It is, however, unnecessary to enlarge upon the general relations existing between these various sciences, for the special instances with which we shall be concerned will illustrate them in a sufficient manner.

Up to the beginning of the nineteenth century the interest of astronomers was centred chiefly upon our nearest neighbours in space, that limited group of bodies comprising the sun, moon, planets, and comets which form the Solar System. About the stars very little was known. It was clear that their distances from our system were very great even in comparison with the distance of the farthest known planet. Apart from differences in brightness and colour they exhibited no striking individual characteristics. They were mere points of light, and as they appeared to maintain their distances from one another unchanged, their chief use was to serve as fixed marks of reference to which the motions of the members of the Solar System might be referred. In addition to the stars, a limited number of dim cloudy masses, to which the name of nebulæ was given, were found to exist in certain parts of the heavens, and their fixity with regard to the stars indicated that their distances also were enormous.

The foundations of Sidereal Astronomy, as we now know it, were laid by the Herschels, who between them explored the whole heavens. Their discovery of binary stars revolving round one another in elliptic orbits demonstrated that the sway of the law of gravitation extended throughout the material universe; the soundings of the elder Herschel gave the first reliable indications of the extent and shape of that universe, and the discovery that the nebulæ were to be numbered not by hundreds but by thousands showed that they were not exceptional but normal forms which masses of matter might assume.

In considering a group of objects we may fix our attention upon the characteristics which they possess in common, or upon those which distinguish one from the other and mark them out as individuals. When the number of objects in the group is small the study of the individual peculiarities may be of more importance than that of the qualities common to all. But in dealing with a large population such as that formed by the stars the common characteristics are those which demand chief attention.

As a rule the characteristics are such that they are possessed by different individuals in different degrees, and one of the first steps in a scientific enquiry is to determine what proportion of the total population possesses a certain amount of any specified quality.

Such enquiries belong to the domain of statistics, and differ in no essential respect so far as method is concerned from enquiries of a similar nature with which we are familiar. In fact, they are questions which can only be answered by taking a census. But the difficulties in the way of taking a census of the sky are considerable.

So long as the only method which could be employed was direct visual observation of each separate star, the accumulation of a considerable body of information regarding the characteristics of the stars as a whole was a slow and tedious process; and it was not till the photographic plate was substituted for the eye that sufficient data began to be obtained to render statistical discussion of the majority of the characteristics really possible. Not only is visual observation slow and tedious, but when measurements of distance or angle are involved we find most perplexing disagreements between the results obtained by different observers, or even by the same observer at different times. Moreover, when it is a question of making a drawing of an extended object such as a nebula, the multiplicity and delicacy of detail baffle accurate delineation.

On the other hand the photographic plate registers at one and the same time many distinct objects, and the records thus obtained are permanent and can be re-examined whenever it may be desired. In addition it possesses this great superiority over the eye, that by prolonging the time of exposure, impressions of objects can be obtained whose light is too faint to affect the most sensitive vision.

One has only to compare recent photographs of nebulæ, showing a wealth of detail, with the crude drawings of the same objects made after many hours of patient examination, to realise how enormous is the advance in the knowledge of those objects which the employment of photography has rendered possible.

Moreover, in recent years telescopes greatly exceeding all former ones in light-gathering capacity and the perfection of their optical parts have been constructed—they all belong to the other side of the Atlantic, one notes with some regret—and with these vast regions of space which hitherto were utterly beyond reach are now being explored. The result of these and other improvements in the instruments of research is that data are being accumulated at a rate altogether impossible in former years, and astronomers are now almost overwhelmed with the abundant material pouring in upon them.

The application of photography on a large scale began in 1887, when a scheme was set afoot for constructing a chart and catalogue of all the stars down to the twelfth magnitude. The co-operation of the principal observatories of the world was secured, and it was hoped that the task would be completed within a few years. Unforeseen difficulties arose, some observatories withdrew, others were late in getting to work, and although thirty-three years have elapsed, this census is not yet finished.

Meanwhile another census, all-British in design and execution, has been successfully completed. John Franklin-Adams, a London merchant, one of those amateurs who have done so much in this country for the promotion of Astronomy, after retiring from business conceived the plan of photographing the whole heavens. For this purpose he obtained from Messrs Cooke and Sons a lens designed by Mr Dennis Taylor. The photographs of the southern skies were taken at the Cape in 1903–4, and those of the northern skies at Mervel Hill, near Godalming, between 1905 and 1909. The southern series was repeated by his assistants in 1910–11. Failing health compelled him to abandon his intention of examining the plates himself, and he therefore presented them to the Royal Observatory at Greenwich, making provision for the statistical discussion to be undertaken by members of the staff there.

The international census is primarily concerned with the accurate determination of the places of the stars. The scale of the Franklin-Adams charts is too small to lend itself to this purpose. On the other hand, while the international plates do not show stars fainter than the twelfth magnitude, the Franklin-Adams limit is the seventeenth magnitude.

It may here be desirable to explain the precise significance of the term 'magnitude' as used in Astronomy. From very ancient times the stars have been arranged in order of brightness, the brightest being said to be of the first magnitude, while the faintest visible to the naked eye were of the sixth. Stars of intermediate brightness were assigned to one or other of the four remaining magnitudes. The classification was a rough one, but when the development of astronomical instruments enabled differences in brightness to be observed with a greater degree of precision, an exact definition of magnitude became necessary. It is therefore now universally agreed that if the amount of light emitted by a star A is 100 times the amount emitted by a star B, then the magnitude of B is 5 units greater than the magnitude of A. It is to be noted that, contrary to the ordinary acceptation of the term, increasing magnitude signifies, not increasing brightness, but increasing dimness. With regard to intermediate differences of magnitude, it may be remarked that one unit corresponds to a light ratio of about $2\frac{1}{2}$, so that, for example, a standard star of magnitude 2 is about $2\frac{1}{2}$ times as bright as a standard star of magnitude 3.

Returning now to the Franklin-Adams plates, it was, of course, clearly impossible to count all the stars which they showed, and therefore typical areas were selected and the stars of different magnitudes counted. The result of the counts thus made by Chapman and Melotte are shown in the accompanying table :

THE NUMBERS OF STARS OF DIFFERENT MAGNITUDES

Magnitude	Number
1	11
1–2	27
2–3	73
3–4	189
4–5	650
5–6	2,200
6–7	6,600
7–8	22,550
8–9	65,000
9–10	174,000
10–11	426,000
11–12	961,000
12–13	2,020,000
13–14	3,960,000
14–15	7,820,000
15–16	14,040,000
16–17	25,400,000

Although the faintest stars shown on the plates are of the seventeenth magnitude, there must be many still fainter, but a mathematical discussion of the numbers actually counted leads to the conclusion that the total number is finite. It would appear that the stars of the twenty-fifth magnitude are more numerous than those of any lower or higher magnitude, and that the total number of all magnitudes is of the order of 3000 millions. It may be remembered that Lord Kelvin, reasoning from dynamical considerations, estimated the number to be about 1000 millions. These numbers are large, but so far as the mere figures are concerned they may be compared with our national debt of £8,000,000,000 or with the population of the globe, which is estimated to be about 2000 millions.

We may next enquire what information can be derived from these photographs regarding the distribution of the stars in space. Distribution depends upon distance and direction, and regarding the former of these elements the photographs can yield no direct information. It is obvious that the dimmer stars must on the whole be more remote than the brighter ones, but we now know that estimates of distance based on apparent brightness alone are likely to be much in error, because they leave out of account the differences in absolute brightness. As a matter of fact some of the faintest stars are comparatively near us, while many of the brightest are at a great distance. Disregarding, then, the element of distance, we can obtain directly from the photographs information regarding the distribution of the stars in direction. The main point which emerges in this connection is that as we approach the plane of the Milky Way the number of stars which can be counted in an area of given size increases very rapidly, indicating that the stars separately distinguishable as such and the massed legions of the Milky Way form but one system. Until more is known regarding relative distances it is impossible to form an exact idea of the geometrical form of the universe. The working hypothesis which is generally accepted at the present time is that the shape is something like a bun or lens, our sun being near the centre, and that encircling it, perhaps in spiral fashion, are the star-clouds which constitute the Milky Way.

Another group of problems of a geometrical character are those relating to the motions of the stars.

The term 'fixed' as applied to the stars is a misnomer. Its partial justification arises from the fact that the apparent motions are so slow that the changes in position which they produce can only be detected by careful measurements made with accurate instruments at times separated by a considerable interval. Halley was the first to detect any motion, and this he did by comparing the observations of his own day with those made by the astronomers of Alexandria sixteen hundred years earlier. Even with modern instruments and methods the detection of motion is difficult, and the number of stars which have been found to be in motion relative to the sidereal system as a whole forms a very small percentage of the total. Still, there is no doubt that all are moving, and it is only distance and faintness which prevent observation of the resultant changes. Dynamics shows that it is impossible for bodies attracting one another in accordance with the law of gravitation to be in a state of relative rest, unless they are in contact with one another. Either their distances apart or the directions of the lines joining them must be continually altering, and changes in distance and direction may take place simultaneously. Space contains no fixed landmarks. Indeed, if there were we have no means of recognising them as such ; consequently absolute position and absolute motion are meaningless terms. The question whether dynamics enables us to fix absolute directions in space is an interesting one, but we have no time to consider it now.

The practical problem we have to deal with is the determination of the motions of the stars relative to ourselves, or, more strictly speaking, to the sun, for the change of standpoint is easily allowed for.

The motion of a star can be resolved into two components, one, in the line of sight, producing change in distance, and the other, at right angles to the line of sight, giving rise to change in direction. The magnitudes of these two components are found in entirely different ways. The former or radial velocity is found by observing with the spectroscope the amount by which the lines in the spectrum of the star are displaced from their normal positions. If the lines are displaced to the blue end of the spectrum the star is approaching us; if the displacements are towards the red end, the star is receding. These observations yield an actual speed of approach or recession in miles per minute. The thwartwise motion is observed as a change in position upon the celestial sphere, and is called proper motion. Its amount when multiplied by the distance of the star gives the speed in miles per second at right angles to the line of sight. Unfortunately the number of stars whose distances are known with any great degree of accuracy is very small, and we have for the present to be content with making what deductions we can from the proper motions alone.

It is natural to assume that stars possessing large proper motions are on the whole nearer to us than those whose proper motions are small. Moreover, as we have already seen, apparent magnitude depends upon distance. The determination of the relations between these three quantities is one of the most interesting statistical problems upon which astronomers are working at the present day.

At first sight the proper motions of the stars in any small region of the sky would appear to be entirely haphazard and irregular both in direction and in magnitude. But if average values be taken

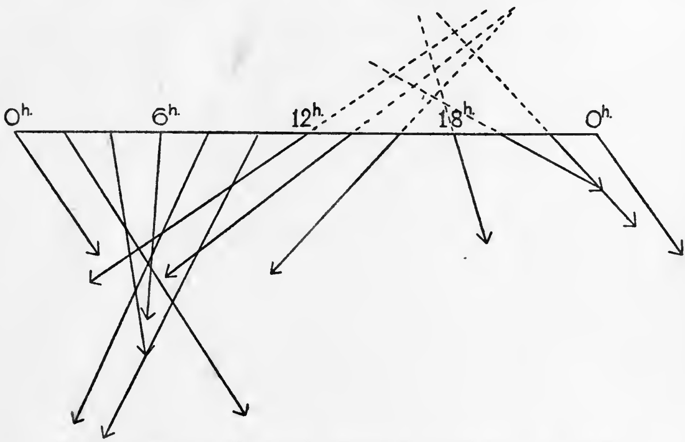

Fig. 1.—Proper Motions of Stars in Zone 24°–32° N.

a regularity discloses itself. Figure 1 shows the average proper motions of the stars lying in a narrow zone of the sky whose central line runs 28° north of the Celestial Equator. The stars in every 2 hours of right ascension, which corresponds to 30° of longitude on the earth, have been grouped together. The diagram shows that the stars seem to be flying towards a point south of the zone, situated about 6^h, and away from a point north of the zone about 18^h. If these lines were drawn as great circles on a celestial globe, they would be found to intersect in points which are in the neighbourhood of 6^h of right ascension, 40° South, and 18^h, 40° North.

Now whatever part of the sky be examined, very similar results are obtained, and so we may say that the stellar system as a whole appears to be moving away from a point which is not very far from the bright star Vega in the constellation Lyra. What is the cause of this apparent motion? The obvious answer is that it is caused by our own sun with its attendant planets moving through space towards the point thus indicated. This discovery is not a recent one. As long ago as 1783 Sir W. Herschel arrived at a similar conclusion from an examination of the motions of seven stars only, and the point he indicated is not very far from those found by more recent investigators.

Though modern determinations of the position of the solar apex, as this point is named, are in fairly good agreement, there remain to be explained considerable differences between the positions deduced from the examination of different classes of stars. The diagram (Fig. 2) shows some of the stars in the constellations Lyra and Hercules together with the positions of the solar apex as found in different ways.

It is not surprising that these discrepancies should exist. We cannot expect the motion of the sun relative to one class of stars, say those brighter than the seventh magnitude, to be the same as that relative to fainter ones. The difference between these two motions is an indication that the two classes themselves are in motion relative to one another.

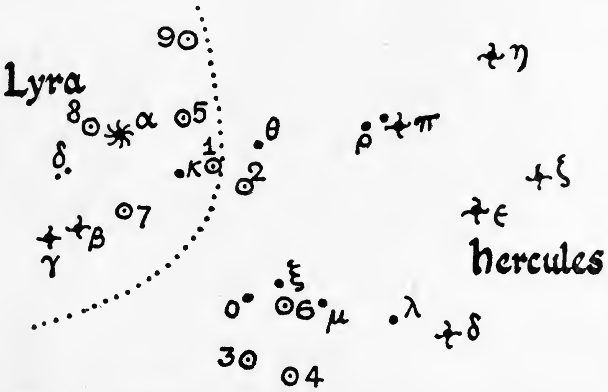

Fig. 2.—DETERMINATIONS OF POSITION OF SOLAR APEX
(1) Eddington. (2) Boss from proper motions. (3) Campbell from radial velocities. Dyson and Thackeray's determinations: (4) from B and A stars; (5) from F, G, K stars; (6), (7), (8), (9) from 5th, 6th, 7th, 8th magnitude stars.

Further light has been thrown upon the problem by the researches of Kapteyn, published in 1904. His conclusions have been confirmed in their main features, and further details have been added by the work of British astronomers—Eddington and Dyson in this country, Hough and Halm at the Cape.

Let me ask you for a moment to suppose that you take your stand at some central spot where traffic converges from many directions, such as the fountain in Piccadilly Circus. Standing there, you count the number of persons passing you in different directions. You will probably find that there are as many going north as south, or east or west. There is no apparent preferential direction of motion when you are stationary. If, however, you walk westward towards Piccadilly you will meet more persons going eastward, and fewer will pass you going westward; and the more rapidly you walk the greater will be the difference in the numbers of those two classes of persons. There will also, relative to yourself, be changes in the direction of motion of those persons going north and south.

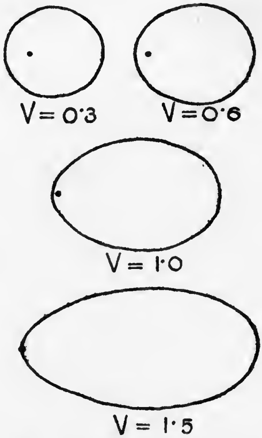

Fig. 3.—Theoretical Curves showing the Number of Stars moving in Different Directions

V is the ratio of the velocity of the base point to the mean velocity of the stars.

Now apply this to the stars. If in any region they were moving in a perfectly haphazard manner, and from a point lines were drawn whose lengths were proportional to the number moving in the direction indicated by the line, we should expect all these lengths to be equal, so that their extremities would lie on a circle. If, however, the motions are referred to a point itself in motion, for example, one of the stars themselves, we find that the diagram loses its symmetry (Fig. 3). It becomes elongated in a direction opposite to that in which the base point of reference is moving, and the elongation increases as the speed of the base point is increased. Mathematical theory enables the shape of these curves to be calculated on the assumption that the stars are moving in a haphazard manner and that there is no preferential direction of motion.

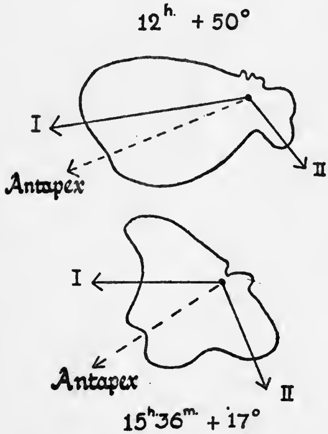

Fig. 4.—Curves showing the Number of Stars observed to be moving in Different Directions

But when diagrams are constructed representing the actual number of stars which are observed to be moving in different directions, the figures which are obtained bear little resemblance to those just described (Fig. 4).

The forms of the curves change as we pass from one region of the sky to an adjoining one, but all have this feature in common, that instead of being elongated in one direction only, they show elongations in two and sometimes three directions. It is clear then that our simple *a priori* hypothesis regarding the nature of the stellar motions is not in accordance with the facts, and must be replaced by some other. Now it has been shown by Eddington and others that the main features of the curves deduced from observation can be reproduced by combining in a suitable manner two of the curves obtained on the simpler hypothesis (Fig. 5). The meaning of this is that the stars do not form a single system, but are to be regarded rather as forming two distinct systems. The motions in one or other of the systems are to be regarded as quite irregular, but the systems as a whole are in motion relative to one another. In relation to one of them our sun is moving at the rate of twenty miles a second, and with respect to the other at a little over eleven miles a second. It is found that the greater number of the stars which exhibit any motion belong to one or other of the two groups, or drifts, as they are now usually designated, but the numbers in the drifts are unequal, being in the ratio of 3 to 2. Now although the greater number of the stars belong to the drifts, there are others which stand apart and form distinctive systems by themselves. The most important of these is that formed by the B stars, which are characteristic of the constellation of Orion, though not confined to it. These are the most massive and luminous stars in the heavens, and of them I shall speak later. In addition to these groups, several clusters of stars have been

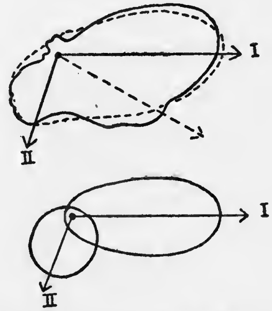

Fig. 5.—Diagram showing the Combination of Two Theoretical Curves to Represent an Observed Curve

recognised, which are characterised by their members moving through space with equal velocities on parallel lines. The Pleiades form such a cluster, the motions of at least fifty of the stars being equal and parallel. Another similar

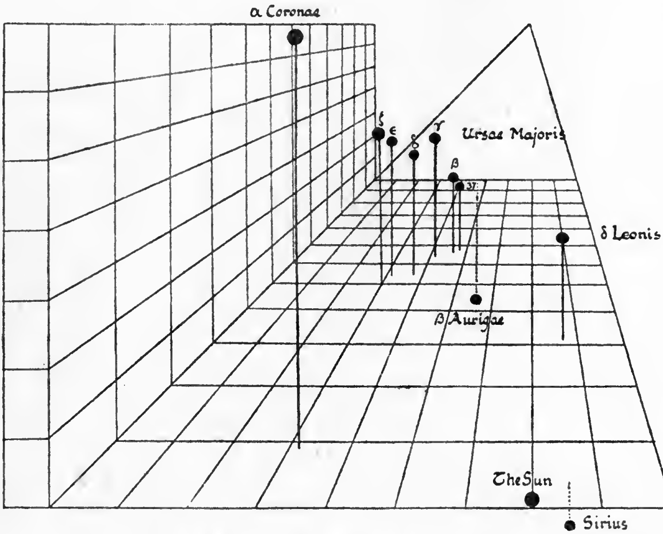

Fig. 6.—Ursa Major Cluster

group is that called the Ursa Major cluster, because to it belong five of the stars in the Plough. It has been found, however, that a number of stars in other parts of the sky, such as Sirius, are also members. The diagram (Fig. 6) gives some idea of the relative positions of the stars composing this cluster.

The measures of the distances of the stars when expressed in ordinary units are too great to convey any meaning. We therefore employ other units more suitable for measuring the heavens. Consider for a moment the distance of α Centauri, the star nearest to us. If we construct a model of the universe on the scale of 1 foot to 20 million miles, our sun will be represented by a sphere less than half an inch in diameter, and the earth will be represented by a speck of dust one two-hundredth of an inch in diameter revolving round the sun at a distance of a little over $4\frac{1}{2}$ feet. The base line which astronomers have to use for measuring the distances of the stars is the diameter of the earth's orbit, and in our model this is $9\frac{1}{4}$ feet long. α Centauri, which is a double star, will then be represented by two spheres equal in size to the sphere representing the sun, placed at Newcastle, 240 miles away. This model enables us to form some idea of the vastness of space, and of the extreme sparseness of the matter which it contains. The actual distance of α Centauri is of the order of ten millions of millions of miles, and therefore we find it more convenient to take as our unit for measuring celestial distances the distance travelled by light, whose speed is 186,600 miles per second, in one year. This length is called a light-year. Now the above figure represents a pathway leading to a point on the distant horizon, the nearer part being paved with square blocks whose sides are 10 light-years in length, and the wall is constructed of similar blocks. In the foreground is the sun, the various stars are represented by balls placed upon rods which serve to define their positions. Two of the stars are suspended by strings below the pavement. The nearest star is Sirius, whose distance is over 8 light-years. The distant group is that

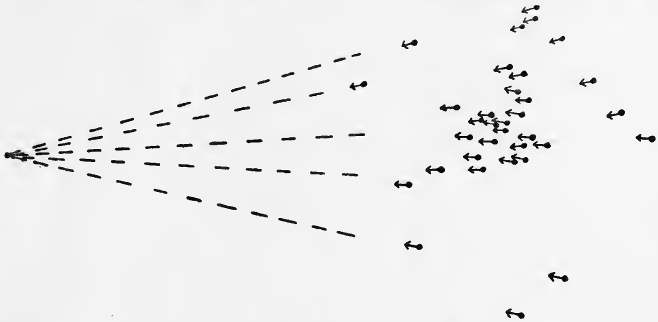

Fig. 7.—TAURUS CLUSTER
The small arrows indicate the direction in which the stars to which they are attached are moving.

formed by the stars in the Plough. All these stars are moving away from the sun along this corridor of time at the rate of 11½ miles per second, and 160,000 years will elapse before they have moved forward a distance equal to the side of one of the blocks.

The diagram makes it clear that as an effect of perspective, stars which are really moving on parallel lines will appear to converge to a point. This convergence of paths is very marked in the case of a cluster of stars in Taurus, whose motions were investigated by Boss (Fig. 7).

The researches which I have just outlined may serve to give some idea of the growth and extent of our knowledge regarding the motions of the nearer stars—that is to say, those within 500 light-years of the sun. Much remains to be discovered, and the remoter parts of the system are still unexplored.

Astronomy, as a system of organised knowledge regarding the stars, would be very incomplete if it confined itself to the investigation of facts concerning their position and motion. A complete survey must take account of their constitution and physical condition. In pursuing investigations into these matters the great instrument of research is the spectroscope. It takes the light of the star, and broadens it out into a coloured band, so that each component element in the light can be separately examined. When the light is thus split up, it is found that the spectra belong to a few well-defined types corresponding to a rough colour classification of the stars as Bluish-white, White, Yellow, and Red. The typical spectra are denoted by the letters B, A, F, G, K, and M.

The bluish-white stars show a B type of spectrum. They are the brightest, hottest, and most massive of all the stars. Their spectra are characterised by the great strength of the hydrogen lines and by the presence of helium lines.

Sirius is a typical white star, its spectrum being of the A type. In spectra of this class the hydrogen lines attain their greatest intensity. The F spectrum is characteristic of the yellowish-white stars. Hydrogen is less prominent, and metallic lines make their appearance.

Our sun is a typical yellow star with a G spectrum. Many metallic lines are now shown,

STELLAR TYPES

Spectrum	Type	Characteristic lines	Colour	Temperature
B	Orion stars	Helium Hydrogen	Bluish-white	10,000°
A	Sirius	Hydrogen	White	8,000°
F	*α* Argûs	Calcium	Yellow-white	7,000°
G	Sun	Metallic	Yellow	6,000°
K	Arcturus	Metallic	Orange	4,250°
M	Betelgeuse	Fluted	Red	2,950°

and these are still more prominent in the orange stars, whose spectrum is K. Lastly we come to the red stars typified by Betelgeuse. The spectrum shows numerous bands, and closely resembles that of sun-spots. To this class of stars belong nearly all the long-period variables. The order in which the spectra have been arranged is by no means fortuitous; it corresponds to the order of decreasing temperature. The last column of the table gives the effective temperatures of the different types of stars—that is, the temperature of the outside layers which radiate the heat and light which we receive. Of course the temperature in the interior must be very much greater. At the centre it is probably of the order of one million degrees.

In 1914 H. N. Russell constructed a diagram in which he plotted the stars according to their absolute magnitude and spectral type. The absolute magnitudes are defined to be the magnitudes which they would appear to have were they all at a distance of 32½ light-years. I have constructed a similar diagram, using other data which have been published more recently (Fig. 8). Altogether some six hundred stars are represented. It is seen at once that the stars from A to M fall into two very distinct groups. In the first place we have a group running horizontally, the absolute magnitudes lying between -1 and $+2$, and then there is a second group running diagonally downwards. The M stars are thus separated into two sets entirely distinct from one another. The K stars also form two sets, though the separation is not so definite, whereas the G, F, and A stars are intermingled. The B stars form a group by themselves, and their absolute magnitudes show that their luminosity is very great. The position which our sun occupies in this diagram is marked by a large star.

Now if we ask how it happens that there should be two groups of M stars differing so greatly in brightness, the only possible answer is that the

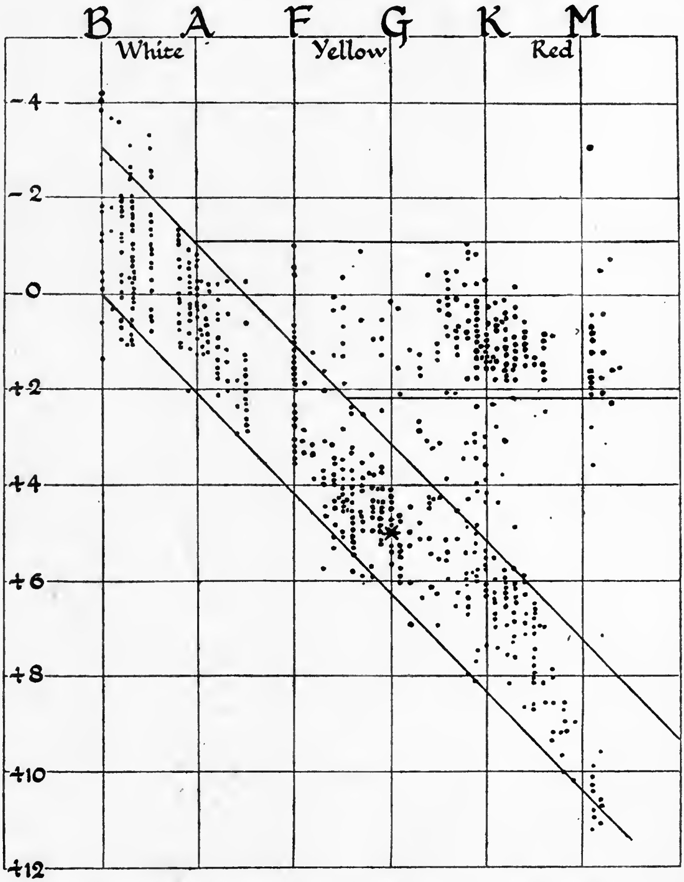

Fig. 8.—Absolute Magnitude and Spectral Type

stars in the brighter group are very much larger than those in the other. The spectrum and surface temperature being the same, the amount of light emitted per square mile of surface must also be equal. Since the difference in magnitude between the two groups is on the average 9 units, it can be shown that the brighter stars must have surface areas 4,000 times greater than the fainter ones, or that their diameters are in the ratio of 64 to 1.

It has been found by examination of binary stars that the masses of the stars do not differ very greatly from that of the sun, the ratio in the case of the majority ranging from three to one-third. Suppose then that a faint M star has a mass equal to that of the sun. Being cooler, it will be somewhat denser, and we may therefore take its diameter to be about 700,000 miles. The diameter of a bright M star of equal mass would then be over 30 million miles, so that it is a veritable giant.

Russell therefore propounded the theory that the members of the upper group of bright stars are giants, while those in the lower one are dwarfs by comparison. Within the last two months a brilliant verification of this theory has been obtained. On December 13, 1920, the diameter of α Orionis (Betelgeuse) was measured at the Mount Wilson observatory by means of an interferometer attached to the 100-inch reflector, and was found to be one twenty-second part of a second of arc. The distance is uncertain, but assuming it to be 130 light-years, we find the diameter of Betelgeuse to be 170 million miles, which is nearly six times the diameter calculated for our hypothetical star. We assumed, however, that the absolute magnitude of our giant was 1, while that of Betelgeuse is −2. A recalculation with this latter value makes the diameter 250 million miles. In any case there is now no doubt that the stars may be divided into giants and dwarfs, Betelgeuse being a typical giant, while our sun is a typical dwarf.

Proceeding from considerations of this kind, Russell suggests a sequence of stellar evolution which may be briefly sketched as follows.

A star begins its existence, or at least begins to be visible, in the condition of a red giant. Under the action of gravitation it contracts, and in consequence of this contraction its temperature rises and it passes upwards through the spectral series M, K, G, F, A. If massive enough, it may even attain the B stage. During all this time the temperature in the interior has been increasing very rapidly, and Eddington has shown that the pressure arising from thermal and light radiation, though at first negligible, ultimately becomes so great that it may overpower gravitational attraction and cause the disruption of the star. If the star is rotating, centrifugal force will assist rupture, and in this way the production of binary stars may be explained. After attaining its maximum temperature, the star, whose diameter has now contracted to dimensions comparable with that of the sun, starts on its downward course, retracing the spectral series in the order B, A, F, G, K, M. It loses more heat than it can gain by contraction, and at length, after reaching the red stage, passes below the limit of visibility.

It is not pretended that this theory is correct in all its features. It will certainly need revision in the light of fuller knowledge. But at least it affords a working hypothesis which appears not to be in serious conflict with any facts at present known.

We may now go on to enquire what was the star before it began its visible career. In what state of aggregation or form did the matter of which it was composed exist?

Ever since Laplace propounded the Nebular Hypothesis of the origin of the Solar System there has been a general acceptance of the view that nebulæ precede stars in the order of evolution. It seems natural to assume that widely diffuse aggregates of matter form systems dynamically unstable, and that under the action of gravitation they will tend to condense into more compact and stable forms. It is true that mathematical investigation has seriously challenged the competence of the Nebular Hypothesis to account for the origin of the Solar System, but the grounds of this challenge are for the most part irrelevant when the problem of the origin of a single star, or even of binary or multiple stars, is under discussion. However, in the absence of exact knowledge regarding the forms and constitution of the nebulæ, speculation regarding the relations existing between them and the stars was of little value, and it is only within the past few years that observational material has been obtained which really affords an adequate basis for fruitful theorising.

The nebulæ are classified according to shape as irregular, planetary, and spiral.

The irregular, as the name implies, present no simple geometrical outline. They often cover large areas in the sky, while condensation in the neighbourhood of particular stars and wisps of nebulous matter joining star to star indicate without question the existence of a relation between the nebular and stellar forms. The best-known and most typical nebulæ of this class are the great nebula in Orion, whose brightest portion envelops the quadruple star θ Orionis, and the nebula surrounding the Pleiades. To this class also belong the dark nebulæ whose existence is deduced from the apparent obscuration of the light of the stars in certain parts of the Milky Way.

The planetary nebulæ are so named because they exhibit a regular circular or oval outline enclosing a disc which in some cases is of fairly uniform brightness but in others mottled. They thus bear a superficial resemblance to planets. Recent photographic and spectrographic work, however, leaves little doubt that these nebulæ are to be regarded as shells of matter. Their varying appearance may be ascribed to differences in the thickness of the shell. When it is thin, the resultant appearance is that of a ring, such as that of the well-known ring-nebula in Lyra, but when the interior space is more completely filled, the disc presents a more uniform degree of brilliancy.

These nebulæ also are closely associated with stars, for in nearly every case a stellar point of condensation is found at the centre.

Although these two types of nebula are so different in form, the spectra are similar. They are bright-line spectra, proving that the matter which emits the light exists in the form of gas. The spectral lines are those characteristic of hydrogen and of an unknown element to which the name ‘nebulium’ has been given. The most conspicuous lines ascribed to nebulium are two green ones, and hence these gaseous nebulæ are often called green nebulæ, in contradistinction to the white nebulæ whose spectrum is continuous.

The opinion was held till recently that these gaseous nebulæ stand at the beginning of the path of stellar evolution. It was supposed that they immediately preceded a class of stars, very few in number, whose spectra contain bright lines, and that these again developed into the B stars. The close association of the nebulæ with stars seemed to lend support to this view.

However, if Russell's theory of stellar evolution is correct, it is difficult to find a place for them. They certainly cannot come at the beginning or end of the series, next to the M stars.

Only about 150 planetaries are known, but if they were the initial stages of stars we should expect them to be much more numerous.

These, and other considerations which I have not time to mention, suggest that the gaseous nebulæ are products of a side-path of evolution. It is very significant that the nebulium lines appear at a certain period in the history of new stars, and there is little doubt that new stars are due to the occurrence of some event of a catastrophic nature, probably a collision between two masses of matter. In such a case temperature would be raised rapidly to the point at which radiation pressure becomes significant, and thus shells of matter would be driven off giving rise to planetary nebulæ. Perhaps this may even be the origin of the diffuse irregular nebulæ. The appearances which several of them present suggest rather expulsion from, than attraction to, the stars with which they are associated.

The spiral nebulæ are far more numerous than the nebulæ belonging to the other two classes, the numbers within the reach of modern telescopes being estimated to be about a million.

The typical form is a central globular condensation, from two diametrically opposed points of which proceed arms which coil round the nucleus in spiral form. Along these arms are numerous knots and condensations, some of which are almost of stellar aspect.

The appearance of the spirals depends upon the angle of presentation, some being seen full face, so to speak, while in others the plane of the spirals passes through the earth, and the nebula takes on a spindle-shaped form. In these latter forms the darker and presumably cooler matter of the arms is seen projected against the more luminous central nucleus.

The spectra of these nebulæ are quite different from those of the irregular and planetary types. They are continuous, crossed by dark lines, and so resemble those of the stars.

The main problem to be solved in connection with the spiral nebulæ is whether they are a part of our own sidereal system, or are to be regarded as island universes, systems coequal with our own, situated far beyond its limits.

The evidence which supports the latter view may be briefly summarised.

In the first place, nearly all appear to be moving relative to our system with very great velocities: whereas the average speed of a star is of the order of 20 miles per second, that of the nebulæ is about 500 miles per second. In the second place, with very few exceptions, they appear to be receding from our system.

Thirdly, in some of them temporary stars have appeared, and the average maximum magnitude attained by these stars, when compared with the average maximum attained by new stars known to belong to our own system, indicates that the spirals are a hundred times as distant as the Milky Way, which is the region in which new stars almost invariably appear. This estimate of distance agrees with deductions made by Curtis from consideration of proper motion and radial velocity. For the spirals examined he obtains an average distance of 10,000 light-years.

An explanation of the spiral form has recently been given by Jeans. He has studied the dynamics of a rotating mass of gas, and has shown that under certain conditions, depending upon density and speed of rotation, it will disintegrate. A sharp edge will be formed along the equator, and from two opposite points on the edge matter will spill out. Filaments will thus be ejected in a continuous manner, forming spiral arms in the equatorial plane of the nebula.

This explanation makes the spiral formation a consequence of rotation, and rotation has actually been observed in a few nebulæ. For example, by comparison of photographs taken at different periods, Von Maanen has found that the matter at a distance of five minutes of arc from the centre of a nebula in Ursa Major would make a revolution round the nucleus in 85,000 years, and that there is also an outward motion of the matter along the spirals.

Photographs of other nebulæ which are seen sideways show nuclei with sharp equatorial edges, such as Jeans' theory demands.

If we attempt to make an estimate of the amount of matter in any one of these nebulæ, basing our calculations upon the observed angular dimensions, and making reasonable assumptions regarding their distances from us, we find that there is sufficient matter to construct many thousands, even millions of stars, equal in mass to our sun.

It seems therefore not at all improbable that these nebulæ are the stuff out of which universes of stars are made. The actual steps in the process can only be dimly guessed. All the appearances suggest that gravitation breaks up the spiral arms ejected from the rotating central nucleus into comparatively short portions which ultimately condense into stars. Stars so formed would continue to rotate about the centre of the nebula in a plane coinciding with the original plane of ejection. Disturbances due to the attractions of other bodies would cause them to deviate in the course of time from the fundamental plane, but the system would maintain a considerable degree of flatness, such as is actually the case in our own galactic universe.

These and many other problems which I have not touched upon must be thoroughly investigated before an ordered picture of the course of cosmic evolution can be formed. They may appear to have but little bearing upon human life and activity, but it must be remembered that the value of a science is not to be measured by its immediate practical utility or economic significance. Nevertheless, as civilisation is largely a product of the exact sciences, so Astronomy, being in a very real sense their parent, may claim no small share in directing the thought and progress of the race.

J. B. Dale

III

PHYSICS

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III
PHYSICS

IN the later decades of the nineteenth century there was a general feeling among natural philosophers which, though not often explicitly expressed, is evident in the writings of that time, that the great guiding principles of this science had been well and securely established, and that little remained for workers in physics to do beyond filling in the details of the subject. This state of things has been entirely changed as a result of a succession of remarkable discoveries, which started with the discovery of the electron, the X-rays, and radioactivity in the closing years of the nineteenth century. The consequence of these and of succeeding discoveries has been a veritable revolution, with the result that the state of physics at the present time is not unlike the contemporary picture of the economic and political condition of the globe. It is true that some of the old principles—as, for example, the fundamental laws of thermodynamics—have emerged from the welter as securely established as ever. The laws of dynamics as formulated by Newton and his successors are now seen, on the other hand, to have only a restricted validity, and are applicable without modification only to slowly changing or slowly moving systems. There are, in fact, many old-established laws which one now hesitates to employ dogmatically without fear of contradiction. On the other hand, new guiding principles have arisen which were undreamed of only thirty years ago.

Of these the most far-reaching is the principle of relativity. I am aware that Professor Nicholson has explained to us that it is impossible for a mathematician to explain this principle to a physicist; *a fortiori*, therefore, it is more than impossible for a physicist to explain it to a layman. However, I feel that I can speak about what I do not understand, perhaps not as well, but with as much right, as any other kind of philosopher. I shall therefore take the liberty of saying a few words about relativity. In reality there are two—not altogether unconnected—principles of relativity. The older, which may be regarded as a particular case of the other, is sometimes referred to as the restricted relativity principle. It is based on the empirical fact that it has been found impossible to detect absolute motion by any mechanical, optical, electrical, or other experiments. So many promising experiments in this direction have been tried with negative results that I feel confident that the restricted principle rests on a very secure foundation. The gravitational relativity principle is based on the experience that all material bodies are equally accelerated in a gravitational field, and I do not feel that the empirical basis for this principle is so secure as that of the restricted form. At any rate, I have for some years been engaged on very delicate experiments in the Wheatstone laboratory to see if small differences in the gravitational acceleration of different kinds of matter cannot be detected. If these experiments lead to a negative result the position of the gravitational theory will be strengthened thereby. If, on the other hand, they should give a positive result, I do not anticipate that it will be beyond the ingenuity of Professor Einstein to frame a theory which will take account of them.

Many of the most pressing problems of physics at the present time are bound up with what is known as the quantum theory. This theory has arisen as a result of the failure of dynamical principles to account for a number of important physical phenomena. There is, of course, no convincing *a priori* reason why dynamics should be adequate to account for all the phenomena of physics, but a belief in such adequacy had, in fact, become widespread. The recalcitrant phenomena were in the main those dealing with heat radiation, the specific heats of bodies at low temperatures, and photoelectric effects. It is difficult to formulate the quantum theory accurately within the compass of a nutshell, and in fact it is still not quite certain just what its kernel consists of. This much at any rate seems certain, that in the interactions between radiation and matter which result in the transference of energy from the radiant to the material form or *vice versa* there are processes which take place discontinuously and are incapable of description in mechanical terms. The basis of the quantum theory, however, appears to be broader than this. It has been variously formulated by different authors. I prefer to regard it as a restriction on the systems which are dynamically possible. Such a formulation has been given by Dr W. Wilson of this College, and has been very successful in the recent developments of the subject, particularly in the hands of Continental writers. According to him, the dynamically possible systems are restricted to those for which twice the value of the integral of the kinetic energy with respect to the time taken over a complete period is equal to an integral number n times a universal constant, h, known as Planck's constant. Ordinary dynamics lays no such restriction on the motions which are possible.

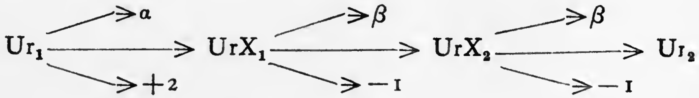
One of the great triumphs of the quantum theory has been its use in the foundation by Bohr of a theory of the emission spectra of the elements.

We have direct and convincing evidence that the atoms of the chemical elements are made up of a positively-charged nucleus, which contains almost the whole of the mass of the atom, in association with such a number of relatively light negatively-charged electrons as are required to make the whole structure electrically neutral. The simplest atom, that of hydrogen, consists of a single negative electron having $\frac{1}{1850}$ of the total mass in association with a single nucleus having a positive charge of equal magnitude and possessing a mass equal to $\frac{1849}{1850}$ of that of the atom. According to Bohr's theory the electron and the nucleus are in a state of relative motion similar to that of the earth and the sun. According to the quantum hypothesis, however, only certain orbits are possible, namely, those which satisfy the quantum conditions referred to in the last paragraph. Such orbits are executed without loss of energy by radiation, and are called by Bohr the stationary states of the atom. There are an infinite number of such states corresponding to all the positive integral numbers. Each of these states is a potential hydrogen atom, but the state for which $n = 1$ possesses much less energy, and is therefore much more likely to be formed than any of the others. This particular state is taken to represent the ordinary hydrogen atom. The other states are only likely to occur in appreciable numbers under exceptional conditions, such as at high temperatures or under intense electrical stimulation as in vacuum-tube discharges. Another essential part of the content of Bohr's theory is that the atoms are able to pass spontaneously from states of higher to states of lower energy, and the energy thus lost appears as monochromatic radiation whose frequency is determined by the quantum laws. It is this emission which constitutes the line spectra of the element. For every transition between any two stationary states there will be a single line, and all the possible transitions to a given end state will give an infinite series of lines. Such a series constitutes a series spectrum like Balmer's series. In this way Bohr has accounted quantitatively for all the known series spectra of hydrogen, one of which has been discovered since he predicted its existence. It would take us too long to go into the other successes of Bohr's theory, such as the explanation of the series of enhanced lines of helium and of other elements, the fine structure of the lines as modified by the Stark effect, and so on. It must suffice to say here that Bohr has given us the first real theory of spectroscopic phenomena.

We have so far assumed that the positively-charged part of the atom consists of a minute but relatively massive nucleus. The most direct and convincing evidence for this lies in the results of experiments by Sir Ernest Rutherford and his collaborators on the scattering of the positively-charged α particles, which are emitted by radio-active substances, in their passage through different kinds of matter. These α particles have been proved to be helium atoms which have lost two negative electrons — in other words, the nuclei of the helium atoms — moving at very high speeds. These α particles are so minute and their momentum is so great that the majority of them pass through thin layers of various kinds of matter without sensible deflection. Their motion is in fact almost unaffected by the light negative electrons they encounter. It is only when they come into relatively close contact with the massive positive nuclei that their paths are sensibly deflected and they are scattered through large angles. A close study of this relatively infrequent large-angle scattering has shown that the law of force between the α particle and the nucleus is the ordinary electrostatic law of the inverse square until the two centres approach within one-millionth of one-millionth part of a centimetre of each other. It has also shown that the electric charge of the nuclei of the different atoms is equal to the charge on the electron multiplied by the atomic number— that is to say, by the number which is obtained when the different chemical atoms are arranged in the order of their atomic weights starting with hydrogen. Thus the successive atomic numbers of the light elements are H = 1, He = 2, Li = 3, Be = 4, B = 5, C = 6, N = 7, O = 8, and so on. The nuclear charge of boron, for example, is $5e$ where e is the magnitude of the charge on the electron. The boron atom is made up of this nucleus associated with the 5 electrons which are required to make the structure neutral. The configuration of these electrons will be determined almost entirely by the charge on the nucleus; so that we see that it is the nuclear charge which will determine the properties of the atoms, the mass of the nucleus being a relatively secondary matter.

The structure of the nuclei of the various atoms furnishes a very fascinating problem. The first evidence in this direction was obtained from the phenomena of radioactivity. You will recall that radioactive substances emit two kinds of electrically charged rays, the α and β rays respectively. Of these the α rays have been proved to be identical with the nucleus of the helium atom; so that it is clear that the nuclei of the heavy radioactive atoms contain a number of helium nuclei among their constituents. The β rays are rapidly moving negative electrons, and there is convincing evidence that these also come out of the nuclei of the radioactive atoms. As a result of the expulsion of these rays the radioactive elements are spontaneously transforming themselves into other elements. A very clear insight into the nature of the products of these transformations is obtained when they are considered from the point of view of the charge of the nucleus of the atom. An α ray consists of a helium nucleus and has a positive charge equal to the charge of two negative electrons. For the sake of brevity let us refer to the electronic charge as the unit charge; it is, in fact, the natural unit of electric charge. Then the expulsion of an α particle will reduce the nuclear charge by two units. On the other hand the expulsion of an electron, which is negatively charged, will increase the nuclear charge by a single unit. Inasmuch as we have seen that the chemical properties and the chief physical properties of the elements are determined by the magnitude of the nuclear charge, each such expulsion of an α or β particle will in general result in the generation of an atom of a new element. Furthermore, the ejection of an α particle will diminish the atomic weight of an atom by 4 units, the atomic weight of helium being very close to 4. The expulsion of a β particle, on the other hand, will leave the atomic weight of the atom unaffected. One result of this is that the expulsion of a β particle will generate an atom of an element whose chemical properties are different from those of the parent atom, but whose atomic weight is the same as that of the parent atom.

As a further illustration let us consider the first three changes which are undergone by the radioactive element Uranium. These may be represented by the following scheme:



At. wt. 238	234	234	234
Nu. chge. 92*e*	90*e*	91*e*	92*e*

Uranium, whose atomic weight is close to 238 and whose atomic number is 92, emits an α particle and turns into UrX_1. This element, therefore, has an atomic weight 234 and a nuclear charge 90*e*, *e* being the magnitude of the electronic charge. It in turn emits a β particle and turns into UrX_2, which has the same atomic weight but a nuclear charge one unit greater than UrX_1. UrX_2 emits a β ray and forms a new element known as Ur_2. This has the same atomic weight, 234, as UrX_1 and UrX_2, since only β rays are involved in these two transformations. On the other hand, it has a nuclear charge one unit higher than that of UrX_2 and two units higher than that of UrX_1. This brings its nuclear charge back to the Ur_1 value. We have seen, however, that the chemical properties of the elements are established by the nuclear charges of their atoms. It follows that the elements Ur_1 and Ur_2, which have equal nuclear charges but different atomic weights, should be chemically inseparable. As a matter of fact that is found to be the case. Such pairs of elements, which have the same chemical properties, but whose atoms have different masses, are known as isotopes. The recent experiments of Aston have shown that many of the common elements, such as chlorine, consist in reality of a mixture of isotopes. When the different isotopes are sorted out it appears that the various atomic species have atomic masses which are whole numbers in terms of oxygen = 16, except hydrogen, whose atomic mass in terms of this unit is 1·008. Hydrogen has been shown not to be a mixture of isotopes, so that this deviation from unity is not due to the admixture of a relatively small proportion of an isotope of mass 2 or 3.

We have seen that the heavy radioactive atoms contain helium and electrons as part of the structure of their nuclei. What of the light atoms? This problem has been successfully attacked by Rutherford by the very direct method of bombarding the nuclei of the light atoms by α particles and examining the properties of the fragments which are ejected as a result of what is practically a head-on collision. These fragments in their general properties resemble the α particles which cause their ejection. The two can, in fact, only be distinguished when their characteristics are investigated from a quantitative standpoint. They may, for example, be able to penetrate for a greater or less distance in air, they may be deviated to a greater or to a less degree by an electric or by a magnetic field. These are the tests which serve to differentiate them, the presence of either class of projectile at any point being ascertainable by the scintillations which they cause on a zinc sulphide screen. By tests of this kind Rutherford has been able to prove that hydrogen can be knocked out of the nucleus of the nitrogen atom but not out of oxygen or carbon. He has also adduced considerable evidence in favour of the view that a body of mass 3 and charge 2 can be ejected from the nuclei both of nitrogen and of oxygen. Such a body would be an isotope of helium, which has a nuclear charge of 2 units and mass 4.

The consideration of the energy which may be stored in the nuclei of atoms furnishes problems of the greatest interest and importance. We have seen that the heavy radioactive atoms are continuously undergoing spontaneous disintegration, and that this process consists primarily in a decomposition of the nucleus. This decomposition is accompanied by a continuous evolution of heat energy, as is evidenced by the fact that radioactive bodies are always at a somewhat higher temperature than their surroundings. Precise measurements of the quantity of heat thus generated have shown that in proportion to the amount of material decomposition taking place, it is of an altogether higher order of magnitude, in fact a million or more times greater, than that generated in chemical reactions such as the combustion of coal. Owing to the difficulty of concentrating considerable quantities of intensely radioactive materials, this spontaneous generation of heat is not likely to prove economically important, although, owing to the wide dissemination of the radioactive elements, it may be an important factor in maintaining the heat of the earth and the celestial bodies.

The disruption of hydrogen from the nitrogen nucleus by Rutherford has shown that a similar process is to a certain extent under our control, and is operative with elements which are available in almost unlimited quantity. It is clear that if this artificial disintegration of the elements is accompanied by a development of heat of the same order of magnitude as that generated in the spontaneous disintegration of the radioactive elements, and if it can be started and controlled without undue expenditure of energy, we shall have obtained a prime source of energy which will make all the present sources fade into insignificance. These are large questions which it may take some time to answer satisfactorily, but

Rutherford has already obtained some measure of an answer to the first of them. We have seen that under bombardment by α particles there is evidence that the atoms of oxygen and nitrogen give off bodies of mass 3 and charge 2 units. If the mass of these nuclear fragments has been correctly determined, an elementary calculation shows that they possess more kinetic energy than that of the α particles which stimulated their ejection. It thus appears that without making any allowance for the energy left in the α particle and in the residue of the disrupted atom, the process involves the tapping of energy from a source within the disrupted nucleus, and the ratio of this gain of energy to the amount of matter involved in the process is of the same order of magnitude as in the spontaneous radioactive disintegrations. Owing to the high importance of this result, it is necessary that it should be established beyond the possibility of doubt. This, I think, can hardly be held to have been done. The evidence is based on measurements of the range of the ejected particles (*i.e.*, the distance they can travel in air without being stopped) and of their deviation by a magnetic field. The combination of these two measurements determines the ratio of the square of the mass of the fragments to their electric charge to within perhaps ten per cent., but there are no measurements which give these two quantities separately. It appears that the experimental data are satisfied about as well by an isotope of hydrogen of charge 1 and mass 2 as by the helium isotope of charge 2 and mass 3. If the hydrogen isotope is taken as the solution, it is found to have considerably less energy than the ejecting α particle; so that the proof of the generation of energy falls to the ground. I should like to add that there are qualitative considerations which would take us too long to go into which make the body of mass 3 the more probable alternative. The position may therefore be summarised by saying that the experimental results make the controlled generation of energy from the nucleus a probable fact, but they do not establish it with absolute certainty.

Apart from the question just under discussion, of the magnitude of the energy, it does not seem possible to reconcile all the data which have been obtained without assuming that the fragments are ejected as the result of a secondary nuclear explosion rather than a merely dynamical impact between the nucleus and the α particle. So far the projectiles (α particles) which have been able to cause these explosions possess an enormous concentration of momentum, such as it would be impossible to obtain except from radioactive sources. The application to large-scale phenomena is therefore far from being realised. It may be that other agencies such as electrons or high-frequency X-rays will be able to cause instability in atomic nuclei. An indefinite supply of either of these could be obtained by known laboratory methods. There is an unpleasant possibility in the nuclei of atoms which I should like to mention in passing. It is possible that if these effects get going at any time on any considerable scale they may spread from atom to atom with explosive violence, and that would be the end of all things.

I now come to the last problem which I shall consider. Is the hydrogen nucleus the positive electron out of which all the other nuclei are built up? I think there can be little doubt that it is. In the first place it has an electric charge equal and opposite to that of the negative electron, and it is the only body in the universe which has this charge. This in itself is a sufficiently astonishing fact which may prove to be one of the ultimate riddles of Nature. But, you may say, if the hydrogen nucleus is the unit out of which the nuclei of the other atoms are constructed, why are the atomic weights what they are? For example, why are they H = 1·008, C = 12·00, O = 16·00, Cl = 35·5 and not H = 1·000, C = 12·00, O = 16·00, Cl = 35·00? The answer in the case of chlorine has been given by Aston, who has shown that it is a mixture of two isotopes whose atomic weights are 35·0 and 37·0. Similar considerations have been shown to apply to a number of other elements, and they probably account for nearly all the deviations from whole numbers in the atomic weights. They will not, however, account for the atomic weight 1·008 of hydrogen, which has been carefully examined and found not to consist of a mixture of isotopes. If, however, the heavier elements are formed from hydrogen with evolution of energy we should expect the atomic weight of hydrogen to be greater than unity. One of the theorems which the recent advances in theoretical physics have given us is that the mass of any material system is proportional to the total energy of the system. The fact that the only positively-charged fragments which are ejected from the nuclei of the radioactive atoms are helium (of mass 4 and charge 2) suggests that the main substructure in the various nuclei is a helium nucleus formed by the combination of 4 hydrogen nuclei and 2 electrons. In our view the fact that the mass of this is less than four times 1·008, minus the mass of the negative electrons, merely means that the substructure is formed with evolution of energy. This energy will be measured by the difference between the two sides of the equation between the respective masses involved in the reaction. A similar correction for the mass of the evolved energy should strictly be made in writing the equation between the masses involved in any chemical reaction, but in practice the amount of energy interchange in chemical reactions is of a lower order of magnitude, and can be disregarded. In concluding, there is one other interesting application of these ideas which I should like to mention. It is now very probable that in the evolution of the older stars from the nebulæ the heavier elements are continuously being formed, in some manner which we do not yet understand, from the lighter ones such as hydrogen or helium. These processes should be expected to furnish an enormous amount of heat, far greater than that which can arise from the Helmholtz theory of mutual gravitation of the contracting parts. By these considerations the duration of the existence of the heavenly bodies is enormously increased over that given when gravitation is regarded as the source of their energy.

O. W. Richardson

IV

ORGANIC CHEMISTRY

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IV

ORGANIC CHEMISTRY

THE suggestion that a distinction might be made between the chemistry of minerals and that of substances formed by living organisms seems to have been first made in 1675, when Lémery, a French chemist, published a general treatise on chemistry. In this work the subject was treated in the three main divisions of Vegetable, Animal, and Mineral Chemistry; but the distinction seems to have been adopted merely for convenience of handling the subject and evidently was not based on any precise chemical difference between the three groups. The author was, however, clearly aware that the substances of vegetable or animal origin were more complex than those derived from minerals.

Very little progress was made until more than a hundred years later, when the conception of chemical elements and the methods of detecting these had been developed. Thus about the year 1790 Lavoisier reported that substances of animal or vegetable origin differed from mineral substances by the presence of carbon; they also contained hydrogen and oxygen. Lavoisier was also able to detect the presence of nitrogen and phosphorus in many substances of animal origin, and, being unable to find these elements in plant materials, he considered that the two types of product were different in character, and he therefore was inclined to retain the distinction made previously on purely formal grounds.

With the advance in methods of analysis it was soon found that nitrogen and phosphorus are present in many substances obtained from plants, and, Lavoisier's distinction being no longer justified, the chemistry of the two groups became fused together under the name 'Organic Chemistry,' or the chemistry of substances formed under the influence of vital forces. It was recognised that the substances obtained from plants or animals always contained carbon. Moreover, quantitative analysis showed that they were usually more complex than the compounds obtained from mineral sources, and were of a quite different chemical nature, being more easily decomposed by heat and by chemical agents.

These distinctions between the two groups were mainly of a chemical nature, but another fundamental difference seemed to justify the separation of mineral from organic chemistry. It had been found that many compounds of mineral origin could be reproduced by synthesis in the laboratory, but all attempts to obtain 'organic' compounds in this manner failed; in fact, it was supposed that in living nature elements obey different laws from those obtaining in minerals. The notion was evidently confirmed by the remarkable difference in the chemical nature of the substances met with in the two groups. This was the position in 1827. By this time chemists had come to the conclusion that it was highly improbable that they would ever succeed in imitating the products of living nature.

In the following year, however, two series of experiments were made which opened a new period in the science of organic chemistry. Alcohol had hitherto been obtained only by the action of the living organism of yeast on sugar—a product of vegetable origin; but Hennell showed that it could be made from ethylene, a gaseous compound of carbon and hydrogen. Then Wöhler succeeded in obtaining urea by heating a solution of ammonium cyanate. Urea had previously been obtained only from the decomposition products of the animal organism. The challenge which these classical researches offered to the vitalistic conception of organic chemistry was not fully recognised at the time; in fact, this view seems to have persisted in more or less modified forms for another quarter of a century. By that time the synthesis of many substances occurring in nature had been effected, and organic chemistry ultimately became merely the branch of chemistry which concerned compounds of carbon. It is true that the distinction between organic and mineral chemistry is by no means sharp, for there are many carbon compounds which contain metals, such as mercury or magnesium, and other elements besides carbon, hydrogen, and oxygen, but the separation offers many advantages, and is justified by the peculiar nature of the chemical reactions of carbon compounds.

Before this modified view of organic chemistry became general, the science had made great progress, chiefly with the aid of the fundamental notions of atoms and molecules. Matter, whether gaseous, liquid, or solid, was supposed to consist of small units termed molecules. In any piece of matter which is chemically homogeneous all the component molecules are alike, and the chemical properties of the form of matter are also those of each individual molecule. These molecules are not indivisible ; they are conceived to be clusters of atoms which are the smallest chemical units of matter. Atoms are chemically indivisible and are the smallest units of the chemical elements which can enter into combination with each other to form molecules. In molecules of the elements the atoms are all alike, but the molecules of many complex carbon compounds may consist of as many as five or six different kinds of atoms, according to the number and kind of the elements present. It should be noted that these complex clusters of atoms are not unalterable ; when they are attacked by chemical means they may be added to or broken up, and in either case new molecules are formed, which may be more complex or simpler than the original.

Investigation of these carbon compounds has shown that some molecules which have quite different chemical properties are composed of the same number and kind of atoms. For example, there are two kinds of molecules composed of two carbon atoms, six hydrogen atoms, and two oxygen atoms. Such cases as this are numerous ; they can be explained only by the assumption that the difference in the molecules results from a difference in the arrangement of the atoms composing them. In other words, it is evident that the molecular structure or architecture is different in the two substances.

The unravelling of the structure of the complex molecules met with in organic chemistry proved by no means a simple matter ; in fact, the modern conception of molecular architecture was attained only after prolonged and tedious research and bitter controversy. The methods used in the earlier work were those of analysis. The pioneers Bunsen, Gay-Lussac, and Liebig showed by their researches that definite groups of atoms in a molecule may be replaced by other groups of atoms or even by a single atom. It was evident that those atoms which are removed together from a molecule, or persist in union throughout a series of chemical changes, are in some way intimately connected together. By an exhaustive application of this kind of process to a molecule, various clusters of the component atoms can be detected; and then, the molecule having been thus resolved into simpler components, the fragments may be imagined to be pieced together and a picture of the complete molecular architecture so obtained. The case of acetic acid may be quoted as an illustration. The molecule of this substance contains two atoms of carbon, four of hydrogen, and two of oxygen. These facts may be summarised by the formula $C_2H_4O_2$, where the letters C, H, and O represent carbon, hydrogen, and oxygen, and the numerals the number of each kind of atom present. Numerous chemical reactions of acetic acid show the presence of the following groups of atoms: (OH), (CH_3), (CH_3CO), and (CO_2); hence, putting these fragments together, the molecular structure of acetic acid appears to be CH_3COOH.

The enormous volume of research which has been carried out on these lines has involved the production of many new molecules which have resulted from the breaking up of more complex material, and these have been utilised in further experiments. The chemist, having based a conception of the molecular structure on these analytical methods, has endeavoured to reconstruct in actual experiment the molecule of the natural product from suitable fragments obtained either from the substance in question or from other sources. The results obtained by the use of this synthetical method are remarkable. To give here a complete list of nature's products which have been synthesised is unnecessary, but attention may be directed to the following interesting cases: fats, simple sugars, tannins, uric acid, caffeine, camphor, limonene (the chief constituent of oil of lemon), menthol, adrenaline (the active principle of the suprarenal capsules), cocaine, nicotine, and such natural dyestuffs as alizarin, indigo, the yellow dye of the dyer's broom, the pigment of gamboge, and the colouring matters contained in the petals of flowers and the skins of various fruits; all these have been synthesised. More complex substances such as hæmatein, the pigment of blood, and chlorophyll, the pigment found in leaves, are at present under investigation; their structures have been partly resolved, and it does not seem too sanguine to hope that these materials also will yield to the efforts of the chemist.

It has been explained in a foregoing paragraph that these syntheses have been effected by building up the desired structures with suitable fragments obtained from various sources. The organic chemist has not confined his work to the imitation of natural products in this way; but, taking portions from various types of molecules, he has synthesised a very large number of entirely new substances. Many of these find use in industry. Some, for example, are highly coloured; among these, some are suitable for use as dyes; others emit a fragrant odour and are employed in the scent industry; others have unstable molecules and are used as explosives; whilst others have specific physiological action and are employed as drugs. At the same time many other substances of purely theoretical interest have been obtained, and the study of these has in many ways led to valuable advances in the theory of the science. In the course of all this work an enormous number of synthetical carbon compounds have been prepared—at the present time about 200,000 are known—and it cannot be denied that the great majority of these have been of very little service to the science or to industry. However, it can hardly be expected that such important results could be attained without the accumulation of a great deal of waste material. No machine ever is completely efficient, and human effort applied to the investigation of nature meets with very many failures.

Attention may now be turned to a more recent development in the work of organic chemists. As already explained, considerable success has been met with in reproducing the simpler products of nature in the laboratory, but enquiry is now being directed to the methods used by nature in forming these. Not only is very great scientific interest attached to these problems, but the successful solution of some of them might ultimately be of high economic value. For example, a knowledge of the processes followed by the plant in forming a valuable product might enable us to stimulate the production, or perhaps a study of these processes might lead to an important artificial synthesis of the substance.

The question of how the plant or animal converts the material which it absorbs as food into such a variety of materials is a very complex one. The case of the plant appears to be the simpler, for the organism here deals at the outset with relatively simple materials, carbon being chiefly assimilated through the leaves as carbon dioxide from the atmosphere, whilst nitrogen and phosphorus are taken up through the roots. The conditions of synthesis are very different from those adopted in the laboratory; for instance, the plant operates at the temperature of the atmosphere whilst the cells of the plant tissue serve as vessels in which the reactions are carried out.

So far as the assimilation of carbon is concerned, the materials available are the carbon dioxide and water vapour of the atmosphere. Experiment in the laboratory has shown that these simple materials may be converted by the action of light to oxygen and a very simple carbon compound termed formaldehyde. The molecular composition of formaldehyde is simple; it contains carbon, hydrogen, and oxygen in the relative amounts indicated by the formula CH_2O, and its formation by this process may be represented as follows: $CO_2 + H_2O = O_2 + CH_2O$. It has been known for many years that oxygen is given off by the leaves of the growing plant, and search for this formaldehyde in green leaves soon proved that this substance was present in small quantity. It seems probable, therefore, that the first step taken by the plant in the assimilation of carbon has been detected. In the process the reaction between the materials is brought about by light, which is undoubtedly assisted in its catalytic action by the chlorophyll of the leaves, but the precise *rôle* of the latter substance is not yet fully understood.

The fact that light can excite chemical reaction has been known for very many years, but this discovery of the production of formaldehyde seems to have drawn the attention of organic chemists to the possibility of its use as a synthetic reagent, and at the present time our knowledge of the photochemistry of organic compounds is rapidly expanding. It has been found that this agent can not only effect the synthesis of complex substances, but also may break these up into simpler materials. Thus formaldehyde may be converted into sugars, and the latter may be partly broken up by light into formaldehyde and other products. At present, then, it appears that light may be used by the plant to effect syntheses through the medium of the primary product formaldehyde, but also partially to break up the complex products into others, which in turn may undergo renewed reaction.

Research carried out by biochemists has shown that another type of reagent or group of reagents takes part in the chemical reactions of the plant. These substances are also catalytic in their action ; they are termed enzymes. At present very little is known of the chemical nature of the enzymes ; they are very unstable substances, and this property, together with their physical nature, renders their examination very difficult. The majority of those which are known appear to act solely as degrading agents, and have a specific action with definite types of compounds. The investigation of the enzymes is hardly beyond a preliminary stage, but sufficient is known of them to enable a classification to be made according to the type of substance which they decompose and the nature of the decomposition which is effected.

Still less is known concerning the formation of nitrogen and phosphorus compounds in the plant ; the problem is undoubtedly very complex, and has been very little discussed except in a few cases where the materials used by the chemist in his laboratory syntheses can be assumed with some degree of certainty to be present in the plant.

From this very brief review of one aspect of the recent work on carbon compounds it will be seen that most important discoveries have been already made in the chemistry of living plants. In spite of the very great difficulties which must be overcome, these preliminary advances, viewed together with the constant progress of organic chemistry in the past century, justify a confident hope of the success of future investigation.

S. Smiles

V

BIOLOGY

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V

BIOLOGY

TO take stock of the present position of the Biological Sciences and to discuss their mutual relationships is a task from which the boldest might well shrink, but one which it seems very desirable to attempt at the present time, even though it be possible to carry it out only in the most imperfect manner. Scientific knowledge of all kinds has been accumulating during the past century at an altogether unprecedented rate, and if we are not to be completely overwhelmed and smothered by the products of our own mental activities it is before all things necessary that the monstrous heap should be organised in some rational manner.

Biology—the science of all the innumerable phenomena manifested by living things—has much in common with Chemistry and Physics. Indeed, in so far as living organisms carry on their functions and work out their destinies by chemical and physical means, Biology may be looked upon as a sort of super-chemistry and super-physics, and obviously presupposes an adequate acquaintance with both these departments of learning. The living organism, taken as a whole, however, is something more than a mere physico-chemical machine. It possesses an individuality and exhibits a purposive behaviour which raise it to an altogether higher plane of existence, and in this fact, apart altogether from the question of convenience of treatment, lies the justification for separating Biology from Chemistry and Physics and regarding it as one of the cardinal sciences.

It has long been recognised that the content of our science, as thus defined, is far too vast and comprehensive to be treated academically as a single subject, and in our universities it is customary to divide it primarily into Zoology, Botany, Anatomy, and Physiology, the two latter, together with Medicine, being treated rather as applied than pure sciences, and developed with special reference to a single organism—Man.

There is much to be said for this arrangement as a mere matter of convenience, and doubtless on the whole it has justified itself, but it has many grave disadvantages, and a strictly logical foundation can hardly be claimed for it.

The divorce between Zoology and Botany in particular has been a serious hindrance to the development of Philosophical Biology, which in many seats of learning has been allowed to fall helplessly between the two stools, while undue stress has been laid upon the descriptive and systematic aspects of the subject. Similarly, the anatomists and physiologists of our medical schools have been seriously handicapped by their failure to recognise fully that the structure and functions of the human body can be adequately understood only in the light of the evolutionary history of man as indicated by the structure and functions of the lower animals.

We have not, even yet, fully appreciated the essential unity of the whole of the organic world and the close interrelationship and interdependence of all the many subdivisions of biological science. Unless we recognise this fundamental truth, however much we may add to our accumulations of detailed knowledge, we shall make little progress in our philosophical interpretation of the laws of life, and shall encounter many unnecessary difficulties in endeavouring to apply our knowledge for the material and intellectual welfare of mankind.

It is, I take it, one of the main functions of our universities to secure the adequate representation, in their curricula and in their schools of research, of all departments of learning, though some doubt may be entertained as to whether the applications of science to human needs might not be more appropriately dealt with in special schools of technology. To this point I hope to return later on. It is no less important that within each of the main categories the subject-matter should be properly classified with a view to securing adequate treatment of all its aspects and the harmonious development of the whole.

CLASSIFICATION OF THE BIOLOGICAL SCIENCES

Pure Biology

A. Descriptive and Systematic

- Morphology
  - Anatomy, Histology, Cytology
- Embryology
- Palæontology
- Systematic Zoology, Anthropology, etc.
- Systematic Botany, Bacteriology, etc.
- Biogeography

B. Experimental

- Physiology
  - General (Animals and Plants)
  - Biochemistry, Biophysics
  - Physiology of Special Functions
  - Experimental Psychology and Animal Behaviour
- Experimental Morphology and Embryology
- Genetics
- Biometrics

C. Philosophical

- Theory of Organic Evolution
- Theory of Heredity
- Psychology, Metaphysics
- Sociology, History, etc.

Applied Biology

Medicine, Surgery, Parasitology,
Plant and Animal Breeding, Forestry,
Agriculture, Horticulture, Fisheries, etc.

BIOLOGY

The scheme of classification of the Biological Sciences which I have adopted for this occasion makes no attempt at completeness of detail. In drawing it up I have been guided primarily by what appears to me to be the logical course of study which a student should pursue in an ideal curriculum. It will be seen at once that the customary distinction into Zoology and Botany as primary divisions finds no place in the schedule, for the object of such a curriculum would be to produce neither botanists nor zoologists but, what is far more important, biologists. The distinction between Zoology and Botany would cut across our entire classification, and most of the subdivisions proposed could be treated, though greatly to their detriment, exclusively from the botanical or from the zoological standpoint. Specialisation on the botanical or zoological side, like specialisation in human anatomy and physiology, or in any other of the numerous departments, should come at a later stage. It is not pretended, however, that the scheme as it stands would be suitable for an actual curriculum in any existing university. It is meant rather to give a bird's-eye view, and to indicate the nature and scope of the subject-matter that would have to be dealt with in an institute or department devoted to the study of biological science, in order that the subject might be developed as an organic whole rather than as a series of lop-sided excrescences. It is also hoped that the scheme may serve as a suitable framework upon which to hang a few remarks on recent developments in various departments, though in this respect I shall of course be obliged to confine myself to those portions of the subject in which I may fairly claim to take a special interest.

You will see from the chart on p. 116 that the primary division proposed, so far as Pure Biology is concerned, is threefold, into (1) Descriptive and Systematic Biology, (2) Experimental Biology, and (3) Philosophical Biology. Descriptive and Systematic Biology stands first because it deals with the description and classification of the innumerable organisms which constitute our material, and thus affords the necessary foundation for both the other departments. The experimental biologist cannot carry on his work until he knows what material he has to work with. The value of his results depends very largely upon the correct description and identification of the plants or animals which form the subjects of his experiments, and very often upon a knowledge of the relationship of these organisms to one another. The philosophical biologist, again, if his work is to have any value at all, must base his theories upon the facts supplied by the describer, the systematist, and the experimenter, and thus secure the ultimate unification of the whole science.

The description and classification of plants and animals has always formed one of the chief interests of mankind, and it is significant that one of the first intellectual performances ascribed to Adam was an attempt at zoological nomenclature. Eve, apparently, was not considered qualified to assist at this important inauguration of the biological sciences. It is a far cry from Adam, or the primitive race of mankind which he may be supposed to typify, to the present day, but his descendants have never ceased to interest themselves in the same subject.

With the gradual increase of our knowledge our studies under this first main heading have naturally subdivided themselves, until, at the present day, we have come to recognise the following branches, each regarded more or less as a separate science by its own highly specialised votaries.

Morphology — really indistinguishable from Anatomy, though perhaps a little wider in its scope —deals with the form and structure of the organism, and describes the mechanism upon which the life of the organism depends. It has, of course, its Zoological and its Botanical aspects, and because of the immense number and variety of living things is a subject of inexhaustible interest. Morphological studies can only bear their full fruits when they are undertaken—as Comparative Anatomy—from the evolutionary point of view. The purely empirical study of a single isolated organism, like the human body, would be almost meaningless to the philosophical biologist—as meaningless as the study of a brief period of history without any reference to its antecedents. With the improvement of our methods—above all with the advent of the microscope and the elaboration of microscopical technique—our morphological studies are becoming more and more minute and exhaustive, and it has become necessary to recognise two subordinate branches dependent entirely upon microscopical investigation. These are Histology, which deals with the tissues of which the organs of the body are built up, and Cytology, which deals with the cells of which the tissues in their turn are composed. It is in the domain of Cytology that much of the most important progress in biological science has lately taken place. The cell is the unit of organic structure, and the study of cells—even if undertaken exclusively from the morphological standpoint—furnishes more than enough occupation for the lifetime of a highly-trained specialist. Each one of the many thousand different kinds of cells already known to us has a minute structure complex beyond our powers of analysis, and it is upon this structure that that of the entire organism depends.

The study of the microscopical structure of the germ-cells has given us our first real insight into the nature of the mechanism whereby individual peculiarities are handed on from parent to offspring, and Cytology has thus become a necessary adjunct to the study of Heredity. In this connection we cannot but be struck with the fineness of the texture of the living organism. So fine is it that our most powerful microscopes and most accurate methods of micro-chemical analysis are inadequate to reveal the presence of organic entities whose existence is fully established by indirect methods. I may refer more particularly to the so-called Mendelian factors, whose material existence in the chromosomes of the nucleus has recently been so ably demonstrated by Professor Morgan and his colleagues in America. These invisible structural units may perhaps be almost of the same order of magnitude as the molecules of the physicist, and their investigation brings us very near to the border-line between the biological and physical sciences. It is in this direction, I believe, that some of the most important and far-reaching developments of Biology are to be expected in the near future, but in saying so much I fear that I have already encroached upon the domain of another department of our subject, that of Biochemistry and Biophysics.

The study of Embryology, or form in the making, which deals with the development of the individual organism from the unicellular egg, has occupied a large share of the attention of biologists during the past century. To it we owe the great generalisation known as the Recapitulation Hypothesis, which has thrown a flood of light upon the problems of organic evolution and done much to facilitate a rational classification of plants and animals. The classical case is that of the Ascidians or sea-squirts. These highly degenerate animals used to be regarded as invertebrates and placed near the shellfish or Mollusca, until the brilliant investigations of the Russian embryologist Kowalesky showed that up to a certain point their development follows upon typically chordate lines, marked by the appearance of notochord, central nervous system, and gill-slits exactly as in higher vertebrates. In short, they pass through an active chordate stage in their life-history, after which they settle down to an inactive and sedentary life and lose almost all traces of their aristocratic ancestry.

Embryology has long been regarded as the final court of appeal in the interpretation of anatomical structure, and though the celebrated germ-layer theory has fallen somewhat into disrepute of late, there can be little doubt that the exceptional modes of origin of certain organs in particular cases, anomalous as they may seem, do little more than emphasise the general applicability of the theory. We may still hold that, speaking generally, the epidermis and nervous system, together with the most essential parts of the organs of special sense, arise from the outer layer or epiblast of the embryo; the lining epithelium of the alimentary canal and its outgrowths from the inner layer or hypoblast; the skeletal, connective-tissue, muscular, vascular, and excretory systems from the middle layer or mesoblast; and that these three layers are homologous or morphologically equivalent throughout the greater part of the animal kingdom. In short, a knowledge of Embryology must still be regarded as an indispensable part of the equipment of the student of Comparative Anatomy and of Organic Evolution.

If this be true of Embryology, still more so is it of Palæontology, which brings us into close touch with the actual record of the past history of the organic world as laid down and preserved in the stratified rocks which make up so large a portion of the crust of the earth. The investigations of geologists and palæontologists have accomplished marvellous results in the last few decades. We have long known that the geological record as a whole is entirely in accordance with the theory of organic evolution. Geologists and physicists are now prepared to grant us ample time for the long and tedious processes which the evolution theory postulates. It is, of course, impossible, from the nature of the case, to arrive at anything approaching exactitude in our estimates, which vary, say, from 100 to 1000 million years or more, but a few million years more or less hardly count nowadays; all that we need concern ourselves about is that there should be time enough, and we have no longer any time-rationing to fear.

The great advances which Palæontology has made in recent years consist not so much in the discovery of new types of extinct plants and animals as in the linking together of previously known types in less and less discontinuous series.

Perhaps the most striking instance is the almost complete elucidation, chiefly in America, of the ancestry of the horse. Similarly, but less completely, the whales and the elephants have been traced back to remote forebears utterly unlike their existing selves. The ancestry of man still remains a much-vexed question, but, after all, it is only matters of detail that are involved in the discussion. Whether, with Huxley, we derive the human stock directly from some ape-like form, or, with Wood Jones, trace it further back to the lemur-like Tarsius before effecting a junction with any other line of descent, makes no difference to the belief, universally accepted by all who are qualified to express an opinion, that man is but the final product, in one particular direction, of the process of organic evolution. The most reactionary theologian would hardly venture to challenge the conclusion that my friend who speaks nothing but Italian came from Italy, even if I had first believed that he travelled by steamer through the Mediterranean and across the Bay of Biscay and afterwards thought it more probable that he came by rail through France and crossed from Calais to Dover. Yet this is exactly the sort of criticism that is even now being levelled at the so-called Darwinian theory by people who are unable to keep pace with the intellectual advance of their generation.

We can safely trust the labours of the palæontologists to fill in more and more completely the gaps in our knowledge, and enable us to reconstruct in all important details the great tree of organic life. Already their work has proved, beyond the possibility of reasonable dispute, that the theory of organic evolution is indeed founded upon the rocks.

That department of Biology to which the term Systematic is more especially applied deals with the classification of plants and animals, and evidently overlaps all the other departments already mentioned, for the systematist takes cognisance of extinct as well as of living organisms, and bases his conclusions on the observed facts of morphology, including even the microscopical structure of tissues and cells. He classifies his material in accordance with the mutual resemblances and differences exhibited by individuals, and the more detailed his investigation of their structure the more satisfactory will his classification be. His object, however, is something more than the mere naming and recognition of species, genera, orders, and so forth, for what may be called museum purposes. He is, or should be, an evolutionist, for he also aims at reconstructing the great tree of life by tracing all the phylogenetic relationships of the organisms which he studies. The successful systematist, whether in Zoology or Botany, must, from the nature of the case, be a specialist. The number of species of plants and animals, and the literature pertaining thereto, are so vast that no one human intelligence could deal with them all. The day of Linnæus has long gone by.

Thus we have our Protozoologists, our Mycologists, our Spongologists, our Malacologists, our Entomologists, our Ornithologists, our Anthropologists, and so forth, and the most appropriate sphere for the labours of most of these is clearly that afforded by the great museums, for here only is it possible to accumulate the collections and libraries which systematic work requires. It would be a great misfortune, however, if work of this kind were to be altogether divorced from university teaching and research, for those who know nothing of it can have no proper insight into the endless diversity of the workings of nature or into the true meaning of organic evolution. The universities must co-operate with the museums, and I may take this opportunity of expressing my satisfaction that here in London such co-operation forms a vital part of our zoological organisation, as exemplified by the welcome presence of distinguished representatives of the Natural History Department of the British Museum on the University Board of Studies in Zoology.

Great as have been the labours of the systematist in the past, they will have to be greater still in the future if he is to cope with the material so bountifully provided by nature. The number of different kinds of plants and animals already named and described runs into millions, and each new exploring expedition adds to the list. It is still far easier to discover new species than to find systematists willing and able to describe and classify them. As a consequence of this state of things much of the work in this department has been left in the hands of amateurs or of inadequately trained zoologists, whose lack of insight and experience has often produced disastrous results and only added to the difficulties of their fellow-workers. Some kind of museum training is almost essential for successful systematic work, the importance of which is equal to that of any other department of Biology.

Another thing that is essential for the development of Systematic Zoology and Botany is international co-operation. We have this to a certain extent already, but I should like to see something much more thoroughgoing. I should like to see an international organisation for the systematic enumeration and description of the fauna and flora of the entire globe—something like the international organisation which is now engaged in cataloguing and charting the stars, but I fear the different species of plants and animals might prove to be, if not as numerous as the individual stars, vastly more difficult to describe and arrange.

> For much more eath to tell the starres on hy,
> Albe they endlesse seeme in estimation,
> Then to recount the seas posterity :
> So fertile be the flouds in generation,
> So huge their numbers, and so numberlesse their nation.

It is strange indeed that an Elizabethan poet should have expressed so clearly the difficulties of the systematic zoologist of to-day, for we may suppose that Spenser was thinking not merely of individuals, but also of kinds, or tribes ; what now, for want of a better name, we call ‘ species.’ How these species should be defined, and how they have arisen in the course of evolution, is still one of the major problems of Biology and one for which it is peculiarly the duty of the systematist to find a solution.

The next subject in our scheme of classification is the Geographical Distribution of living organisms, or Biogeography, in connection with which we again find a copious literature and a high degree of specialisation. The interest attached to this subject is twofold. On the one hand the distribution of plants and animals over the face of the earth throws considerable light on the geography of the past, and on the other it affords much valuable evidence with regard to organic evolution. The latter point of view is the one which alone concerns us now, and the keynote of the whole argument is supplied by the phrase 'adaptive radiation,' which has lately come so much into vogue amongst writers on the subject. This phrase expresses the broad conclusion that the great groups, of the animal kingdom at least, have radiated from their original homes in various directions, adapting themselves in the course of their migrations to new conditions of life, and thus giving rise to new types. No conclusion could agree more competely with the theory of organic evolution.

We come now to the second of our main categories, Experimental Biology, and here indeed we have a vast field before us. It is often said that Biology is becoming an experimental science, and though this is a comparatively recent development it has already proved surprisingly fertile in results. Inasmuch as you may experiment in any way with any organism, or even with any part of an organism, it is evident that there is no limit to the subject. One man may spend his entire life in breeding flies and observing the endless permutations and combinations of minute differentiating characters which make their appearance from time to time in his cultures. Another might occupy himself in observing the effects of drugs of all kinds applied to as many animals as he could lay his hands on. In either case, the importance of the results obtained would depend upon their systematisation, and their applicability to the solution of problems of general interest. As in other departments of science, much time and effort may be wasted for want of proper co-ordination amongst the workers and judicious selection of the experiments with a view to the solution of specific problems. Merely haphazard and aimless work is of little value. One might spend a lifetime in measuring and weighing the pebbles on the seashore, but the results attained would hardly deserve to be called scientific, although they might constitute a perfectly truthful record of observed facts.

For convenience of treatment in our imaginary Institute of Biological Science, I should be inclined to subdivide this portion of the subject into (1) Physiology, (2) Experimental Morphology and Embryology, (3) Genetics, and (4) Biometrics.

Physiology, the study of function as contrasted with that of structure, has long been recognised as of primary importance in our university curricula. Although usually regarded as a distinct science, it clearly overlaps almost every other department of Biology. Thus it cannot by any possibility be divorced from Anatomy, and it plays a fundamentally important part in the theory of Heredity. It is perhaps unfortunate that our interest in ourselves as human beings has resulted in the concentration of attention upon the functions of the human body, almost to the exclusion of the lower animals, so that the development of this branch of Biology has been a very lop-sided growth. This will no doubt be corrected in the future, with much benefit to all concerned. Already the botanists have set the example and made great progress in their study of the physiology of plants.

Physiology, then, is an enormous subject, and still requires subdivision before it can be adequately treated. I must not forget that my distinguished colleague, Professor Halliburton, will speak with much greater authority later on, but perhaps I may be allowed to say a few words on this matter, if only from the point of view of an interested surveyor of the field who has to content himself with looking over the fence.

The general Physiology of Plants and Animals would deal with the complete vital activities of individuals regarded each as a working whole; the interrelationships of the different functions and their co-ordination in the maintenance of life. The subject would have to be treated from a comparative point of view, for the correct understanding of function requires the illumination of the evolutionary hypothesis almost as much as does the interpretation of structure.

Biochemistry and Biophysics are those branches of Physiology which form the connecting links with the sister sciences and bring the living organism into immediate contact, so to speak, with the inanimate world. They say the last word in the analysis of function from the purely mechanistic standpoint, but they have not yet supplied us with the solution of the riddle of life.

The Physiology of the Special Functions, such as Digestion, Respiration, Excretion, Locomotion, and so forth, would probably have to receive special treatment if only on account of the vastness of the subject-matter, just as the physiology of plants will probably always be treated as distinct from that of animals. These are matters of convenience rather than of logical arrangement.

Experimental Psychology, including what is commonly called the Behaviour of Animals, is a branch of Physiology which is at present very much in evidence. It forms one of the chief battle-fields upon which the incessant and wearisome conflicts between the 'mechanists' and the 'vitalists' are carried on. As Professor H. S. Jennings says, "The chief interest of the subject of the behaviour of animals undoubtedly lies, for most, in its relation to the development of psychic behaviour, as shown by man. The behaviour of the lowest organisms must form a fundamental part of comparative psychology." Jennings' admirable book on this subject illustrates very well the kind of work that is being done in this field, and his attempt to trace the function of intelligence right down to the unicellular organisms is full of interest.

As examples of the extreme mechanistic school we may mention the voluminous writings of the distinguished experimentalist Loeb, who, if I understand him rightly, would interpret the most complex actions even of the highest animals as mere reflexes, of exactly the same nature as the tropisms and tactisms of the lower forms of life and equally explicable as purely mechanical effects of stimulation. The well-known work of Hans Driesch, on the other hand, is frankly vitalistic in its interpretations, but we must leave the discussion of his "Entelechy," along with that of Bergson's "Élan vital," to the philosophers.

The study of animal behaviour merges almost imperceptibly into Experimental Morphology and Embryology. In this direction a wide prospect has been opened up during the last few decades, and one which has attracted many of our ablest biologists. Some of the earliest experiments in this field were those on the regeneration of lost parts, whereby a mutilated organism is able to make itself whole again. Developed to a surprising extent both amongst plants and amongst the lower animals, this power gradually diminishes as we ascend the vertebrate series, until in man, unfortunately, it almost reaches a vanishing point. Even amongst the lower vertebrates, however, limbs may be removed and regenerated almost *ad libitum*, and the experimental regeneration of the lens of the newt's eye from the margin of the iris seems to defy any purely mechanistic explanation. Such experiments, taken in connection with similar ones performed upon developing embryos, show conclusively that the living organism has a very remarkable power of self-regulation, and that the whole is something much more than the mere sum of its parts. The laws which govern these regulatory processes are at present very little understood, and the results obtained experimentally often appear at first sight to be curiously capricious. Thus, if you remove the eyestalk and eye of a crayfish by cutting it off at one level, another eye and eyestalk will grow in its place, but if you cut it through at another level a feeler will develop instead. This is taken by some to indicate that the eyestalk is really a modified biramose appendage which has undergone change of function in the course of evolution, but this does not help us to understand the mechanism of the mysterious substitution.

Closely akin to these experiments are those on transplantation and grafting of organs. One of the most remarkable of these again concerns the vertebrate eye. It has been shown in certain cases that if the outgrowth of the embryonic brain known as the optic vesicle, and destined to give rise to the retina, be removed from its normal position and inserted beneath the skin on some other part of the head, its presence there will cause the development of a lens in the new situation, just as the lens develops from the superficial layer of the skin opposite to the vesicle in the normal eye. This experiment demonstrates in a very beautiful manner the correlation which exists between the development of different organs of the body. Evidently the appearance of the lens depends upon the presence of the optic vesicle, and it is probably initiated by a stimulus exerted by some chemical substance secreted by the latter. In this field again we find abundant material for the logomachies of mechanists and vitalists.

The student of Genetics, which may be defined for our purposes as the experimental study of heredity, concerns himself with the unravelling of the mysterious connection which exists between parent and offspring. Since the opening of the present century this department of our science has been dominated almost exclusively by the Mendelian school, who are developing in every direction, and in the most thorough manner, the line of investigation initiated by the Abbé Mendel himself half a century earlier.

The knowledge that new organic types, differing in one or more respects from any of their ancestors, may arise from the crossing of different varieties, and may, in certain cases, propagate their own peculiarities, has not only been very fruitful in results of practical importance, but has rendered possible a minute analysis of the heritable constitution, and shown how this may be resolved into factors capable both of being separately transmitted from one generation to the next and of forming permutations and combinations with one another in accordance with the laws of chance.

The factors whose existence is demanded by the geneticist to explain the results of his breeding experiments have not as yet been identified as visible entities in the germ-cells, but, as I have already indicated, the American school, under the leadership of Thomas Hunt Morgan, have localised them with a high degree of probability—if not of certainty—in the chromosomes of the nucleus, and even devised a means of mapping out their distribution therein. It is here that the work of the cytologists and that of the geneticists comes so closely into touch, for our recently acquired knowledge of the behaviour of the chromosomes in cell-division, and especially in the maturation of the germ-cells, and in the process of fertilisation, supplies exactly the mechanism for the permutation and combination of factors required by the Mendelian investigator.

Arresting and important as these results undoubtedly are, however, we must be on our guard against supposing that the factorial hypothesis can ever provide a complete solution of the problem of heredity. A great deal of nonsense has been talked about organisms being built up entirely of separately heritable unit characters, and about all species having arisen through the crossing or hybridisation of pre-existing forms. The truth appears to be that the Mendelian factors, after all, are only modifying agents. There are probably no special factors, for example, that determine whether a normal human being shall have a nose or no nose, but there may be factors which determine whether his nose shall be of one particular shape or another, just as there appear to be factors which determine the colour of his eyes.

The common-sense view of heredity teaches us that an organism resembles its parents because it commences its existence with a certain stock of body-forming substance similar to that with which its parents commenced, and because this material is subjected during its development to a series of stimuli similar to those which influenced the development of the parents. Like causes produce like effects, and two series of developmental events, starting with similar material and taking place under similar conditions, must lead to similar results. The offspring *must* resemble the parent. If there be different modifying factors, Mendelian or otherwise, present in the two cases, the results must differ accordingly, and we know from abundant experience that such is actually the case.

What may be the nature of the so-called Mendelian factors which modify the development of an organism in so many respects we do not know, but we may fairly suspect them to be chemical in their mode of action. We know that almost infinitesimal quantities of certain chemical substances may produce profound effects upon the living organism. The embryos of the fish Fundulus react towards the presence of magnesium chloride by developing a single eye in the middle of the head instead of one on each side. The mechanism of the process is as yet entirely unknown, but it is a perfectly definite reaction to a perfectly definite chemical stimulation.

We are here brought face to face again with our Biochemistry, and in this connection I should like to give one more illustration of the close interdependence of the different branches of our subject. The problem of the inheritance or non-inheritance of somatogenic or so-called acquired characters has long been one of the burning questions of Biology. It has given rise to endless scholastic argument and interminable hair-splitting, the one school occupying itself in producing 'conclusive' evidence and the other in demolishing it. The difficulty in devising and carrying out really critical experiments has so far proved almost insurmountable, but quite recently an entirely new way of attacking the problem has been opened up by the experiments of two American workers, Messrs Guyer and Smith. These ingenious investigators base their operations upon our recently acquired knowledge of the marvellous properties of the blood in its reactions against the intrusion of foreign albuminoids. The study of the blood from this point of view has developed almost into a new science, which perhaps might have been included in our schedule as a separate category, but which is really a part of Biochemistry and Biophysics. Thus the injection into the blood-stream of the red corpuscles, or of the spermatozoa, of a distinct species of animal causes the appearance in the blood of an 'antibody' or 'lysin' which has the property of dissolving and destroying the foreign substance. The modern theory and practice of immunisation depend primarily upon this remarkable reaction.

Guyer and Smith, by injecting an emulsion composed of the lenses of rabbits' eyes pounded up in normal salt solution into the blood of fowls, produced a 'lens-sensitised' serum capable of dissolving the lens-substance. This serum was then injected into the blood of pregnant rabbits, and it was found that the young animals were born with defective lenses. The result, so far, was of course merely the appearance of a bodily or somatogenic character in response to a definite chemical stimulus. But the experiments went much farther than this, for it was found that the eye-defect produced thus artificially in the young rabbits was inherited for at least six generations, and actually became more and more strongly marked as time went on. Moreover, it was shown that the defect could be inherited through the male parent alone, thus precluding all possibility of intra-uterine affection.

These wonderful results have not yet been subjected to the full flood of destructive criticism which is sure to be poured out upon them by the scholiasts, but the work was done with so much care, and the evidence is so plain and straightforward, that it is difficult to see how they can be refuted.

The next heading in our schedule is Biometrics, which is the application of statistical methods to biological problems. We owe the initiation of this line of investigation in great measure to the late Sir Francis Galton, and his work is being assiduously followed up by the British school of biometricians under the leadership of Professor Karl Pearson. Unfortunately there appears to be what almost amounts to a congenital incompatibility of temperament between the mathematical and the biological faculties. It is rarely indeed that the two are fruitfully united in one individual, and perhaps that explains why so many biologists are apt to look a little askance at Biometrics. Possibly, also, the data with which the biometrician works are themselves too complex and too lacking in precision to yield their full quota of value to this method of treatment. Doubtless many important results have been obtained, as in the case of Johannsen's work on pure lines and the attempt to distinguish sharply between mutations and fluctuating variations connected therewith, and the method is one which may be fruitful enough in cautious and experienced hands. It is not, however, for one who has no mathematical capacity to speak of it, and in any case space does not permit of more than a passing reference.

We come at length to the last of our three main categories—Philosophical Biology, and here we can afford to be very brief, for we have already trespassed upon this field repeatedly. The chief content of this division of our subject is undoubtedly the Theory of Organic Evolution. It is safe to say, as I have already indicated, that this theory stands more firmly grounded to-day than at any time in its past history, and that it is still the most fruitful source of inspiration for the biological investigator. The evidence for the evolution of the whole organic world in a tree-like manner from comparatively simple beginnings is conclusive, though the *modus operandi* remains much in dispute. Natural selection, though no one doubts its general efficiency as a controlling agent, is no longer regarded by most biologists as the universal explanation that some of Darwin's immediate followers tried to make of it. Weismannism has almost sunk beneath the weight of its cumbersome machinery. Lamarckism, on the other hand, has long been tentatively lifting its head above the troubled waters, and if once the difficulty about the inheritance of acquired characters can be satisfactorily disposed of we may expect to see the great French philosopher at length coming into his own.

In one direction the theory of Organic Evolution has made noteworthy progress in recent years, and that is in connection with the much disputed problem of the origin of living things. The curious attempts to people the earth by means of casual immigrants from other planets, either carried by meteorites or in the form of ultramicroscopic 'cosmozoa' propelled hither by the *vis a tergo* of the sun's rays, are being superseded by the idea of extending backwards the process of evolution from the organic to the inorganic world without any break of continuity. The study of the chemical evolution of the carbon compounds, and the possibilities of their synthesis in the laboratory, have done much to encourage the new suggestion, which certainly seems far more rational and consistent than any other so far presented to us. It is true that we are thrown back in a certain sense upon the discredited theory of spontaneous generation, but it is a spontaneous generation of quite a respectable character. We are not asked to believe in the sudden conversion of lifeless matter into living organisms, however simple these may be, but in the gradual evolution of the organic from the inorganic; as the simplest protoplasmic stage was reached we may suppose that the organic matter gradually entered into new relations, or energy-exchanges, with its environment, and that these actions and reactions constituted the life of the organism. At this stage it is for the metaphysician to carry on, though perhaps even the humble biologist may be allowed to interpret the living organism not only as an elaborate machine, nor merely as the outcome of a long process of evolution, but as a being which stands on a higher plane and has a deeper meaning for the structure of the universe than its inanimate surroundings.

Although my original intention was to speak only of Pure Biology, I feel that I can hardly take leave of my subject without saying a few words on the practical applications thereof. How far applied science should form part of a university curriculum is, as I have already hinted, a very debatable question, but all are agreed that pure and applied science must remain in close organic connection with one another. I have often thought how delightful it would be if we could have an institution or a society devoted entirely to the encouragement of useless investigations, but I fear that is an ideal which will never be attained in this utilitarian world.

When I say 'useless,' of course you will understand that I am being guilty of a terminological inexactitude. I ought to say, investigations that appear useless to those who know no better.

Let me give you just one example. We are told by Professor Hermann Müller that it was not until the close of the eighteenth century that the true purport and significance of flowers began to be perceived. Christian Conrad Sprengel then laid the foundations of our knowledge concerning the processes of fertilisation, and pointed out the important part played by insects in carrying pollen from one flower to another. Now Charles Darwin, in one of his classical works, tells us that he heard from a New Zealand correspondent that the clover in that country never seeded because there were no humble-bees to carry the pollen. When I was in New Zealand some twenty years ago, however, I was informed on excellent authority that the clover-seed raised in the province of Canterbury alone was then worth £30,000 a year to the farmers. This was because, in the meantime, a private individual had, at his own expense, introduced humble-bees into the country. Sprengel's discoveries in Pure Biology had indeed fructified abundantly.

At the present day we owe more than we can possibly realise to the applied sciences that draw their inspiration from Pure Biology. I need only mention Medicine, Surgery, Parasitology, Forestry, Agriculture, Plant and Animal Breeding, Horticulture, and Fisheries.

We must not forget, however, that the debt is to some extent mutual, and that the human anatomists and physiologists, for example, may justly claim that it was the mainly utilitarian study of their subjects from the medical point of view that laid the foundations of Comparative Anatomy and Physiology, to say nothing of Botany and Zoology.

Nevertheless, pure science must be regarded as the root of the tree, and applied science as the fruit, or at least as that part of the fruit that is usually considered most worth gathering. Other fruits there are, and perhaps more palatable—but fortunately they are not marketable. It is not they that appeal to the holders of the purse-strings; but these important personages would do well to realise that you cannot grow apple-trees in windowboxes; that if you starve the roots your crop of fruit will not be forthcoming.

There ought, of course, to be no competition at all, but the closest co-operation, between pure and applied science. Unfortunately, however, competition is hardly to be avoided so long as those in authority value results only in so far as they can be shown to be immediately applicable to the material welfare of the nation; and it is just this danger of a competition in which market value would always decide the issue which makes it perhaps questionable whether pure and applied science can both attain their fullest development in the same institution. On the other hand, the danger attending the practice of lopping off branches from the tree of knowledge as soon as they begin to bear fruit, and subjecting them to intensive cultivation under conditions of comparative isolation, is no less threatening at the present time.

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VI

BOTANY

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VI

BOTANY

THE end of the nineteenth century marked something more than a formal turning-point in the history of science. The dawn of the twentieth century was ushered in with a real renaissance in scientific discovery, both in the biological and the physical sciences. In the latter part of the Victorian period scientific research seemed to have reached a point at which all the main outlines of the picture had been sketched, and it only remained to fill in the details. In physics, as Professor Richardson has reminded us, it was suggested that future discoveries were to be looked for in the third place of decimals, with the inference that their importance in the scheme of things was to be gauged by the minuteness of their size. In biology, the Darwinian conceptions of natural selection and adaptation were regarded as the last word in evolution, and naturalists were busily occupied in showing to their satisfaction how all the known cases of adaptation and differentiation of species could be accounted for on the principles enunciated nearly forty years earlier by Darwin.

True, there were occasional disquieting indications that this delightful condition of complacent finality in science could not be permanent. One of these was the discovery of Röntgen rays in 1895, quickly followed by the beginning of the radium work by Becquerel, Rutherford, and others. These discoveries were destined in less than a decade to shake physics to its foundations and open up new vistas in science in all directions, affecting also the biological sciences in many ways. Similarly in biology; as early as 1894, Bateson, in his *Materials for the Study of Variation*, indicated that perhaps all was not well with the methods then in vogue in the investigation of evolutionary and phylogenetic problems. But the time was not yet ripe for a contribution which would lead not only to the criticism and limitation of the older comparative morphological method as the only available method of research in biology, but also to the adoption and development of the new method of experimental breeding. From our present point of view it is possible to see that both these methods have their advantages and their limitations, while they derive mutual benefit from co-ordination.

This subject will be pursued farther at a later stage in this paper, but I wish first briefly to survey, as far as time will permit, certain aspects of the progress in botanical research during the last two decades. The development of botanical science during this period has certainly been a phenomenal one, and he who aspires to have even a speaking acquaintance with its many modern sides must be a man of extraordinary activity and industry. I shall therefore venture to touch upon only a few of the striking discoveries in botanical science which seem to stand out as landmarks in the work of the last two decades, and I shall also refer briefly to some of the applications of botany which have transformed the subject from a largely academic science into one of the great instruments of benefit to mankind.

Palæobotany

Although palæobotany touches upon industrial problems in connection with coal, its main interest to the botanist is in connection with the light it throws on the evolution of the plant kingdom. The records of the rocks reveal transitional groups and generalised forms which throw a flood of light on the origin, relationships, and descent of the great plant-groups which have survived down to the present time. One of the most striking contributions of palæobotany to plant-phylogeny has been the discovery of seeds attached to plants with fern-like habit and foliage in the coal-measures. Before making further reference to this discovery it is desirable to make a few remarks about the significance of the seed as an evolutionary structure.

One of the great achievements of evolution, both in plants and animals, has been the enclosure and protection of the young developing organism. Among primitive organisms, such as Algæ and many groups of invertebrate and even vertebrate animals, the reproductive cells are quite commonly discharged into the sea-water, where fertilisation occurs, and following this, the whole development takes place in an unprotected condition. Environmental fluctuations can do their worst in directly distorting or inhibiting the development of the young. The organism in the most immature and sensitive stages of its life-cycle is completely at the mercy of the elements, and it is not surprising that a very small percentage survive.

But the higher plants and animals show many steps in the development of special structures and conditions for the protection of their offspring during these early stages. The problem has been more or less successfully solved in a variety of ways. The great advantage of such devices to the species is a sufficient reason for their perpetuation and improvement as well as for the various stages of their production. In plants this has resulted in the seed, a structure which has apparently evolved in its main features independently in different phyla. The last step in the perfection of this process in animals is found in the mammals, in which practically the whole development takes place under the protection afforded within the body of the mother.

As Professor Oliver has said, "With the evolution of the seed, the plant rose at a bound to a higher plane, and this structure in its perfected form has become the very focus of the plant's existence." Botanists recognise that the essential feature in the evolution of a seed was the retention of the megaspore within the sporangium after the number of functional megaspores had been reduced to one. An approach to this condition is to be seen in the living Lycopod genus Selaginella, where the number of megaspores is reduced to a single tetrad of four, which germinate within the megasporangium. In the Palæozoic Lycopod Miadesmia this process went a stage farther ; the single megaspore is retained in its sporangium, which is in turn protected by an integumental covering leaving only the apex exposed at the micropyle. In a related form, Lepidocarpon, a seed of similar type is found, with one functional megaspore and three abortive ones, and Dr Scott has shown that as this seed develops an integument grows up as a new structure and surrounds the sporangium.

From the point of view of protection to the developing embryo, the condition in Selaginella, in which the female gametophyte develops within the megaspore, only rupturing its wall when nearing readiness for fertilisation, might be compared with the eggs of birds and some reptiles, the calcareous shell of which protects the embryo in all but the latest stages of development. Furthermore, it is by no means fanciful to see a parallel as regards protection between the Gymnosperms and Angiosperms among plants on the one hand, and the Marsupials and higher Mammals on the other. Just as the Gymnosperms are characterised by open carpels, usually in a cone, while the Angiosperms have closed carpels affording an additional protection to the young embryo, so the Marsupials, the later development of whose young takes place in an open pouch, were supplanted by the higher Mammals, in which the complete protection of the mother's body is afforded until all the critical stages of development are passed through.

Returning now to the palæozoic seeds, whose investigation has been of so much importance in recent palæobotany, Williamson as early as 1875 described fossil seeds from the Lower Carboniferous of Lancashire. But only since the beginning of the present century has it been shown that these seeds belonged to plants having the habit, foliage, and essentially the male reproductive organs of ferns. In 1903 Oliver and Scott showed that the seed known as *Lagenostoma Lomaxi* belonged to the fern-like plant *Lyginodendron Oldhamium*, and founded the group of Pteridosperms for palæozoic fern-like plants bearing seeds. Subsequent studies, notably by Professor Oliver and his pupils, have shown that nearly all the supposed ferns of that period really bore seeds of a variety of types, the detailed anatomical investigation of which has thrown a flood of light on these early stages in the evolution of the seed. Although much yet remains to be done, this important chapter in the history of palæobotany has shown that terrestrial plants responded to the need, or at any rate benefited from the advantages of protection of their embryos by developing various types of seed-structures at a very remote period.

A still more recent development in the study of fossil plants, and one which is perhaps of even wider significance, is in relation to the still more ancient Devonian flora. Important as is the development of the seed habit, the transitional stages by which Algæ gave rise to land-plants is an even more informing chapter in evolution. How sea-weeds, living in a dense water-medium and relying partly upon buoyancy, requiring little differentiation of tissue to give them rigidity or for purposes of conduction (since their nourishment could be absorbed at any point), and showing relatively little differentiation in the functions of parts, could develop into land-organisms with roots in the soil, more or less rigid stems to hold them upright against the stresses of an aerial environment, and leaves for assimilation, has remained until very recently a matter of speculation. It was supposed that the transition from water to land took place at such a remote epoch that direct evidence of how it occurred could hardly be expected. Bower, in his classical work *The Origin of a Land Flora* (1908), has considered the many phases of this problem from the point of view of comparative morphology.

But recent discoveries, chiefly by Kidston and Lang, from beautifully petrified material collected by my colleague Professor Gordon in the Old Red Sandstone of Rhynie, Scotland, have shown that fossil material bearing directly on this problem has long been at hand, but without the means for its adequate interpretation. Recent discoveries in Norway also bear directly on this question. The remains at Rhynie consist of a silicified peat deposit composed almost entirely of the stems and rhizomes of a plant called by these authors Rhynia, in which the structure is almost perfectly preserved. The stems sometimes showed dichotomous (forked) branching, but they bore neither roots nor leaves. The rhizomes, however, bore primitive rhizoids, no doubt functioning like those of mosses, and the stems also had peculiar bulges or emergences which later evolution may have developed into leaves. The stems contained a simple conducting system, and bore large terminal sporangia of a remarkably primitive type, apparently formed directly from the stem-tip and without any mechanism for dehiscence. A related genus, Hornea, has since been described from the same locality, and another type, Asteroxylon, whose stems had essentially the vascular structure of a simple modern Lycopod. The remarkably generalised character of these plants may be judged from the fact that Rhynia has already been grouped by different botanists with the Thallophytes (Algæ), Bryophytes (Mosses), and Pteridophytes (Ferns), with all of which it seems to have relationships.

The importance of these discoveries stimulated Arber, shortly before his lamented death, to write a book on the Devonian floras in which he correlated all that was then known concerning Devonian plants in its bearing on our understanding of the earliest land-plants. He shows that Rhynia is probably none other than the petrified condition of Psilophyton, a plant described by Sir William Dawson in the form of impressions from the Lower Devonian of Canada as early as 1859. The scepticism which long surrounded these and many other rather obscure impressions described by Dawson, Penhallow, and others from the Devonian of Canada and Scotland is now largely dispelled; Sir William Dawson's early descriptions are seen to have been for the most part extremely accurate, and the great value of these early researches is now apparent.

From a consideration of all these forms, Arber concludes that there were three separate lines of evolution from thallophytic Algæ to cormophytic land-plants: (1) The Sphenopsida, derived from Algæ with branches in whorls and leading to such fossil forms as Sphenophyllum; (2) Pteropsida, from Algæ with large and numerous scattered branches, leading to the ferns and their descendants; and (3) Lycopsida, from Algæ with aerial axes rarely branched, but sometimes dichotomous, leading to the modern Lycopods and their allies. In this way a subject of fundamental theoretical interest, which was until very recently supposed to be a field only for speculation, has now become a matter of direct investigation, and we have before us for the first time the possibility of learning something of the actual steps by which plants emerged from the water and began their conquest of the land. The evidence at any rate, as Dr Scott says, "clearly supports the hypothesis that land-plants arose from highly organised Algæ of the sea," and that land-vegetation therefore is not descended from primitive terrestrial types, but from highly developed marine types, in general agreement with the views of Church.

Ecology

Another branch of botany which may be said to have taken its rise about the end of the last century is ecology, or the study of the sociology of plants as they jostle each other in their various habitats. This science grew out of plant-geography, and its general foundations were laid by Drude (1889) and Schimper (1898), who were essentially plant-geographers. In 1895 Warming published an ecological plant-geography in which laws of succession in the vegetation on a given area were recognised, and his *Œcology of Plants* was afterwards published at Oxford in 1909.

In America the end of the last century was marked by the pioneer studies of Cowles and Clements, the former on the vegetation of the great sand-dunes which border Lake Michigan, and the latter (with Pound) on the vegetation of Nebraska.

In America, where the vegetation covering great areas, but slightly modified by human interference, can be studied in its broad outlines, the relation of types of vegetation to climate and to succession has been most emphasised. In Britain the British Vegetation Committee, which was formed in 1904, led to a more detailed consideration of vegetational types in relation to soils and the various climatic factors such as temperature and rainfall. Its labours resulted in the *Types of British Vegetation*, edited by Tansley in 1911, in the International Phytogeographical Excursion through the British Isles in the same year, followed by another through the United States two years later, and in the founding of the *Journal of Ecology* in 1913. In the meantime ecology has tended to become a more exact science, using instruments for the determination of soil evaporation, transpiration of the plant, diurnal variations in sunlight, moisture in the air, rainfall, temperature, and various other factors that affect the plant in relation to its environment. Indeed, ecology in these aspects is now recognised as an essential branch of plant-physiology, for obviously a study of the reactions of the plant in its natural environment in association with its fellows will throw light upon many questions which could not be answered by laboratory experiments. This method of making the plant answer questions where it lives and grows is a most promising field for future research, bearing as it does on questions of ecology, physiology, variation, and evolution.

Microscopic Research

The microscope is, of course, one of the biologist's most valuable instruments of research, and in recent years it has been put to new uses. Valuable and essential as are the ordinary methods of fixing and staining for the investigation of structure, there are many other facts about protoplasmic structure which can only be determined in the living cell. Methods of micro-dissection of living cells under the highest powers of the microscope by means of an extremely fine glass needle carefully manipulated have been developed in recent years, and have led to results of the greatest interest. The work of Kite and Chambers, in particular, in the study of the physical properties of living protoplasm, nuclei, and chromosomes under these conditions has opened up a new field of research which promises to be of great value in conjunction with the usual methods.

Another new use of the microscope, in a field where the biologist meets the chemist, is in connection with colloid chemistry. By the use of the so-called ultra-microscope, the principle of which is the illumination of particles by a beam of light on a dark background, the nature of the various types of colloidal aggregation which lie at the base of protoplasmic structure and activity may be determined.

While on this theme I cannot refrain from mentioning the discovery in recent years of the existence of ultramicroscopic organisms, so minute as to be beyond the reach of our highest-powered microscopes, and whose presence can only be detected by the effects they produce. They pass through fine-grained filters which will stop bacteria, and the resulting virus can be shown to have multiplied by the effect it produces. One of the best known of the diseases produced by this type of organism is the foot-and-mouth disease in cattle. Poliomyelitis and infantile paralysis, as well as other diseases in man, have been shown to be due to such organisms, and the same is apparently the case with certain plant diseases, such as the mosaic diseases and the infectious chlorosis of Abutilon, though the latter is transmitted only by grafting. Until very recently, yellow fever was believed to be due to another ultramicroscopic organism, but Noguchi has found that this disease is produced by a spirochæte. In one stage, however, its virus passes through the finest porcelain filter, so it would appear that this must represent an ultramicroscopic reproductive stage in the life-cycle of the spirochæte organism. There are also indications that some of the bacteria have, in addition to their ordinary methods of reproduction, a more minute or gonidial stage which is also in some cases a filter-passer.

The existence of such organisms, far more minute than bacteria, exhibits the problem of the origin of life in an entirely new light. Some of these organisms can pass through filters which will stop others. They therefore differ in size among themselves, and it becomes a question what is the minimum size which a living organism can have and exhibit the phenomena of growth and reproduction. Experiments with these organisms, when methods of cultivating them have been developed, may lead us to new views of what constitutes life. The number of protein molecules in the smallest of them cannot be a large one, and here again we are reaching the borderland between the organic and the inorganic. The question even of spontaneous generation under these circumstances takes on entirely new aspects. To search for it in bacteria and torulæ is beside the mark, yet in this still more minute realm of the ultramicroscopic organisms or viruses the transition from the inorganic to the organic may conceivably be taking place even at the present time.

Practical Applications

But fascinating as are the scientific problems connected with life as it manifests itself in plants, botany in the last twenty years has also reached out into many realms of applied science. While there is not time to dwell upon these phases, I may mention the enormous and steady development of mycology in connection with the study of plant diseases, especially as they affect agricultural crops. The frequently complicated life-cycles and the relations of the parasite to the host-plant have now been investigated in detail in a great variety of fungi. In every country the trained mycologist has become a necessity as an ally of the farmer in combating fungus diseases. With the further development of tropical agriculture his services will become still more important, and the recent foundation of the Imperial Bureau of Mycology at Kew will be of immense service in acting as a clearing-house of information and centre for aid in connection with mycological work being carried out in all parts of the Empire. The enormous yearly losses not only to agricultural crops, such as wheat and other cereals, but even to many valuable forest and timber trees through the attacks of parasitic fungi, emphasises the important part which the mycologist must play in preserving and extending the food and timber resources of the world. An even more important method of dealing with these pests, by the production of resistant varieties, is exemplified by the work of Biffen and others in breeding rust-resistant wheats. The botanical problems connected with cotton- and rubber-production and a host of other economic applications are too many-sided to consider here, but it may be pointed out that there is a great need for adequately trained botanists who are capable of grappling with these problems as they arise. The rapid development of the applications of botany has left us in this country very inadequately equipped for the development of these aspects of applied research, and overseas the matter is still more urgent.

In these fields plant-pathology usually merges into the study of physiology, a field of botany in which the advances are so manifold that few of them can even be mentioned here. But we may cite as theoretical problems having a practical bearing such recent topics as the electro-culture of seeds and crops, the origin and action of vitamines and auximones and their relation to the plant organism, the symbiotic relation between fungal hyphæ and the roots or other organs of plants in such families as the heaths and the orchids. It is well to remember also that in the intensive study of any crop and its improvement, the problem ultimately becomes one in the broad aspects of its physiology, and its environmental and genetic relationships.

Genetics

Coming now to the subject of genetics, probably all will agree that this represents the field of the most intense activity in the whole realm of biology during the last twenty years. As a movement it dates from 1900, when Mendel's principles of heredity were simultaneously rediscovered by de Vries in Holland, Correns in Germany, and Tschermak in Austria.

The story of Mendel and his experiments with garden peas in the cloister garden at Brünn, in Moravia, is now almost a household tale; how by experimenting on the inheritance of single characters at a time, such as yellow or green, round or wrinkled peas, he discovered the fundamental principle that the differences were independently inherited, the determiners for each pair of characters segregating independently in the germ-cells; and how his discovery, having been made before its time and published in the obscure *Verhandelungen* of the Brünn Natural History Society in 1866, lay unrecognised until the principle was rediscovered at the end of the nineteenth century.

The effect on biology of this rediscovery, and of the publication of de Vries' *Mutation Theory*, which began in the following year, was dramatic in the extreme. If these results were true, then it appeared at the time that all the neo-Darwinian conceptions of evolution by the slow and gradual accumulation of imperceptibly small variations were swept away. Later developments have brought these results into a truer perspective in relation to Darwin's views, but the first impact of the new conception was sufficiently startling. Small wonder, then, if to a biologist taking up research in these first five years of the century, it seemed that to be young was very heaven. Biology received an impetus to new development such as physics had received some five years earlier. Under the influence of de Vries on the Continent and Bateson in this country, mutation and Mendelism became the watchwords of the hour.

To de Vries belongs the credit of introducing the method of experimental breeding into modern biology. The mutation theory of evolution, which postulated the sudden origin of new species and varieties, was based upon some twenty-five years' breeding experiments with evening primroses

(Œnothera) and other forms. From carefully controlled pedigree cultures de Vries showed, particularly in the Œnotheras, that new forms, differing in many characters from the parent species, suddenly arise as variations or mutations and perpetuate their peculiarities in later generations. The majority of these forms differed from the parent species of de Vries' experiments (*Œ. Lamarckiana*) not in single characters, but in all their parts. A mutation such as *nanella* was not only a dwarf, but also differed conspicuously in the size of its flowers and the shape of its leaves. It was recognisable as a new type from the first leaves following the cotyledons, and the same is true of many of the others. Another mutant, *lata*, not only differs strikingly in its leaf characters, but also in habit, in the shape of its buds and petals, and in the sterility of its pollen. This type will be referred to again later. Perhaps a dozen mutations in all were described by de Vries as arising suddenly and perpetuating their characters in later generations. Many others have been described since.

The publication of these results was concluded in 1903, and if I may be permitted to refer to my own interest in them, it was primarily to determine the nature of the change when a new type appeared. What had happened in the interior of the plant, in its germ-cells, to give rise to the new form? With this question in mind, it may be imagined that when, in 1905, the opportunity came to attempt an answer, the work was taken up with great vigour. The method was to examine by cytological technique under high magnification the structure of the dividing nuclei in the pollen-forming cells of the various types. In the following year the first results were announced, and they included the finding of different numbers of chromosomes—*i.e.*, a new structure of the nuclei—in certain forms. This most unexpected and gratifying result led to a decade of active research, in which various workers have taken part. During this period the nuclear structure or chromosome number of many of these new derivative varieties or species was worked out; and by this means the various relationships which they bore to the parent species were determined, and an explanation was also found for a number of their hereditary peculiarities. These were some of the results of an intensive cytological study of this group of forms during a period of ten years.

In order to make plain the bearing of these results on the general conception of mutation, it will be necessary to sketch briefly some of the developments of the last two decades in cytology or the study of cell-structure. That organisms are composed of innumerable cells, which are all derived from the division of a single cell—the fertilised egg—is known to all of us. The process of cell-division is not, however, a simple cutting in two, as in the multiplication of bacteria, but a remarkably complex phenomenon in which the nucleus plays the chief *rôle*. Its contents during the period of mitotic division become separated into a group of bodies known as chromosomes, whose number and relative size and shape are constant for the nuclei of any species, as is also to some extent their arrangement in the nucleus. These bodies represent the only parts of the nucleus which are passed on as structural entities from cell to cell. There are many reasons for believing, many lines of experimental evidence which indicate, that they are chiefly concerned in the determination of hereditary differences; and that the stability of the species and the regularity of its developmental stages are controlled in large measure by the presence in every cell of a nucleus, in which these structural relationships of parts are maintained from one cell-generation to another. In the process of mitotic nuclear division the essential feature is the splitting of the chromosomes lengthwise, so that the daughter nuclei get exactly equal shares of every part. The meticulous accuracy with which this process is carried through shows that it is of fundamental importance in the development and life of the organism.

Another point to which reference must be made is this. There is one unique division in the life-cycle of the organism in which, in the maturation of the germ-cells, the chromosomes, instead of dividing, arrange themselves in pairs, which then separate, and in this way, with complications which need not concern us, the number of chromosomes in the germ-cells is reduced to half that in the body-cells. In fertilisation the nuclei of two germ-cells unite, but their chromosomes maintain their separate identity, and so the double number is restored. In recent years it has been shown in various plants and animals that the paired arrangement of the chromosomes frequently takes place immediately after fertilisation and persists throughout the development of the individual; and, whether the pairing is early or late, each pair consists of one chromosome of maternal and a corresponding one of paternal origin. When the chromosomes differ from each other in size or shape, as they are now known to do in many organisms, it can then be shown that there are two of each kind. Every nucleus in our bodies thus contains corresponding elements derived from both parents. So fine are the meshes of the warp and woof out of which we are woven.

Now we have seen that the number of chromosomes is a constant for each species; *e.g.*, in the lily family, the genus Lilium has twelve pairs of long chromosomes, while Aloe has seven pairs of different lengths. In the evening primroses (Œnothera) the fundamental number is fourteen, or seven pairs. But mutations were found having fifteen, twenty-one, and twenty-eight, as well as other numbers. How these new numbers arise we have not time to consider, except to say that different processes are involved. But having arisen, a given number is characteristic of every cell of the new form. In *Œnothera lata* there are always fifteen chromosomes in the nuclei, in whatever part of the plant the chromosomes are counted. These and other facts, such as the doubled number (twenty-eight) of chromosomes in *Œ. gigas*, led to the point of view that each mutation is a cell-change, originally happening in the nucleus of a particular germ-cell and handed on from one cell generation to another by the mitotic mechanism.

But only a certain number of the mutations of Œnothera show a visible change in the structure of their nuclei. These exhibit peculiar types of hereditary behaviour depending on how their extra chromosomes are distributed in the germ nuclei. In other mutations, such as *brevistylis* and *rubricalyx*, there is no visible change in the structure of the nuclei, and the inheritance of the new character follows the simple Mendelian rule. In such cases it appears that a change, probably of a chemical nature, has occurred in one portion of a particular chromosome. We thus arrive at a means of accounting for not only the manner of inheritance, but also the origin of all Mendelian characters. Why this should be so would take us too long to discuss here. Nearly all of the two or three hundred different mutations which have appeared in the fruit-fly Drosophila in the experiments of Morgan have been of this Mendelian type.

Soon after the work of de Vries appeared, the objection was raised that *Œnothera Lamarckiana* was probably a garden hybrid. But this objection has been set at rest by the discovery of the same process of mutation in various other species, including wild species with small flowers studied by Bartlett in America, in which the chances of crossing are very remote. It has also to be remembered that complete absence of crossing is an exceptional condition in plants and an impossible condition in bisexual animals. However evolution has taken place, the unit in which it occurred must have been an interbreeding population of closely related forms. Evidence is accumulating that some apparently 'good species,' for example among the roses, are really crypthybrids, but this does not necessarily lessen the evolutionary significance of the variability which they exhibit.

A more serious objection to mutation as an evolutionary factor is that the new forms which appear are all abnormal, or pathological, or at any rate too weak to compete with their neighbours. It is undoubtedly true that many mutations may reasonably be placed in these categories, but by no means all. Occasionally new dominant or otherwise progressive mutations appear (*Œnothera rubricalyx* is one, *Œ. gigas* another) which will furnish a new starting-point for evolution in fresh directions. If we could have been present when the first composite flower appeared we should undoubtedly have looked upon it as a degenerate monstrosity. Yet the Compositæ, on account of the advantages afforded by their aggregate heads of flowers combined with their methods of seed distribution, have become the most successful and cosmopolitan family of flowering plants.

In the last few pages I have confined myself rather closely to the Œnothera story, because of the part it played in connection with the development of a number of the views which are now current in genetics. It is one of the lines of research which have helped to bring about a convergence of the originally independent sciences of cytology and experimental breeding. In other fields, such as the study of the sex-chromosomes in many animals and recently in certain plants, and in some of the experimental breeding work with Drosophila as well as in Œnothera, a definite relation has been demonstrated to exist between the chromosomes and the external characters and hereditary behaviour of organisms. Much remains to be done, but the correlation of these two fields is undoubtedly one of the most promising lines of future research in connection with genetics and the problems of evolution. Those who attempt explanations of heredity, variation, and development which disregard or are contrary to the known facts of cell-structure are likely to find themselves following blind trails which lead nowhere. Genetics has become the most active and many-sided recent movement in botany, or even in biology at large, and its future development will represent the synthesis of results obtained from many independent lines of research.

In the immediate future much attention will no doubt be devoted to the appraisement of the relative values to be attached to different evolutionary factors, such as natural selection, mutation, orthogenesis, and the neo-Lamarckian factor. The latter is now again receiving serious attention, and seems destined to form the basis of explanation of many cases of adaptation, particularly those in which recapitulation is involved in the development of the organism. If these results are to represent steady progress they must be based not upon speculation, but upon an experimental analysis of the incidence and action of the various factors involved, and their relationship to each other in bringing about that panorama of events which we call organic evolution.

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VII

PHYSIOLOGY

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VII

PHYSIOLOGY

THE problem of all others which is common to Physiology and to the majority, if not the totality, of the sciences, is money. Research has been endowed to a comparatively limited extent in this country, and many of us who have been across the Atlantic have watched enviously the millions of dollars bestowed upon the universities there. During recent years expenditure by our Government has necessarily been reckless, and there is now a tendency to economise; but one feels hopeful that economy will not begin with education and research.

Science played a part in our recent difficulties, and, for the first time, I think, in the history of the subject, physiologists were actually called into council to help the nation out of some of those difficulties. The late war has been very aptly described as the 'war of the Sciences'—Chemistry, Physics, Engineering, and others, all had a share in it. It was mainly in connection with the feeding of the nation that physiologists found their opportunity and exercised it, I trust with beneficial results.

The War brought home to us many new aspects of life, but peace has its problems as well as war. They may not be so urgent, or so apparently urgent, but they are none the less to be faced. An engineer would not be expected to manage his business properly unless he had some knowledge of engineering. We are all of us engineers so far as our life is concerned, and it is necessary that all of us, if we wish to obey the laws of health and to maintain a healthy race, should have some knowledge of the way in which our wonderful bodies work. Among the many questions which have arisen since so-called peace has been with us is that of the workers. It has been found not only desirable, but from every point of view imperative, that their lot should be made happier than it was in previous years. I do not know whether higher wages have made the worker any happier, but, in order to discover how to relieve the workman of tedium and fatigue, there was established some time ago a committee, under the chairmanship of the present President of the Royal Society, to investigate the causes of industrial fatigue. Well, that has been economised away.[1] That again, I am afraid, is an instance of economy in the wrong

[1] Since delivering this lecture I have heard from its chairman, Professor Sherrington, that arrangements have been made to carry on the work of the Industrial Fatigue Board under the auspices of the Medical Research Council.

quarter. I must not, however, spend too much time in speaking of financial questions, for, although money is the root of all evil, it is also the root of a great deal of good. I must hurry on to speak more particularly of some of the other problems which modern Physiology has to grapple with.

Physiology is the bed-rock upon which Pathology, *i.e.*, Physiology 'gone wrong,' is built. Physiology used in the Scottish universities to be called 'the Institutes of Medicine,' because that truth was realised by the earlier physiologists. It is obviously necessary, in order to diagnose or to remedy disease, where things have gone wrong, that we should have a preliminary knowledge of how things happen when they go right. It is my duty, my principal duty so far as teaching is concerned, to deal with medical students. Medical students are a very hard-worked body, and from time to time what is called the medical curriculum has to be revised, as new subjects enter into the sphere of medical learning. But the entrance of every new subject means that one is attempting to pour a quart into a pint jug, and so one sympathises with the increasing strain thrown upon our budding doctors, and one has of necessity to shorten some of the other subjects in order to make room for the new ones. There is a movement going on just now in the Faculty of Medicine of the University of London to curtail somewhat considerably not only Physiology, but also Anatomy, Chemistry, and some of the earlier studies which the student indulges in before he proceeds to the actual study of disease at the bedside. As a physiologist, and speaking as a physiologist, I of course regret any cutting down of the number of hours, or in the number of classes, which the student has to attend in my own particular subject, but at the same time I can quite realise that there are many details which apparently have no practical bearing at present, and which might quite well be omitted from the student's course of study. What is called Applied Physiology, actual practical application of physiological knowledge to medical practice, is really what is wanted. At the same time, one never knows what may not ultimately be of practical utility. There are many subjects which are studied with no ulterior or utilitarian aim which in the end turn out to be of inestimable practical benefit. So it is never absolutely safe to say that this or that is of no use because it has not yet been shown to be directly applicable to the relief of disease and injury. A legend—I do not suppose it is very much more than a legend—is told of an old professor at one of our ancient seats of learning, who, after a laborious life devoted to very intricate research, thanked God that he had never done anything that was or would be of use to anybody.[1] That is an extremist view. Professor Karl Pearson, of University College, in a recent address, has taken the opposite standpoint. He says: "I am a scientific heretic. I do not believe in Science for the sake of Science, but only in its application to man. Thought and learning are of little value unless they are translated into action." Well, there you have the opposite poles of opinion. The moderate person will take a place somewhere between the two extremes. Let me, however, introduce my argument concerning the usefulness of what looks (or looked) like the

[1] I have let my allusion to a legendary professor stand as I actually spoke it at the lecture. But since then my notice has been called to the fact that it is no legend, and that it did not occur at the close of a busy life. It was Professor Caley, of Cambridge, who made the remark. He was a famous mathematician, and in speaking to his friends on a piece of brilliant mathematical work he told them that "the most delightful thing about it was that under no conceivable circumstances would it be of the slightest use to anyone." In reference to this story my friend Sir John MacAlister wrote me a letter from which I quote the following sentences. They really point in a striking way the moral I was trying to drive home. "I was dining last night with an old friend and colleague of Professor Caley's, and reminded him of the story you have quoted in your lecture. He assured me, as I always believed, that it was 'only his fun,' for no one had a firmer faith in the value of all pure science, and his remark was only a sarcasm at the expense of the so-called 'practical' scientists. The particular piece of work he was referring to ultimately proved to be a remarkable example of the very thing you are aiming at. The working out of the problem became known as 'Caley's Tree,' and proved to be the missing link in the efforts of chemists to arrive at a solution of the arrangement of atoms, and enabled them not only properly to order their discoveries, but successfully to prophesy new ones."

useless by alluding to one or two occurrences in the past.

It was an experiment made by nature—an apple falling from a tree—that led Sir Isaac Newton to the discovery of that great physical law of the universe known as gravitation. Similar accidents have also been the indicators which have pointed out to thoughtful people practical work, at first undertaken with no ulterior object, but merely as a means of gratifying their desire for research, which has sometimes been called curiosity.

Towards the end of the eighteenth century there was in Bologna, in Italy, a man, destined for the Church of Rome, but subsequently turned out for his heretical opinions, who then happily applied himself to Science and became the Professor of Anatomy and Physiology at the university of his native city. His name, which has reverberated down the ages, was Galvani. Those were not the days of palatial laboratories—we have not got a very palatial one here ; we are hoping to get one when we move to Bloomsbury—and, so far as one can gather from what we know of Galvani's work, his arrangements were somewhat primitive, and he either used his laboratory as a kitchen, or, what seems much more probable, he used his wife's kitchen as his laboratory, and Mrs Galvani had her share in the accidental discovery which took place during the preparation of a midday meal.

She was preparing some frogs' legs for dinner, and had got them hung up in a row. Her husband was working a frictional electricity machine in the neighbourhood—in the same room—and she noticed and called her husband's attention to the fact that these apparently dead frogs' legs began to twitch. Galvani was so struck with this singular occurrence that he wanted to try the effect of atmospheric electricity upon the frogs' legs, and, hoping for a thunderstorm, he went up on to the roof and hung up his row of frogs' legs on copper hooks, attached to a railing made of iron. Instead of a thunderstorm there came a gentle breeze, and he noticed that when the toes of the frogs were blown against the iron railings they again began to twitch; *i.e.*, he discovered that, by the contact of dissimilar metals, he had made what was the first electric battery, and, in his contemporary Volta's hands, the voltaic cell was constructed, and that was the progenitor of our modern batteries and of the great branch of electrical science whose name, 'galvanism,' is an indication of its origin. As Helmholtz said, writing nearly a hundred years later, if this little experiment with the frogs' legs and the dissimilar metals had been disregarded as being of no use to anyone, what would not the world have lost, for, in a comparatively short time after the discovery, electric messages by telegraphic wires were travelling with the speed of lightning from one end of Europe to the other. If Helmholtz had been alive now, how much more might he have expanded that theme, for, since he died, radium, X-rays, and other forms of radiant energy related to electricity have been discovered, and have proved of benefit to mankind not only from the commercial, but also from the medical point of view, by relieving disease and suffering.

That is one example of how a comparatively trivial and at first sight purely academic piece of curious knowledge formed the foundation of the electrical science which now pervades every branch of human industry. Let me give you one or two more examples, also from the past.

How would the work of Lister have been possible without the chemical work that preceded his in the hands of the chemist Pasteur? Pasteur occupied himself with the examination of fermenting fluids, and discovered that putrefaction and similar occurrences are of the same nature as fermentation, and it was this that led Lister to apply the principle practically, and to invent the modern system of surgery, in which the agents concerned in fermentation and putrefaction, the germs or bacteria, are excluded from wounds.

Some thirty years ago I was in Basle, and there had the opportunity of seeing Miescher, one of the physiologists in the university of that city, who devoted himself mainly to the chemical side of his subject. He, like all hard-worked people, used to take holidays, and, combining business with pleasure, he not only went fishing for Rhine salmon, but preserved the roes of the salmon for chemical investigations, at which he worked during the succeeding winter months. In making out the chemical composition of the roe of the salmon and of other fishes, he laid the foundations for a correct understanding of the pathology of gout. It seems a long jump from salmon's roe to gout, but that was what it led to. I cannot trace, in the time that I have, all the steps in the process, but the pathology of gout, and the chemical changes that occur there in connection with uric acid, all rest upon the fundamental processes investigated by Miescher in his work on the roe of the salmon.

I will take only one more example from the past, and this brings us nearer to the present. My colleague Bayliss, of University College, was interested for many years before the War in what we are in the habit of calling colloids. It does not matter very much to the non-physiologist what colloids are. They are things like gum and glue and gelatin, and they present a great many interesting peculiarities which Bayliss examined. Who could have dreamt, when he undertook this purely academic piece of work, that he would, in consequence thereof, be able to relieve the symptoms of what is called 'shock,' which occurred so frequently during the recent war? Shock is very often due to loss of blood. How is the person to get on without blood? You cannot always get human blood. You cannot always get the friend, the heroic friend, who will give his blood to save his brother, and you must not use the blood of cats and dogs and cows and sheep, because then the remedy will be worse than the disease. And even when you can get the hero to step forward and give his blood for his friend, the donor must be very carefully selected, and a blood relation, if possible, should be chosen. Well, at the front there were comparatively few soldiers, I suppose, who had relatives at hand who could let their blood be tapped and thrown into these men's vessels, and, as a makeshift, it was found that the injection of a solution of salt would produce temporary relief, because it would increase the blood-volume and enable the comparatively small number of red blood-corpuscles, the oxygen-carriers, to do their work until more had been formed to carry it on. It was found, however, that the effect was only temporary, because the saline fluid leaked out almost as quickly as it was thrown in, until Bayliss, knowing the colloid properties of the blood itself, which resemble very much those of the various colloids he had been investigating, suggested mixing with the saline fluid some gum. He was able to do this with perfect confidence, and as a result the fluid did not leak out of the blood-vessels so readily, more time was given for new blood-corpuscles to be formed, and so thousands of lives were saved.

Coming now to some of the present problems which have been exercising our minds, I should like to preface what I have to say about them by a reminiscence of my own. In 1888, thirty-three years ago, I was a comparatively young man, and I attended almost my first meeting of the British Association, and, being a youngster, I listened with becoming reverence to the words of my elders. The President in that particular year was the great engineer Sir Frederick Bramwell, and, though it is so long ago, I remember, almost as well as if it had been yesterday, the way in which he began his address. He said—I am going to shorten his remarks—"I once spent a pleasant evening listening to the late Lord Iddesleigh, who delighted an entire audience by speaking upon the very important question of 'Nothing.'" Sir Frederick said: "I do not intend to imitate that feat, but I want to speak to you to-night of 'Next to nothing.'" By that he meant those little details, at first sight apparently of trivial import, but which after all are of vital importance, and he showed, with many illustrations, how crude machinery had been converted into useful mechanisms by attention to small, trivial, 'next-to-nothing' details. He took his examples from guns, bridges, ships, and so forth, and I particularly remember that he singled out the telephone as the triumph of genius which the 'next-to-nothings' had done so much to perfect. I do not know whether he would have selected the same instance to-day, for since that time there has been another Postmaster-General!

What is it that distinguishes a good cook from a bad cook? It is attention to small and trivial things—apparently trivial things. And it is just the same with the doctor and with the physiologist. This present time is the day of immeasurably great progress because of the increasing attention that is being bestowed upon the infinitely little. Let me give you a few instances of how these apparently insignificant things have played an important *rôle* in the progress of our science.

There are in our body a number of organs which we modestly conceal. In these days of open blouses you may see the outline of one of them in the neck region displayed to great advantage, particularly in those who have a rather large one. It is known as the thyroid gland. Now the thyroid gland is one of a group of formerly mysterious organs, about which so little was known that they were regarded as next door to useless. The thyroid is only one, and it has been rendered somewhat familiar to the general public because it has been boomed so much in the daily press. It has been found that this thyroid gland manufactures a complex material which it pours out into the blood in very, very small quantities, but which is absolutely indispensable not only to health but to life itself. It is customary to call these chemical messengers that minister to distant parts of the body 'hormones' (stimulators). Some of them do not stimulate, some of them do the reverse, but this particular hormone that the thyroid gland makes is a stimulator, and is necessary for healthy growth. It is known now that, by feeding them upon thyroid, animals not accustomed to that diet —I mean creatures like tadpoles—may be made to grow at a greatly increased rate. The particular material that is formed in the thyroid gland, which has received the name of 'thyroxin,' contains a small amount of an element which was supposed in former days to be exclusively confined to the sea-weeds, or at any rate to the plant world—I mean the element Iodine. This iodine-complex is necessary to maintain health. If too much of it is formed there is a certain kind of disease; if too little is formed there is the opposite disease, in which the symptoms are the converse.

To give you some example of how little there is of this material, I may allude to some work which has been recently carried out in America, and which has resulted in the preparation of thyroxin in a pure form and in sufficient quantities to enable us to analyse it, crystallise it, and subject it to all the various chemical operations which chemists use. Such research would, I think, in the present day be impossible in this country, but in America they do not always have an eye to the main chance, and on several occasions commercial firms have stepped forward for the sake of what looks at least like pure science. Messrs Armour and Co., of Chicago, set up a special plant to enable Dr Kendall to separate out this thyroxin and subsequently examine it—a special plant costing many thousands of pounds. In order to obtain the quantity he required he had to use six thousand pounds of ox thyroid. They can collect that quantity in America in the big packing-houses. Six thousand pounds of thyroid to start with—how much of the material do you think they finished up with? About a hundred and fifty grains. There are seven thousand grains in a pound, and a little arithmetic will convince you that it was a very small fraction of a pound of this material which they got in the end. But it was sufficient for Dr Kendall to carry out his researches upon. The quantity which is daily thrown into our blood, which looks like next to nothing, must be an enormously minute fraction, if one may use so apparently contradictory a phrase. But this minute fraction is necessary for the maintenance of normal nutrition.

I will now take an example from another of these so-called 'ductless glands.' There is, sitting on the top of each of your kidneys like a cocked hat, a little red thing which is called the supra-renal body—the adrenal body or gland, some people call it. Many years ago it was noticed that, when this little gland becomes diseased upon both sides simultaneously, so that finally there is no healthy gland left, the patient becomes weaker and weaker, and gradually dies. The particular train of symptoms was called Addison's disease. Dr Addison, who discovered the disease in question, was one of the old physicians at Guy's Hospital some two generations ago. What he then found has since been confirmed, not only by observations on the disease, but also by experimentation upon animals, viz., that these two insignificant-looking little bodies are indispensable for life, and that their removal or disease brings about death within a comparatively short time. We do not know all the different chemical materials which they pour into the blood, but one of them, at any rate, has been separated and obtained in a pure crystalline form by taking many hundredweight of suprarenal glands (also in the Chicago houses) and analysing them. A minute quantity, again, of this material, which is called adrenaline, has thus been prepared. It is extraordinarily powerful ; a very little goes a tremendously long way, but that, little though it is, is indispensable for the keeping up of life, for the maintenance of the heart-beat, the contraction of the blood-vessels, the passing of the necessary amount of sugar into the circulation, and the maintenance of muscular tone. So accurate now is our knowledge of the exact constitution of adrenaline that it has been prepared in the laboratory by synthetic processes. We do not need to go to the gland to get it; we can prepare it chemically.

If you take a solution of one part of adrenaline in a million of water, and inject a few drops of this extremely diluted solution into the blood-system, there are marked physiological results on the heart and on the blood-vessels. Doses of this nature make the doses of the homœopathist, the product of a past generation, appear gigantic in proportion.

In a course of public lectures similar to the present which I organised in King's College two years ago, and which have since been published under the title *Physiology and National Needs*,[1] the topic of foods loomed largely, as was only natural at that period of rationing. Among the subjects there treated at length is the very important one of Vitamins. It admirably illustrates the 'next-to-nothing' principle.

Foods as they occur in nature contain not

[1] Constable (London, 1919).

only the bricks out of which the body is built (proteins or albumins, starches, sugar, fat, salts, and water), but in addition minute amounts of certain accessory materials, which make body-building possible, and without which growth, health, and even life itself are impossible. It is to these accessory materials that the name 'vitamins' has been given. They are present in such small amount that it is only quite recently that their presence has been demonstrated, and even now we are ignorant of their chemical nature. They are all ultimately products of the plant-world, and it has been shown that there are several of them. They differ in their distribution and solubilities; when they are withheld from the food, diseases which are called 'deficiency diseases' occur. Up to the present, research has centred around three of them; they have been labelled A, B, and C.

Vitamin A is contained in the green parts of plants, and it is readily soluble in fats. When the cow eats grass this vitamin passes into her milk, and gives to butter its supreme value as compared with butter-substitutes made by hardening vegetable oil, where, owing to the high temperature employed in their manufacture, any vitamin which might have been present is destroyed. In animals which ultimately rely on marine vegetation, such as the fishes, the vitamin is mainly stored in the oil of the liver ; hence the well-known superiority of cod-liver oil over such an oil as olive oil in treating malnutrition has been at last explained. Absence or deficiency of this vitamin is the cause of stunted growth, and so fully are most observers convinced that this is one cause of rickets, and the associated condition of bad teeth, that it is now usual to speak of it as the 'anti-rachitic' factor.

Vitamin B is contained in the embryo-plant in the seeds of cereals and similar grains used as food. In highly milled grains it is removed by the polishing process, and the use of polished rice in the East led to the development of a terrible scourge called beri-beri. Superfine white flour made from highly milled wheat is equally imperfect as a food, and our war-bread, in spite of our distaste for it, had the advantage of containing the health-giving vitamin. The disease, if produced, can always be cured, and cured rapidly, by adding the polishings of the grains to the diet. So prominent a symptom is neuritis in the complaint, that the term 'anti-neuritic' factor is often applied to this particular vitamin.

Vitamin C is the 'anti-scorbutic' factor ; it is present in fresh fruits and most edible vegetables. It was the absence of these commodities in foregone times, when sailing-ships toiled on their long voyages, that made scurvy the curse of the navy and the mercantile marine. In our own day similar deprivation, such as occurs in polar expeditions, or during prolonged sieges, has led to outbreaks of scurvy once more.

On the ordinary mixed diet of the present time, outbreaks of such deficiency-diseases do not occur, because an element missing in one form of food (*e.g.*, white bread) will almost certainly be supplied in another. The main danger in this country is among the poor, unable to obtain sufficient variety, and this is specially seen in the bringing up of children when the chief fat supply may be inferior cheap margarine. Rickets is far too common to-day. Another danger manifested also in children is infantile scurvy; and this is caused by the extensive use of sophisticated foods (in which the vitamin has been destroyed) in place of the foods, such as milk and fresh fruit, made in Nature's laboratory.

The small amount of the various vitamins which is necessary is really marvellous, and makes one wonder whether they will ever be separated in quantities sufficient for analysis. I, however, am an optimist, and believe that in the future these difficulties will be overcome just as in the past, when the outlook appeared to be as hopeless.

To illustrate the small amount which is needed, may I give you two definite examples?

My friend Professor Hopkins of Cambridge, the pioneer in this country on vitamin research, fed his experimental animals on physiologically correct amounts of purified foodstuffs, but they all sickened and died, not because the food was poisonous, but because it lacked these accessory factors. He then, in a similar batch of animals on the same diet, added daily a small amount of natural food, viz., about a teaspoonful of milk, an amount which, as regards its actual value for nutriment and energy supply, is negligible. Nevertheless this quantity was quite sufficient to convert the otherwise useless diet into an efficient one, on which the animals were able to grow, thrive, and multiply. The milk was, owing to the vitamins it contained, able to bring about this happy result.

Let me take my second example, a still more striking one, from the work my colleague Dr Drummond is carrying out now in special reference to cod-liver oil, which is particularly rich in the A (the fat-soluble) vitamin. I say particularly rich, but all these terms are relative. The absolute quantity present must, in terms of ounces or even grains, seem unimportant. There are many brands of cod-liver oil in the market, and those which are specially lauded by their makers are those which are refined so that their appearance and taste are made more agreeable. Refining is a drastic process, and vitamins are delicate and are injured or destroyed by high temperatures unless the oxygen of the air is excluded. Dr Zilva, of the Lister Institute, has compared the anti-rachitic value of the crude oil and of these so-called ‘ refined ’ products, and found that the potency of the latter is reduced many hundred-fold as compared with the natural oil. Dr Drummond has now in his laboratory a crude fish-liver oil of which a single drop per diem is able to accomplish the work of Hopkins' teaspoonful of milk. A drop of this oil, remember, is not a drop of pure vitamin, though if it were the result would be marvellous enough. It is a drop of oil, and when one deducts from its weight the amount of the known substances in it, which can be estimated (the actual oil or fat, etc.), what remains? Merely an infinitesimal quantity. Could one go further as an illustration of the overwhelming importance of the infinitely little?

There are many other chemical substances that have been separated out from the different parts of the body which, in a high degree of attenuation, will produce marked results. One of them I should like further to allude to, because it has brought into prominence, or was itself brought into prominence, by a study of what Harvey discovered, the circulation of the blood. I must take for granted that, although the majority of my readers are not physiologists, they at least do know that blood goes round and round in the body. The heart is the great pumping-station, and the tubes that lead from the heart to the other portions of the body are what we call the arteries, while the tubes that lead back to the heart and carry the blood home again are what we call the veins. But what is the object of the blood being pumped along the arteries? It is not for the mere fun of the thing. It is because the arteries terminate in a network of extremely minute vessels which require a microscope to see them, called the capillaries, and it is in these that the blood does its work. And 'efficiency' is the watchword here. This work has to be very expeditiously performed, because the blood only remains in these tiny tubes for a second at most during each journey it makes. They were originally called capillaries, I suppose, because a hair was about the smallest thing that people of those days could think of, but they are very much finer than the finest possible hairs. A hair is a solid thing, a capillary is a hollow thing; and capillaries together form a network of minute vessels, through which the blood moves comparatively slowly in order to enable it to give its oxygen and nutriment to the tissues through which it passes, and to remove from the tissues the waste material that they do not want and carry this to the organs of excretion, by means of which it is got rid of.

I think you will at once see the reason why physiologists have devoted a considerable amount of attention to the capillary circulation. It is because, as I said, it is the part of the circulation where the blood actually performs its duty. The flow along the other vessels is merely a means to an end ; the main thing is the distribution of the blood as it flows along these microscopic vessels.

I have alluded to the question of shock as being due to loss of blood. It was found during the War that there were many cases of shock in which there was comparatively little loss of blood. Shock has been divided into various categories. There is 'shell-shock,' due to injury to, or a thorough shaking up of, the brain (concussion). That is not the shock which I am speaking about. There is 'primary shock,' in which the person is knocked 'all of a heap' as the result of a sudden injury ; and there is what I want more particularly to dwell upon, viz., 'secondary shock,' in which the person, after the immediate effects of the wound have passed away, goes back into a condition of collapse, although he may have lost very little blood. Where has the blood gone to ? It is found that it has dilated the capillaries, and has become stagnant there. It has got side-tracked in certain parts of the body, so that not so much is available for the more important organs, which, on account of their comparatively bloodless condition, become inactive and may cease work altogether.

Leading into the capillary area—for the total number of capillaries makes a very big area—are a number of little vessels called arterioles. These arterioles are like taps that can be turned on or off, or made to trickle slowly or to flow fast, and the size of these orifices that lead into the capillary area can be varied because certain nerves act upon them.

In some of these cases of shock it has been found, then, that, owing to the nervous upset, an undue flooding of the capillary area is produced. It has further been found, in cases where such nervous upset is negligible, that the capillaries are themselves contractile. They can accommodate more or less blood, because they sometimes become narrower, sometimes wider, and the normal condition of the capillaries that produces an efficient circulation is due, no doubt, to certain chemical materials, and possibly also to certain nerves analogous to those I mentioned as controlling the arterioles. It has again been found, in these cases of shock, that the almost universal accompaniment of the condition is crushed tissue—muscle or flesh. Crushed tissue becomes the ready prey of bacteria. The material becomes broken down by the bacteria and poisonous products result, and in many of these cases there is the production of a certain poison called histamine that brings about this particular effect. You can produce the condition of shock by injecting a very minute quantity of it into the circulation of an animal. Whether histamine is really the only poison that acts in these cases of crushed tissue and secondary shock is one of the questions for the future, but, at any rate, if there is any other it must be a poison that acts in a similar way and probably has a similar origin.

I have taken the above as another example of the importance of attention to minute details, and again it illustrates what is so frequently, indeed almost universally, illustrated throughout recent, and I expect will be illustrated in future, physiological work—namely, that the 'next-to-nothings' often prove to be of supreme importance in solving the puzzles that perplex investigators.

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VIII

ANATOMY

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VIII
ANATOMY

THE present-day problems of any Science are so vast that their listing would be no mean work and their full discussion a task well-nigh insuperable. The older he grows the more a scientist realises how little he really knows, how much there is to learn and to unlearn. He lives to see old beliefs slowly crumbling away and new avenues of research opening out with bewildering rapidity and ever-widening vista.

Anatomy serving as the handmaid to the art of the physician and to the craft of the surgeon, the anatomist is primarily concerned with the study of the form and exact positions of the different organs, and of the structure of the various tissues which go to make up the body of man. Such studies, vastly important though they be from a practical and utilitarian point of view, form but a relatively small part of his field of investigation. The anatomist is not content with knowing the form and position of any part of the body; he seeks further to learn its functional correlation with other parts, its intimate architecture whereby it is adapted to play its efficient part in the general body-economy, the complex processes which occur as it is built up from its earliest and simplest beginnings, and the factors which determine its characteristically human form.

Anatomical researches are by no means confined to man alone. Undoubtedly the vertebrate animals, more particularly the so-called higher vertebrates, are being as closely studied by the anatomist as by the zoologist himself in order to gain knowledge which will enable the former to interpret human form, and more especially to glean some insight into the hereditary factors which influence it. Since the days of Darwin the ancestry of man has been an attractive but at the same time a most abstruse and complex study. New evidence is constantly accumulating, evidence gathered not only from the study of other animals, but also from the study of remnants of humankind and man-like forms which, lying buried and forgotten for long ages past, are unearthed from time to time. Concerning this question contention rages high ; its final solution, if solved it ever be, is not yet in sight, and must be the outcome of many years of patient discovery and research.

Some parts of the body are comparatively simple, others of bewildering complexity. The more important the organ, and the more essential the part it plays in the life of the individual, the more intricate is its structure and the greater the problem or series of problems it presents to the anatomist. The brain may be cited as an example of an organ which, on account of its very complexity, has received and is receiving more attention than any other part of the body, and about which so vast a literature has sprung up as to require a lifetime for its perusal and study.

Valuable lessons, however, even in Anatomy, may be learnt from little things, and an appreciation of fundamental problems may be gathered by a consideration of the simplest structures. Of the less complicated parts of the skeleton the apparently simple framework of the body will appeal as an example. If an anatomist can claim sure knowledge of anything to do with the human body, his knowledge concerning a plain ordinary bone should presumably be fairly complete.

In making confession of what an anatomist knows and what he does not know concerning a typical bone no unintelligible technical terms are needful; the story can be told in the simplest and plainest language. A facile familiarity with scientific jargon often passes as a counterfeit for real knowledge, a cumbersome verbiage serving as a cloak for the most profound ignorance. To give a subject-matter a sufficiently long and impressive name, and to let that pass for real knowledge, is a pitfall into which the scientist himself often falls.

The bones of the body serving for the support, and in many cases for the shielding of the more important and delicate soft parts, are particularly liable to be affected by strains and stresses, as the result of the resistance they offer to the effect of the body-weight and to the pull exerted upon them by the muscles.

The thigh-bone may be quoted as an example of a bone which in the standing position is constantly being pressed upon by the weight of the body, and is frequently subject to the strain exerted upon it by large and powerful muscles. A study of the general architecture of a human thigh-bone will be a convincing proof of its beautiful adaptation to resist the forces to which it was subject during the lifetime of the individual to whom it belonged.

The thigh-bone may be described as consisting of an elongated column or shaft which expands into two massive parts at either end. If it be cut through in its length and the sectionised bone examined, it is found that the columnar part or shaft is occupied by a central cavity. This central cavity, known as the marrow cavity, is filled during life with soft marrow, and is a general characteristic of a great many bones of the skeleton; it is a familiar feature of a so-called 'marrow-bone' of an ox or a sheep. It will further be noticed that the walls of the central or marrow cavity consist of hard, condensed bone, thickest towards the middle of the shaft and gradually thinning or tapering off towards either extremity. The large expanded extremities are seen to be covered by a tenuous shell of condensed bone of almost egg-shell thinness, and are not occupied by a cavity, but filled by an open-work or honeycomb arrangement of bony substance usually termed 'spongy bone,' but for which the term 'lattice-work' is preferable.

The shaft or columnar part of the thigh-bone is especially liable to what, for the sake of simplicity, may be termed breaking forces, and a simple experiment will furnish a convincing proof that a tube or hollow cylinder is a most efficient construction for resisting such forces.

If a stick about half an inch in diameter is slowly broken by holding it in the two hands and pulling either end towards the holder, it will be noticed that on the far side the substance of the stick is rended or pulled apart, while on the near side it is forced together or crushed. Further, it will be obvious that at the beginning of the breaking it is the parts of the stick towards the surface which first give way, torn apart on the far side and crushed on the near, while the more deeply situated substance of the stick towards its centre must feel the breaking force to a less extent, and is in a situation where it cannot play such an efficient part in resisting the strain. From this it follows that if the material of the stick were taken away from the more deeply situated central part and heaped on or towards the surface a greater difficulty would be experienced—or, in other words, a greater force would be required—in breaking it. In short, the stick would be more resistant to a breaking force.

The result of taking away substance from the central part of the stick and heaping it on the surface would be to fashion a tube or hollow cylinder, and hence the commonplace that with a given amount of material a hollow cylinder is a much more efficient construction for resisting a breaking force than a solid column.

The middle part of the thigh-bone is the part of the bone where the breaking forces to which it is subject are most effective, and here the walls of the central or marrow cavity are thickest. Towards either end of the bone breaking forces are less effective, and thus is explained the gradual tapering of the walls as they are traced to the two ends of the bone. In the building of the shaft of the thigh-bone the bony substance is obviously used with the greatest economy, but at no sacrifice of efficiency.

The principle of the hollow cylinder as a mechanical contrivance exhibiting the maximum amount of resistance with the expenditure of a minimal amount of material occurs most extensively in nature, being exemplified by the bamboo cane, the quill of the bird's feather, and many other familiar objects too numerous to mention. The contrivance was discovered in the building of plants and animals millions of years ago, and by comparison man discovered it but yesterday, and applies his discovery in manufacturing his tubular bridge, the tubular frame of his bicycle, and in many other ways.

About the middle of last century the anatomist first recognised that the spongy bone or lattice-work arrangement of bony substance, as seen in the expanded extremities of the thigh-bone, was not an unintelligible or haphazard arrangement. By careful study of the disposition of the minute bars of bony substance constituting the lattice-work, and estimating the chief stresses to which these parts of the bone are subject during life, it was discovered that the arrangement was such as to render the bone wonderfully efficient in resisting the forces to which it is subject, with the most economical use of the bony material imaginable.

The chief stresses to which the expanded ends of a bone such as the thigh-bone are subject are compressing or pushing forces and tension or pulling forces. If the lattice-work at the lower end of the thigh-bone be examined, the mechanical value of the arrangement of the bars of the lattice-work can be readily appreciated. In the standing position the weight of the body presses vertically in the length of the thigh-bone, and will tend to compress the lower end. If the lattice-work which goes to make up this end of the bone is sliced through, it presents the appearance of such delicate lacework as to suggest frailness. The minute slender bars which constitute the lattice-work have, however, a very definite arrangement. Most of the bars are vertically set—that is, exactly in the direction in which the pressure of the body-weight makes itself felt. Although any individual bar could not by itself bear any considerable pressure force, yet the total complement of the vertical bars endows this end of the bone with a resistance easily sufficient to withstand any compression to which it is subject during life. The vertically set, pressure-resisting bars are connected or tied together by shorter cross-bars. These cross-bars tying the vertical bars together prevent their spreading or buckling, as they are liable to do when being pressed upon from above, and are an essential and important part of the arrangement. In short, the lattice-work occupying the lower end of the thigh-bone is, in builder's parlance, a complicated system of struts and ties.

One of the most beautiful examples of lattice-work, enabling a comparatively small, light bone to withstand enormous stresses, is found in the heel-bone. The bones of the human foot are disposed in the form of an arch supporting the body and transmitting the weight thereof from the leg to the ground. The arched foot is one of the distinguishing features of man, enabling him to stand and walk in the erect posture with facility.

The foot-arch may be described as consisting of two pillars set at an angle to one another. The body-weight bears upon the angle or apex of the arch, and is thence transmitted by the two pillars to the ground. Of the two pillars, one longer and more obliquely set slopes groundwards towards the toes, while a shorter, more vertically set hinder pillar is provided by the heel-bone. Owing to the more vertical set of the hinder pillar, the larger component of the body-weight is transmitted to the ground through the heel-bone, which is consequently subject to very considerable compression in the standing position.

If the heel-bone be examined, it gives the impression of being a solid mass of bone, but if it be cut through with a saw it is found to consist almost exclusively of spongy or lattice-work bone surrounded by the merest shell of condensed bone. The bars of the lattice-work are so slender as to make the spongy bone present in the lower end of the thigh-bone look quite coarse in comparison. Two chief sets of bars can be easily recognised in the lattice-work. One set slopes downwards and backwards from the upper part of the bone—that is, from the apex of the foot-arch—to the lower and hinder part of the bone where it is opposed to the ground. This set of bars is disposed exactly in the direction of the lines of pressure to which the heel-bone is subject when the body is in the standing position. The second set of bars curves upwards towards the back of the bone, and crosses the pressure-bars at right angles. This second set of bars obviously ties the pressure-bars together and prevents their spreading or buckling.

Fastened to the hinder part of the heel is a great cord, easily recognisable and felt in one's own leg as it extends downwards from the calf to the heel. By means of this cord the calf-muscles are enabled to pull upon the heel-bone and raise it from the ground, as happens when standing upon the toes and at every step in walking. The calf-muscles are one of the most powerful muscular combinations found in the body, and can exert an enormous pull. This is readily understood when it is considered how easily the heel can be raised from the ground even when one is standing on one leg. Under these circumstances the calf-muscles of one leg can lift nearly the whole weight of the body with the greatest facility. As a consequence the heel-bone is subject to an enormous strain when the calf-muscles set to work and pull upon it. It is enabled to withstand this strain owing to the disposition of the slender curving bars of the lattice-work, which will be found to impinge upon the back of the bone where the cord of the calf-muscles is fastened, and are accurately disposed in the lines of strain to which the bone is subject when pulled upon by these muscles.

The second set of bars in the lattice-work of the heel-bone obviously play a twofold part. They tie the first set of bars, or pressure-bars, together, enabling them more efficiently to resist the compression due to the body-weight, and at the same time they enable the bone to resist the enormous strain put upon it by the powerful pull of the calf-muscles.

In spite of the great stresses to which it is subject, the heel-bone never exhibits the slightest indication of undergoing distortion, either in the direction of being crushed or being pulled apart. A dried heel-bone, the relic of a once heavy and powerful man, was weighed, and was found to consist of scarcely three-quarters of an ounce of bony substance. It is almost inconceivable that with such an inconsiderable amount of material a bone of the size and of the resisting qualities of the heel-bone could be built.

Lattice-work as a structural device for resisting stress is a human discovery later than that of the hollow cylinder, and is rapidly coming more extensively into vogue. The graceful lattice-work supports of the enormous cranes now at work on the Aldwych site win the wonder and admiration of the crowded Strand. Lattice-work plays an important *rôle* in the construction of the cranes themselves, and renders them such beautiful and attractive objects as compared with the cumbersome solid cranes in use but a few years ago. The lattice-work masts distinguishing the warships of the United States of America are further examples of the usefulness and efficiency of the device.

The lattice-work arrangements found in the lower end of the thigh-bone and in the heel-bone are comparatively simple and easily interpreted, but in other parts of the skeleton the arrangements of the lattice-work are far more complicated, their functional meanings are far from clear, and at present seem almost to defy analysis. The subject is receiving considerable attention at the present time, and there has lately appeared a most portentous tome in which new views are promulgated and the matter is discussed at considerable length. A perusal of this work will convince the reader how far we are from the right understanding of the mechanics of bone architecture. Invaluable information regarding the most effective arrangement of material for withstanding strain and stress might be gleaned by the architect and the engineer, should they turn their attention to a study of the human skeleton.

The building of a bone is one of the most wonderful series of events occurring during the growth of any animal provided with a bony skeleton. As the happenings are exceedingly intricate and complex, it will only be possible to notice some of the most interesting.

The human thigh-bone first makes its appearance in miniature, and modelled not in bony substance, but in cartilage. Cartilage is a tissue which, in consistence like hard india-rubber, is very elastic and flexible, and well serves the purpose of a skeleton substance in the early stages of life. Owing to its flexible skeleton, containing as it does a large proportion of cartilage, an infant often escapes severe injury in an accident which plays havoc with the more rigid skeleton of an adult.

When cartilage is sliced and examined under the microscope it is found to be an exceedingly simple tissue—innumerable small bodies, or cells, occupying minute cavities in a homogeneous substance having a striking resemblance to ground glass in appearance. It may be aptly compared to almond toffee, supposing the toffee were replaced by a resistant semi-translucent jelly ; the almonds being the counterparts of the cells, the toffee the substance in which the cells are embedded. One of the remarkable features of cartilage is that it is a bloodless tissue, containing no blood-vessels. Being devoid of blood-vessels, the method by which cartilage is nourished is not properly understood, and awaits explanation.

The growth and expansion of cartilage is apparently a simple process. The cartilage cell periodically divides into two. Each of the two resulting daughter-cells vigorously manufactures embedding material, exuding it, as it were, all round itself, and thus becomes separated from its twin sister. Subsequently each daughter-cell will in its turn divide, giving rise to two new cells, and thus the process goes on. As many cells are similarly engaged, the cartilage grows and expands, each new cell adding its quota to the whole.

There comes a time when, at one definite spot situated at about the middle of the cartilaginous miniature of the future thigh-bone, the cartilage sickens and dies. This is the only possible interpretation of the peculiar appearances presented by the cartilage, and in very truth death first lays his finger upon us before we are born.

The dying cartilage is invaded from without by a veritable army of cells which practically eat their way in. This statement, simple though it appears, is pregnant with interest. One is apt to regard all the tissues of the body as working harmoniously together for the common good, every group of cells in its allotted place concerned in its allotted task. This, however, is not absolutely true. There is evidence to show that competition may take place between the various cells of the body, that under certain circumstances some cells may grow and proliferate apparently at the expense of others. Further, cells of one kind may wander beyond the territory to which they properly belong and encroach upon another territory or tissue—in other words, invade it. It is known that such encroachment or invasion, to the ultimate detriment both of the invaders and the invaded tissue, takes place abnormally in disease, and is, for instance, characteristic of that fell scourge, cancer.

Such invasion may be due to a peculiar abnormal aggressiveness of the invading cells, whereby not only do they multiply very rapidly, but they acquire, in some way or another, the power of breaking down the barriers by which, under usual conditions, they are limited.

The invasion may not, however, be entirely due to the aggressiveness of the invading cells, but may be due, partially at all events, to a diminished resistance of the invaded tissue, a resistance which under normal circumstances is sufficient to keep neighbouring cells of another kind in their proper place. For aught we know, different types of cells in the body may be constantly striving to invade one another, but are kept in their proper places by a mutual and evenly balanced antagonism.

It can now be realised why the invasion of the cartilage by cells from without as a normal occurrence in the history of bone-formation is of such peculiar interest, and why its proper understanding is of such predominant importance. It may throw light upon the elucidation of the abnormal invasions occurring in cancer, in the investigation of which so much time, energy, and patient perseverance have been spent, with unfortunately so little tangible result.

Do the changes in the cartilage whereby it loses its vitality precede the invasion of the cells from without, and does the loss of this vitality imply a diminution of resistance inviting the invasion? Or, on the other hand, do the invading cells exert an influence resulting in the degeneration of the cartilage? Of the two phenomena, the degeneration of the cartilage apparently takes precedence, and consequently may be the circumstance which determines the invasion. Concerning the factors which are the influential cause of the degeneration of the cartilage we are absolutely in the dark.

The cells invading the cartilage seem to be engaged in three main activities. One set, which may be termed 'excavators,' burrow their way into the dying cartilage, in which they eat out relatively large irregular spaces. A second set, 'bone-builders,' deposit bony substance on the irregular remnants of the dying cartilage, these remnants providing a scaffolding upon which the bone is built. A third set, 'vessel-formers,' are concerned in the appearance of a complicated system of minute tubes or pipes linked up with the vessels outside, the fluid, that is the blood contained therein, serving to bring material necessary for the working of the bone-builders, and to carry away the excavators' waste. The scene, as it is viewed under the microscope, is wonderfully like a busy house-building, with excavators, scaffolding, builders, supply-pipes, and sewers. The single pipe system, serving, at one and the same time, to bring the needed material and to conduct the waste away, must make a sanitary engineer green with envy.

The bone-building cells are the active agents by means of which the bone appears in place of the cartilage, and it behoves us to know whence they come. To make an honest confession, we have no certain knowledge. Some authorities maintain that the cartilage cells, set free by the excavators from their spaces in the groundwork of the cartilage, are, as it were, rejuvenated, and take on a new *rôle*, that of building bone. Such an explanation is, on the face of it, most unlikely. It is more probable that the bone-builders come in from without with the invading cells. If this suggestion be granted, we should be further enlightened as to the nature of these cells. Are the bone-building cells proficient in bone-building alone and incapable of any other activity, or are they cells which build bone under certain circumstances, but which, under other circumstances, might set to work to build some other tissue? This is an interesting problem which is yet to be solved.

If we may believe that the bone-builders are *bona-fide* trade unionists who would not lay a finger on any other job, is there any particular part of the body whence they come? One investigator, who sacrificed the promise of a brilliant career as an anatomist eventually to become a Cabinet minister, adduced striking evidence to demonstrate that the bone-builders migrated to the deeply situated cartilage from the surface of the body. If this be true, the discovery may be one of the deepest significance, for the following reasons. The zoologist classifies animals into two great groups—the Vertebrates, or animals possessing a backbone, and Invertebrate animals without a backbone. Of the two groups, the Invertebrates are the lowlier and more primitive, and it is believed that the Vertebrates, which appeared at a later date in the world's history, sprang from Invertebrate ancestors. One of the great differences between the Vertebrates and the Invertebrates is, that when the latter are provided with a hard resistant skeleton it is found on the surface of the body, and is built by surface-cells. The shell of an oyster and the coat of armour of a lobster or a crab are familiar examples of such outside skeletons. The Vertebrates, on the other hand, are provided with an inside skeleton deeply embedded in the body. In some primitive Vertebrates—in certain fishes, for instance—the inside skeleton is constructed of cartilage, and never advances beyond the cartilaginous stage. The presumption that the deep body-cells of the Vertebrates have learnt, in some way or another, to manufacture a cartilaginous inside skeleton, and farther than that they cannot go, is justifiable. When an inside skeleton of greater rigidity than can be afforded by cartilage becomes a functional necessity to a Vertebrate it is possible that the surface-cells, who learnt the business of manufacturing bone or bone-like substance in the Invertebrate ancestor untold ages ago, migrate from the surface and take their share in providing the Vertebrate with an inside skeleton. This is, of course, pure hypothesis, but as such it offers an explanation of the battle of the tissues ultimately resulting in the replacement of cartilage by bone, and proves, at any rate, that the problem of the origin of the bone-building cells is one of considerable interest.

When the actual process of laying down bone first begins, the bone-building cells seem to be marshalled and ordered in a very definite manner, being ranged in rows about the scaffolding furnished by the remnants of the cartilage. Examining them under a microscope, one is reminded of a vast army of soldiers, each unit in its appointed place engaged in its ordered business. The bone-building cell is a most indefatigable workman, literally putting every atom of its energy into the business. It builds bony substance all round itself until it is eventually entombed in the results of its own activity, and a minute bony nodule is formed. About the bony nodules thus first laid down other bone-building cells range themselves, repeat the process, and the bony tissue extends by a process of concretion. Once the bone-building cell is encased by the bony substance it has manufactured its energetic activities cease, and, as far as we know, it plays for the rest of its existence the more placid *rôle* of controlling and maintaining the welfare of the bony material in its near neighbourhood. Unlike the cartilage cell, it never divides, and consequently the bone cannot expand, like the cartilage, by interstitial growth.

The bone first laid down has a temporary existence only. Occupying the central part of the shaft, it soon disappears by being excavated and absorbed by the excavating cells. As a result, a small central cavity, the rudiment of the marrow cavity, comes into existence. The excavating cells are therefore not only concerned in excavating the cartilage, but are further concerned in absorbing bone.

The stage is now reached where the middle part of the shaft of the future thigh-bone is occupied by bone surrounding a small cavity, the rest of the shaft and the expanded extremities being still cartilaginous. The bone-forming tissue thenceforward spreads towards either end of the shaft, attacking and eventually replacing the cartilage. The cartilage can only defend itself by growing, and this it does with rapidity, expanding in every direction, but retaining its general shape. As the cartilage grows, it presents an ever wider and wider front to the invading bone-forming tissue. Thus it is that the bone as a whole grows in size, the cartilage expanding at either end and being constantly attacked and replaced by the bone-forming tissue.

Of the two processes, the expansion of the cartilage and the attack of the bone-forming tissue, the latter is relatively the more energetic. The time arrives when the cartilage of the shaft disappears and is replaced by bone, the bony shaft being occupied by a central cavity, which by the activity of the excavating cells is constantly enlarging, keeping pace with the bone-formation and slowly creeping towards either extremity.

When the bone-formation reaches the expanded extremities, they are occupied by a considerable bulk of growing cartilage, so large that for a time bone-formation and cartilage-growth keep pace with one another, the bone-forming tissue being seemingly incapable of overcoming the growing cartilage and occupying the ends of the bone. After a time, and this occurs at a comparatively late stage in the history of the thigh-bone, the bulky cartilaginous extremities are affected by a series of changes exactly comparable with the changes which affected the central part of the shaft in the early beginning of bone-formation. A spot centrally situated in the cartilaginous mass exhibits degenerative changes, bone-forming tissue invades from without, the cartilage disappears, bone taking its place, and the process spreads throughout the mass.

The period of life at which this late independent bone-building occurs in the extremities of any particular bone, such as the thigh-bone, seems to be fairly definite, but the process takes place at widely different periods in the different bones of the skeleton. The circumstances which are to be held responsible factors determining the degeneration of the cartilage in the outlying parts of a bone and its replacement by bony tissue are not fully understood. All that can be said is that, in most cases, it seems to be a reaction to functional necessity, endowing those parts of the bone with the greater resistance which they are called upon to exhibit at some particular stage in the life-history of the individual.

After the bony consolidation of the expanded extremities of the thigh-bone is completed, the cartilage is reduced to thin discs intervening at either end between the bony extremities and the bony shaft. Although attacked in front and rear, the cartilage fights gallantly for some considerable time, always growing, but constantly succumbing to the bony invasions attacking it on either side. As long as the cartilaginous discs persist, so long does the thigh-bone as a whole continue to grow longer and longer. At length the unequal conflict comes to an end, the cartilaginous discs succumb and disappear, the bone-formations meet and join, the bony consolidation of the thigh-bone is complete. This final consolidation is completed at or about the time when the full stature of the individual is attained.

The description of the chief events which succeed one another in the building of the thigh-bone has necessarily been somewhat incomplete, cursory, and disconnected. It may, however, be gathered that the ultimate proper fashioning of the bone is dependent upon the due co-ordination of many different activities. These may be briefly summarised as follows. The laying down of the miniature model of the bone in cartilage; the invasion of the cartilage by the bone-forming tissue; the spread and methodical advance of the bone-forming tissue into the cartilage; the growth and expansion of the cartilage at either end, whereby the gradual increase in length and size is attained; the mutual interdependence of the bone-building cells and the excavating or absorbing cells, the latter of which are not only responsible for attacking and absorbing the cartilage, but are further responsible for excavating some of the deposited bone, and account for the appearance and expansion of the central or marrow cavity and the production of the spaces in the lattice-work formation. As the final result of these manifold activities, not only are the tapering walls of the cylindrical or tubular part of the bone a marvel of mechanical proportion, but every one of the innumerable tiny spicules of bone constituting the lattice-work is in its proper place and position, is of the requisite thickness and length.

The cells concerned in the building of the thigh-bone are to be numbered, not by hundreds or by thousands, but by millions. If a census of the cells concerned in the building of the whole skeleton were taken, the figure would be too vast to convey any intelligible impression of the multitude engaged. The organisation whereby every cell is in its right place, doing its proper amount of work and at the right time, is such as to be almost beyond the limits of human conception. The organisation of the vast armies engaged in the Titanic struggle of the Great War was but a feeble and incompetent effort by comparison.

What are the directing influences responsible for this infinitely complex organisation is a question which naturally suggests itself. The first answer to such a question must be hereditary influence. We have learnt from our forebears; the building cells of the body have been schooled through countless generations. This may be an answer, perhaps, but it is not an explanation, and we are looking to the student of Genetics to discover how the factors influencing form are transmitted from parent to offspring.

The stresses to which the skeleton is subject during the lifetime of the individual contribute another influence which determines to some extent the work of the bone-builders. We know this for a certainty. During its building the thigh-bone undergoes not only an increase in bulk, but also a change in form. Although the form-changes are not excessive, they are easily identified. If it were possible to magnify the thigh-bone of an infant and make it of a size corresponding to the thigh-bone of an adult, the one could be distinguished from the other at a glance. That these changes are a reaction, to some extent at all events, to the pressure and pulls to which the skeleton is subject during early life is proved from the fact that, in cases where unfortunate persons have been bedridden from birth and the skeleton has not been affected by the pressures and pulls to which it is usually subject, the bones, in certain respects, retain their infantile characteristics of shape.

During the last few years a very great deal of attention has been attracted to certain small organs in the body known as the ductless glands, which, to quote Sir Wilmot Herringham, we modestly conceal about our person. They include a small rounded body, usually described as of the shape and size of a cherry, but really no larger than a fair-sized pea, which, contained in the cavity of the skull, is suspended from the base of the brain, slung therefrom by a short stalk. Another ductless gland, no larger than a barleycorn, lies embedded in the recesses of the brain—a small object of some historical interest, as the ancient anatomists regarded it as the seat of the soul. The thyroid gland, a name made familiar by the daily press, occupies the front of the neck, and its situation becomes very obvious when it is enlarged, as is the case in goitre. The list is completed by two small bodies contained in the belly and surmounting the two kidneys, together with a somewhat diffuse tissue termed collectively the interstitial glands. If all the ductless glands were collected together they could easily be accommodated in a small coffeecup. Small as they are, and constituting an inconsiderable fragment of the body as a whole, they yet exert a most profound influence, and are of vital importance to the well-being of the individual. It is known that the ductless glands manufacture substances so infinitesimal in amount that their nature is but obscurely understood. These substances pass from the ductless glands into the blood-stream, whereby they circulate all over the body. The amount that can reach any individual cell of the body must be so inconceivably small as to be quite beyond computation.

These ductless glands and the substances which they manufacture—or internal secretions, as they are termed—are being actively investigated by the physiologist. At the same time they are of very special interest to the anatomist, as they exert a potent influence on the building operations of the body. Moreover, it is known that they can and do modify the building of the skeleton in a truly remarkable manner.

The evidence is chiefly negative, and is mainly founded on the events occurring when the ductless glands are the seat of disease, which may be of more than one kind. Under some abnormal conditions the influence of one of the ductless glands may be diminished, under other circumstances it may actually be increased. It is known, for example, that in certain conditions of one of the ductless glands the processes of bone-formation run riot, and the bones, especially those of the head and face, assume monstrous size and shape.

One of the most interesting discoveries is that both unusual shortness, or dwarfism, and unusual tallness, or giantism, may be accounted for by disorder of these little glands. This discovery has a definite bearing upon the present theme, as in giantism not only must the building processes in the thigh-bone, as well as in the other bones of the skeleton, take place more rapidly, the excessive growth of the body as a whole being manifest long before the period at which full stature is usually attained, but, further, they must persist for a longer time than is normally the case, as the time during which the body is growing is usually prolonged. In dwarfism, on the other hand, the building processes must take place less rapidly, and come to an end at an earlier period of life.

So far as the thigh-bone is concerned, the growth in length is mainly dependent on the growth and expansion of the cartilage at its two extremities, and it is conceivable that in giantism the internal secretions circulating in the blood have a specially stimulating effect on the growth of the cartilage, the result being that not only does it expand more rapidly, but, maintaining the struggle for existence for a longer time, does not succumb so easily to the invasion of the bone-forming tissue.

On the other hand, in dwarfism the internal secretions may be altered in character in such a way as to have a depressing effect on the growth of the cartilage, which may expand less rapidly than usual, exhibit less resistance to the invasion of the bone-forming tissue, and disappear in comparatively early youth.

This simple explanation will, however, not suffice to account for all the influences the ductless glands may bring to bear upon bone-growth. We are well assured that not only are the bulk and length of a bone such as the thigh-bone modified by the influence of internal secretions, but its actual shape and general conformation can be altered by alterations in the ductless glands.

Mankind is not built to order from one standard pattern. The Chinaman cannot be mistaken for a Negro, or a Negro for a European. Hair, colour, shape, and a multitude of superficial peculiarities serve to distinguish the different races at a glance. These racial differences are not skin-deep, confined to the surface of the body, but affect the skeleton itself. Skull, limb, and other bones exhibit their own racial characteristics of shape, and can be recognised by their form-peculiarities.

In some disorders of the ductless glands the European may exhibit form-changes, changes which affect the skeleton as well as other parts, recalling the traits of the Chinaman, so much so that 'Mongolian' is a term in common use to describe the peculiar features of these cases. On the other hand there is evidence to show that the Chinaman may exhibit characteristics peculiar to the European, owing to what may be termed for the Chinaman a perversion of the secretion of his ductless glands.

Thus is growing the belief that the secret of race-differentiation is hidden in the ductless glands, that differences in external appearances and conformation of skeleton are dependent on indiscernible differences in these glands and in the character of the internal secretions they furnish.

Holding this belief, one is driven to the conclusion that the ductless glands must influence the ultimate conformation of the thigh-bone, must determine the quality as well as the quantity of its building. The shape of the future bone will depend somewhat on the shape of the cartilaginous model preceding it, but only to a limited extent. The shape, ever changing during growth, must be largely accounted for by the co-ordinated activities of the bone-builders and the bone-excavators. As the sculptor, gradually moulding a mass of clay into some beautiful form, will often add here and take away there to obtain the shape desired, so the bone-builders, by adding here, the excavators, by taking away there, are largely accountable for the fashioning of the bone into its ultimate shape. The multiple and mutually inter-dependent activities of these millions upon millions of cells, whether they be cartilage cells, bone-builders, or excavators, seem to be regulated and co-ordinated by the quintessences distilled in the mysterious laboratories of the ductless glands.

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