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> *Of Nature trusts the mind which builds for aye.*"—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

THURSDAY, SEPTEMBER 5, 1912.

EARLY NATURALISTS.

The Early Naturalists. Their Lives and Work (1530–1789). By Dr. L. C. Miall, F.R.S. Pp. xi+396. (London: Macmillan and Co., Ltd., 1912.) Price 10*s.* net.

IN this account of the naturalists who worked and wrote during the period between the commencement of the Protestant Reformation and that of the French Revolution, Prof. Miall has placed under a considerable obligation those who are interested in the advancement of natural knowledge. The period to which the work is in the main limited constitutes perhaps as natural an epoch as may be found in human history. Whether the period be natural or not, the charming introductory sketch of "Natural History down to the Sixteenth Century" fully justifies the selection of the date at which the author's account of scientific progress formally opens, while the closing date adopted is at least convenient. But the work is one that could only have been written with unusually full knowledge of the scientific happenings since the date of Buffon's death, and it is owing to the possession of this knowledge that the author has been able to assess so authoritatively as he does the extent and the value of the permanent additions to biological truth which marked the period he passes under review.

The work, in the main, deals, as its title implies, with the lives and the labours of the naturalists who flourished during the period in question. In his treatment of the subject Professor Miall strikes a happy mean between the methods of the skilled biographer and of the formal historian of human progress. As a result, he succeeds in enabling the reader to acquire a clear conception not only of what was accomplished during the period, but of the character of those by whom the work was done and of the intellectual atmosphere in which they lived. To the personal interest thus aroused is largely due the force of the incisive estimates provided by the author of men like Clusius and Belon, Ray and Leeuwenhoek, Réaumur and Buffon, to mention only a few of the worthies whose lives are discussed. Even in those rare instances in which the reader may feel inclined to differ from Prof. Miall, it will be admitted that his estimates are the result of complete knowledge and judicial thought; any disinclination to accept the verdicts depends not on the facts, but on the point of view from which these facts are regarded.

There is, however, a certain want of unity in the work. In addition to the accounts of individual naturalists which we conclude from the title to be its main subject, the book contains a series of essays of a different type, each of them as self-contained as the character-sketches of which the work is principally composed. One of these, already alluded to, aptly serves as an introduction to these sketches. Another, on "The Natural History of Distant Lands," is interpolated between the accounts of the earlier Continental and the earlier English naturalists, but scarcely serves as a connecting link between the one group and the other. This essay is, however, so interesting in itself that one welcomes it as a digression, which at least does not carry us beyond the later limit of the period discussed, and may be excused for taking us back further than its earlier one.

Two similar essays, equally self-contained, on "The Investigation of the Puss Moth," and on "Early Studies of the Flower," which are not accorded the position of distinct sections, but are incorporated in other sections, deviate more considerably from the plan of the work as a whole; the former brings us down to the present day, while the latter carries us from Theophrastus to the first De Candolle. Still, both essays are germane to the purpose of the book, and add so much to its value that it would be more than ungracious to cavil at their presence among these delightful and informing sketches of the "Early Naturalists."

THE WANDERING OF THE BRONZE AGE POTTERS.

A Study of the Bronze Age Pottery of Great Britain and Ireland, and its associated Grave-goods. By the Hon. John Abercromby. Vol. i., pp. 163+lxi plates. Vol. ii., pp. 128+plates lxii–cx. (Oxford: Clarendon Press, 1912.) Two volumes, price £3 3*s.* net.

ARCHÆOLOGISTS have long been looking forward to the Hon. Dr. John Abercromby's monograph on Bronze Age pottery, and, as was to be expected, it has proved to be exhaustive and workmanlike. As an indication of the pains which the author has taken, it may be mentioned that there are photographs of 54 Continental beakers, 291 British beakers, 421 food vessels, 570 cinerary urns, numerous photographs of other objects, several plates of details of ornamentation, and a number of valuable maps of distributions. A classified list of the vessels illustrated in the plates would save the reader a great deal of trouble. The purely descriptive matter is as succinct as possible, though all essential information is given, and as there are full references the student knows where to go for further details.

Not only have we data of form, ornamentation, and distribution, but Dr. Abercromby has sought to make them tell a tale by coordinating other finds, such as skulls, implements, beads, &c. He rightly endeavours to give a picture of the life of the people, but some of his speculations on their social condition and religious beliefs are too hypothetical, and are scarcely consistent with the scientific method he adopts when dealing with his immediate subject. His general conclusions may be summarised as follows. About 2000 B.C. it would seem that Britain was invaded by a rugged, enterprising people, mainly of Alpine stock, whose ancestors, perhaps three to four hundred years earlier, had lived beyond the Rhine, not very far north of Helvetia. They had scarcely emerged from the neolithic stage of culture, and perhaps brought no single copper or bronze knife among them, but not long afterwards they possessed such small implements, and perhaps flat axes. Their wealth must have consisted in cattle, sheep, goats, and swine. They were also acquainted with cereals. They were not an inventive people, for they had only two forms of sepulchral pottery, which lasted with small variations for about 500 years, and they never abandoned geometrical ornamentation. Women were buried with as much ceremony as men. They presumably spoke an Aryan language.

The invaders probably landed on the coast of Kent, and in course of time some moved north and others west; these began to cluster on the Wiltshire downs, especially round what is now Stonehenge. About 1880 B.C. the northern branch crossed the Humber into East Riding, where they also found the earlier natives in possession. About this time their influence had reached Hibernia, in the shape of a beaker, though they themselves may not have crossed over so early. Not until about 1600 did they colonise the south coast of Moray Firth, and the extreme north was reached some time later. By 1500 B.C. the direct evidence of the brachycephalic invaders ceases. In the south their ceramic ended, and the skull-type was obliterated by cremation; but they were not exterminated. It is not unlikely that Stonehenge was erected about 300 years after the invasion.

About 1350–1150 there was a remarkable development of material civilisation in south Britain, new forms of small, often beautifully made cups are first met with, and there were skilful artificers in gold; traces of foreign influences are also met with. From about 1150 to 900 B.C. is an obscure period, with diminished material wealth. During the next period (*circa* 900–650), south Britain was entered by new tribes, apparently refugees, who introduced a new form of entrenchment and new forms of pottery, some of which have analogies east of the Rhine, others about the northern base of the Pyrenees. There is no evidence that they spread north of the Thames. During the period beginning *circa* 900, the population increased, and the dead were interred in flat cemeteries, though barrows never fell entirely into disuse; the change was not due to foreign influence, as the contemporary pottery from cemeteries and barrows is identical. The period from 650–400 is obscure; in remote parts like Dorset and Ross-shire, the Bronze Age certainly lasted till about 200 B.C.

This admirable monograph breaks new ground, and will long remain the standard work on the early Bronze Age of the British Islands.

A. C. HADDON.

OUR BOOKSHELF.

The Inter-Relationships of the Bryophyta. By Dr. Frank Cavers. Reprinted from the *New Phytologist.* Pp. vi + 203. Cambridge: At the Botany School, 1911. Price 4*s.*; postage 4*d.*

WE are a little late in announcing that Dr. F. Cavers's series of articles which appeared on the inter-relationships of the Bryophyta in the *New Phytologist,* vols. ix. and x., 1910–11, has been issued separately. It is a great convenience to have the work in this form, and it certainly deserves this distinction. The classification is mainly that adopted in Engler and Prantl's "Natürlichen Pflanzenfamilien," but as a result of his investigations the author introduces some modifications. His proposed divisions are: (1) Sphærocarpales, (2) Marchantiales, (3) Jungermanniales, (4) Anthocerotales, (5) Sphagnales, (6) Andreæales, (7) Tetraphidales, (8) Polytrichales, (9) Buxbaumiales, and (10) Eu-Bryales.

Dr. Cavers discusses more particularly the question of the old primary division of the Bryophyta into two classes, Hepaticæ and Musci, especially in relation to the Anthocerotales and the Sphagnales. He argues: "If the Anthocerotales are to be made a separate class apart from the Hepaticæ, either Sphagnales should also be considered a separate class apart from the Musci, thus making four primary divisions of Bryophyta—Hepaticæ proper, Anthocerotes, Sphagna, and Musci proper—or the Anthocerotales and Sphagnales might be united to form a class between the Eu-Hepaticæ and the Eu-Musci, thus giving three classes of Bryophyta." But he prefers dividing the Bryophyta into ten groups as designated above.

The account of *Riella capensis* is of special interest, and it is to be followed by a more detailed paper on the genus generally. Until 1902 this singular aquatic genus was only known to inhabit the Mediterranean region and the Lake of Geneva. Since then a species has been discovered in the Grand Canary; another in Texas; a third in Turkestan; and a fourth in South Africa.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Determination of the Epicentre of an Earthquake.

IT has been proved by observation with the Galitzin seismographs, both at Pulkowa and Eskdalemuir, that when the first phase P of an earthquake is sharp, the azimuth of the epicentre from the station is uniquely determined by the observations at that station. It follows that if the azimuth of the epicentre is determined at two independent stations suitably situated, the epicentre can be determined from these two azimuths alone.

We have to-day, as an example, verified by construction and by computation that this principle gives accurately the epicentre of the earthquake that occurred in Monastir on February 18, 1911.

The azimuth observed at Pulkowa was 22° 53′ west of south, while the azimuth observed at Eskdalemuir was 55° 56′ east of south. The resulting epicentre we find to be 40·5° N., 20·3° E.

The epicentre deduced by the Pulkowa observations of azimuth and epicentral distance was 40·5° N., 20·1° E.; while the similar deduction from the Eskdalemuir results was 40·3° N., 20·4° E.

It is clear that in this case the accuracy of determination from the azimuths alone equals that of the determinations from the separate stations, and it is known that the earthquake did occur in the region indicated.

The advantages of this new method based on azimuths alone are:—

(1) That it is quite independent of any time reckoning whatever at the two stations.

(2) That it is independent of the determination of the second phase S on a seismogram (which is frequently difficult to fix with certainty).

(3) That it is independent of any empirical tables for epicentral distance, which are admittedly only approximate.

(4) Although only two stations are used, the determination is unique.

We may observe that for a given case the accuracy of determination depends on a suitable choice of the two stations.

B. GALITZIN.
GEORGE W. WALKER.

The Observatory, Eskdalemuir, Langholm,
Dumfriesshire, August 29.

Implements of Man in the Chalky Boulder Clay.

IN NATURE of August 15 Mr. Reid Moir has given us certain interesting facts observed by him in connection with the scratching of flints.

(1) He notes the occasional scratching of what remains of the "cortex" of the original nodule. It does not seem to have occurred to him that such a result may have been produced while the flint was still enclosed in its original chalk matrix. Topley (in his "Geology of the Weald") showed long ago that the chalk strata had in many cases undergone considerable differential movement concomitant with crustal movements; and I have myself seen crushed flints still *in situ* in the chalk cliffs at Ventnor, where there is evidence of intense crustal movement of the strata. So far back as 1880 I noted this, also the extremely fractured and unworn condition of the flints left as a residuum from the solution of the chalk by carbonated rain-water on the top of St. Boniface Downs (see P.G.A., vol. viii., No. 3), and my interpretation of the phenomena there observable has since been confirmed by Dr. A. Strahan, F.R.S., of the Geological Survey. Here we have a sufficient mechanical cause totally independent of anything that may be connoted by the term "glaciation." There seemed, moreover, to be just that slight amount of surface-staining of the fractured surfaces which might be due to meteoric iron-dust.

(2) One fails to see that there is any mystery about the non-striated condition generally of the fracture-surfaces of the flint fragments from the Boulder Clay. How could the soft matrix of the Boulder Clay scratch a flint, or even hold a harder stone with sufficient grip to give it effect as a graving-tool, however great the volume-pressure may have been? When the "glazier" wants to cut glass he does not use putty to hold his "diamond." So much for the talk of "intense glaciation" of hypothetical pre-Crag flints, on which I hope to have shortly more to say.

On the other hand, boulders of the Chalk itself, if at all rounded by the shearing movement of the ice in which they were once embedded, are often scratched and grooved. (See further my paper on the mechanics of glaciers, Q.J.G.S., February, 1883; also NATURE, June 20, 1912.)

I can assure Mr. Reid Moir that the delicate and interesting subject of *patination* presents difficulties to those who (in microscopic and laboratory work) have brought some knowledge of physics and chemistry to bear on the *lithology of the flint*, and that it is not to be dismissed in the easy way he seems to suppose. Nor do I think that even Dr. Sturge (Proc. Prehis. Soc. of E. Anglia) has adequately dealt with the subject or with the possible causes of some phases of "striation." A. IRVING.

Bishop's Stortford, August 22.

THE FIFTH INTERNATIONAL CONGRESS OF MATHEMATICIANS AT CAMBRIDGE.

THE first Mathematical Congress was held at Zurich in 1897, the second at Paris in 1900, the third at Heidelberg in 1904, and the fourth at Rome in 1908. This year's congress met at Cambridge, August 21–28, under the presidency of Sir G. H. Darwin, and was divided into sections as follows:—I, Analysis; II, Geometry; III (*a*), Physical Mathematics; III (*b*), Statistics; IV (*a*), Philosophy and History; IV (*b*), Didactics. Several meetings of the last section were held in connection with the International Commission on the Teaching of Mathematics, which was formed by a resolution of the fourth congress to study and report on the actual state of mathematical teaching in various countries.

Receptions were given by the Chancellor, Lord Rayleigh, in the Fitzwilliam Museum, by Sir G. H. Darwin in St. John's and Christ's Colleges, and by the Master and Fellows of Trinity College. Visits were made to the University observatory and to the works of the Cambridge Scientific Instrument Company. Excursions were arranged to Ely Cathedral, Oxford and Hatfield House. Throughout the week the University and colleges displayed their customary hospitality to the full, and the appreciation of the visitors, both English and foreign, was very evident. The members numbered 572, as compared with 535 at the fourth congress, and included representatives from Brazil, Chile, Egypt, India, Japan, and Mexico. An exhibition organised by the Mathematical Association was arranged in the Cavendish Laboratory, and included English and foreign text-books, examples of school work, models and apparatus, and a most interesting and complete collection of calculating machines. Eight lectures were delivered to the whole congress, and we mention below a few of the less technical points occurring in these, and in the meetings of the didactic section.

Sir G. H. Darwin (Cambridge), in welcoming the congress at the first meeting, referred to the death of Henri Poincaré, whom he described as the one man who alone of all mathematicians might have occupied the position of president of the congress without misgivings as to his fitness. It brought vividly home to him how great a man Poincaré was, when he reflected that, to one incompetent to appreciate fully one half of his work, he yet appeared as a star of the first magnitude.

Prof. E. W. Brown (Yale) lectured on "Periodicity in the Solar System." Newton and his contemporaries aimed at obtaining functions which should express the positions of individual bodies at all epochs. This is now recognised as unattainable; and the position within certain limits of time is expressed by infinite series of terms, some of which are harmonic representing periodic motions, and others expressed as powers of the time representing secular motions. These series are carried to a degree of accuracy exceeding that of the most delicate observation; so that where the calculated positions differ from those observed by a quantity exceeding the possible error of observation, it may be safely assumed that forces are in action other than those postulated in the theory. This is notably the case in the theory of the moon, where the outstanding discrepancy is comparable with the largest of the perturbations due to the planets. Dynamical theory in the case of the asteroids has shown that in the particular case of the problem of four bodies when the mass of one is small, the motion of the latter is unstable for certain ranges of value of the radius vector; and no asteroids have, in fact, been found within these limits. It is possible that an explanation may here be foreshadowed of the dark intervals in Saturn's rings.

Prince B. Galitzin (St. Petersburg) lectured on "The Principles of Instrumental Seismology." The usual seismographic record shows three chief groups of disturbances, due respectively to the longitudinal and transverse waves through the core of the earth, and to the superficial wave round the crust. These, however, are complicated and supplemented by reflections of the deep waves at the surface, and sometimes also by twin earthquakes caused by the primary. The relations between the elastic constants of the core deduced from seismographic observations are in fair agreement with the theory of elasticity of an isotropic medium. But an attempt has been made to construct a more general theory assuming heterogeneity depending on depth. The ideal aim of seismometry must be the determination of the six components of motion of a particle of the earth's crust throughout the whole of a disturbance. Hitherto attention has been confined to the three components of translation. The practical problem of recording the three components of rotation seems to have been solved recently in an apparatus in which induced currents from two pendulums are passed simultaneously in opposite directions through the same galvanometer. There is even reason to believe that the problem of predicting earthquakes is not so hopeless as it would *a priori* seem to be.

Sir W. H. White lectured on "The Place of Mathematics in Engineering Practice." It is matter for surprise that many of the great engineering discoveries of the last century were made by men who had little or no mathematical or scientific training. On the other hand, much good work was done by French mathematicians in the eighteenth century in laying the foundations of naval architecture. The discussions of recent years have tended to the conclusion that the mathematical portion of an engineer's training is best given in the regular manner by a mathematician, rather than in a selected course by an engineer. There can be no doubt as to the value of mathematics, both in indicating the lines along which experiments must be made and in framing a theory from their results. Many problems, such as that of the design of ship propellers, stand urgently in need of the mathematician's help.

At an extra meeting of Section IV, Mr. P. J. Harding lectured on "The History and Evolution of Arithmetic Division." The two methods of calculation prevalent in Europe previous to the introduction of the Arabic numerals were that of the algorists, who used counting-boards ruled with lines representing successive powers of ten on which counters were placed, and that of the abacus. Arabic numerals followed the trend of commerce from India through Arabia and Italy into northern Europe; so far as we know, they first appeared in Italy in 1202. Subtraction was first performed from the left by scratching out the digits successively, a method evolved from the sand-board used in the East, which was small compared with the size of the numerals, so that successive deletion was necessary. From this followed the method of division by scratching, known as the galleon method owing to a fancied resemblance of the resulting disposition of digits to the form of a ship. The modern method of division first appeared in print in Italy in 1494, but it only superseded the galleon method after a struggle which lasted more than a century. In England its ultimate triumph was largely due to the writing-master Cocker, who advocated it to the exclusion of the older method.

At a special meeting of Section III (*a*), Sir J. J. Thomson (Cambridge) gave a lecture illustrated by experiments on "Multiply Charged Atoms," in which he described some recent investigations on positive charges. He explained the parabolic grouping effected by the simultaneous action of electric and magnetic fields, and showed photographs of the parabolic arcs obtained in various particular cases. In the case of mercury atoms, eight such arcs were obtained, due to one or more of the charges originally carried being lost in transit, so that the particles arrived at the screen with their original energy but with reduced charge.

At an ordinary meeting of Section IV, Dr. A. N. Whitehead (London) read a paper on the principles of mathematics in relation to elementary teaching. The only justification for the inclusion of mathematics in a liberal education is the power of abstraction and deductive reasoning fostered thereby. These powers can only be acquired by constant practice, and no short-cuts are possible. But this does not imply that such powers are to be assumed in the pupil from the outset. On the contrary, no generalisation can be made by the pupil until he is familiar with the raw material from which it is to be made. There is no final degree of rigour in deduction, and the degree to be adopted is a matter for the teacher to decide. His personal choice would be approximately the degree of rigour, though not necessarily the content of Euclid's Elements. No compromise is desirable between the purely utilitarian procedure of looking up a formula in an engineering pocket-book and the acquisition of a mathematical habit of mind by years of practice in abstraction and deduction.

Mr. G. E. St. L. Carson (Tonbridge) read a paper on the place of deduction in elementary mechanics. He suggested that, besides the old method of teaching mechanics in which a structure of deduction was raised on a few postulated laws, and the new method in which principles are demonstrated independently by experiment, there is a third method possible in which the logical interdependence of the principles demonstrated is discussed. Not only is this an aid to understanding the foundations of the subject, but they are shown to constitute a broad inductive basis.

A paper by Dr. T. P. Nunn (London) was read on the proper scope and method of instruction in the calculus in schools. He advocated the teaching of integration by means of graphical illustration on the lines originally adopted by Wallis. This should be followed by a consideration of differentiation as the converse geometrical problem. The teacher should avoid all use of such mystic phrases as "infinite" and "ultimately become," keeping carefully to the definition of limit in terms of finite quantities.

At meetings of Section IV (*b*), in conjunction with the International Commission on the Teaching of Mathematics, reports were presented, with a few explanatory remarks, by delegates from twenty-one countries. The reports exceeded 280 in number, forming an aggregate of more than 9000 octavo pages. These may be obtained from Messrs. Georg et Cie., of Geneva; the English reports have recently been issued in two volumes by the Board of Education. The commission was reappointed for a further period of four years, in order that a digest of these reports may be prepared for the use of teachers in each country. The commission has also conducted special investigations, and reports were presented on the results of two of these.

Prof. C. Runge (Göttingen) presented a report on the mathematical training of the physicist in the university. The need for the closer cooperation of the mathematician and the physicist is strongly felt. It would be of benefit not only to the future physicist or engineer, but also to the student of pure mathematics, if in mathematical lectures theoretical solutions were followed up by numerical computations and applications to material problems. It is also felt that mathematical teaching in the university would be improved if the lecturer were assisted by demonstrators who could keep in personal touch with the student, and aid him as difficulties arise. In commenting on this report, Profs. Hobson, Love, and Sir J. Larmor were of opinion that to limit the mathematics of science students to those portions which might be considered of direct utility would destroy that logical unity which is the essential feature of the subject, and relegate it to a subservient position little in keeping with its importance. Sir A. G. Greenhill uttered a warning against the excessive attention engineering pupils are apt to give to descriptive geometry, to the detriment of their studies in the calculus. Sir J. J. Thomson was in favour of physicists learning mathematics from pure mathematicians, if the latter would reserve some of their latest refinements for special lectures.

Prof. D. E. Smith (New York) presented a report on intuition and experiment in mathematical teaching in secondary schools. The object of the inquiry was to ascertain to what extent intuitional methods are at present employed. A general spirit of unrest is apparent. In geometry it may be said that it is the plan of the Teutonic countries to mix the intuitional and deductive work from the outset, while in France, and now in England, the plan is to let an inductive cycle precede a deductive one. The United States is only beginning to talk about the question, whatever tendency there is being towards the Anglo-French plan. The second important movement is the elaboration of the function concept; starting in France within the last twenty years, and vigorously advocated in Germany within the last decade, the movement is, as a whole, too recent to judge of its permanence. A practical form of outdoor mensuration seems to be developing, especially in Austria, Germany, and Switzerland. Geometric drawing and the graphic representation of solids are passing from the hands of the art teacher to the mathematician. Graphic methods of representing functions have become universal in the last generation. The contracted methods of computation that were prominently advocated fifty years ago do not seem to have advanced materially, owing to the feeling that they are not really practical; on the other hand, logarithms have come into general use, and the slide rule is in great favour in technical schools. In general, it may be said that intuitional and experimental methods have made more progress in Austria, Germany, and Switzerland than in England, France, and the United States.

At the final meeting of the congress it was resolved to accept the invitation to Stockholm for the next meeting in 1916. Informal invitations to Budapest and Athens for subsequent meetings were also noted.

THE BRITISH ASSOCIATION AT DUNDEE.

BY the time this issue reaches the reader the British Association will be in full session, and meanwhile there seems to be every prospect of an unusually successful meeting. Dundee is a town of comparatively small population, largely made up of the working classes, but the number of persons resident in the town and neighbourhood who have joined the Association is remarkable. The various towns in which the Association meets are found to differ greatly in this respect, and it occasionally happens that the number of local associates is exceedingly small. Since the year 1901 the Association has held its annual meetings on two occasions abroad and on nine occasions at places within the United Kingdom. The average number of tickets sold at these nine centres before the opening of the reception rooms is 460, and the highest number so sold at any one of the nine was 643; but considerably more than 1100 tickets had already been sold in Dundee by the local committee before the opening of the reception rooms, and by Tuesday evening some 2000 tickets were issued.

This large local addition to the ordinary membership of the Association, together with the unusually large attendance of foreign, American, and Colonial guests, however gratifying it may be to the officers of the Association, renders the task of the local committee a difficult and anxious one. The various halls and Section rooms will be taxed to the utmost, and the various excursions and entertainments will scarcely be sufficient for an attendance so greatly in excess of the estimates that were based on the statistics of recent meetings.

As has already been stated in these columns, the attendance of scientific men from abroad is unusually great, beyond anything indeed that has been seen since the great meeting at Manchester; and this large gathering of foreigners has had its effect in helping to attract the scientific men of our own country. Within the last few days a number of eminent mathematicians, who have attended the recent congress at Cambridge, have made known their intention to be present; geologists are mustering in strength from many countries, tempted to a large extent by the promise of excursions of unusual interest, and a still larger gathering of notable physiologists are coming to do honour to a physiological President.

Every nook and corner of the town is filled almost to overflowing, and members who arrive without having made their arrangements beforehand will have little chance of finding even the simplest houseroom. Private hospitality has provided for between 700 and 800 guests, and every hotel in the town and in the near neighbourhood was filled up many days ago.

It is sometimes said that the British Association is losing ground, but the experience of this meeting shows that the belief is without foundation; not only is the attendance this year fully comparable to the average attendance in the best days of the Association, but there is every prospect also of animated discussion and abundant scientific work. We print this week the inaugural address delivered last night by the president, Prof. E. A. Schäfer, F.R.S., and also the address to be delivered by Prof. H. L. Callendar, F.R.S., before Section A this morning. Other addresses, and reports of the proceedings of the various Sections, will appear in later issues.

Inaugural Address by Prof. E. A. Schäfer, LL.D., D.Sc., M.D., F.R.S., President.

Introductory.

It is exactly forty-five years ago—to the day and hour—that the British Association last met in this city and in this hall to listen to a Presidential Address. The President was the Duke of Buccleuch; the General Secretaries, Francis Galton and T. Archer Hirst; the General Treasurer, William Spottiswoode; and the Assistant General Secretary, George Griffith, who was for many years a mainstay of the Association. The Evening Discourses were delivered by John Tyndall "On Matter and Force," by Archibald Geikie "On the Geological Origin of the Scenery of Scotland," and by Alexander Herschel "On the Present State of Knowledge regarding Meteors and Meteorites." The Presidents of Sections, which were then only seven in number, were for Mathematics and Physics, Sir William Thomson—later to be known as Lord Kelvin; for Chemistry, Thomas Anderson; for Geology, Archibald Geikie, who now as President of the Royal Society worthily fills the foremost place in science within the realm; for Biology, William Sharpey, my own revered master, to whose teaching and influence British physiology largely owes the honourable position which it at present occupies; for Geography, Sir Samuel Baker, the African explorer, who with his intrepid wife was the first to follow the Nile to its exit from the Albert Nyanza; for Economic Science, Mr. Grant Duff; and for Mechanical Science, Professor Rankine.

Other eminent men present were Sir David Brewster, J. Clerk Maxwell, Charles Wheatstone, Balfour Stewart, William Crookes, J. B. Lawes and J. H. Gilbert (names inseparable in the history of agricultural science), Crum Brown, G. D. Liveing, W. H. Russell, Alexander Williamson, Henry Alleyne Nicholson, William Allmann, John Hutton Balfour, Spencer Cobbold, Anton Dohrn, Sir John Lubbock (now Lord Avebury), William McIntosh, E. Ray Lankester, C. W. Peach, William Pengelly, Hughes Bennett, John Cleland, John Davy, Alexander Christison, Alfred Russel Wallace, Allen Thomson, William Turner, George Busk, Michael Foster (not yet founder of the Cambridge School of Physiology), Henry Howorth, Sir Roderick Murchison, Clements R. Markham, Sir William (afterwards Lord) Armstrong, and Douglas Galton. Many of those enumerated have in the course of nature passed away from us, but not a few remain, and we are glad to know that most of these retain their ancient vigour in spite of the five-and-forty years which separate us from the last meeting in this place.

Selection of Subject of Address.

For the Address with which it is usual for the President to open the proceedings of the annual assembly, the field covered by the aims of the British Association provides the widest possible range of material from which to select. One condition alone is prescribed by custom, viz., that the subject chosen shall lie within the bounds of those branches of knowledge which are dealt with in the Sections. There can be no ground of complaint regarding this limitation on the score of variety, for within the forty years that I have myself been present (not, I regret to say, without a break) at these gatherings, problems relating to the highest mathematics on the one hand, and to the most utilitarian applications of science on the other, with every possible gradation between these extremes, have been discussed before us by successive Presidents; and the addition from time to time of new Sections (one of which, that of Agriculture, we welcome at this Meeting) enables the whilom occupant of this chair to traverse paths which have not been previously trodden by his predecessors. On the last two occasions, under the genial guidance of Profs. Bonney and Sir William Ramsay, we have successively been taken in imagination to the glaciers which flow between the highest peaks of the Alps and into the bowels of the earth; where we were invited to contemplate the prospective disappearance of the material upon which all our industrial prosperity depends. Needless to say that the lessons to be drawn from our visits to those unaccustomed levels were placed before us with all the eloquence with which these eminent representatives of Geology and Chemistry are gifted. It is fortunately not expected that I should be able to soar to such heights or to plunge to such depths, for the branch of science with which I am personally associated is merely concerned with the investigation of the problems of living beings, and I am able to invite you to remain for an hour or so at the level of ordinary mortality to consider certain questions which at any rate cannot fail to have an immediate interest for everyone present, seeing that they deal with the nature, origin, and maintenance of life.

Definition.

Everybody knows, or thinks he knows, what life is; at least, we are all acquainted with its ordinary, obvious manifestations. It would, therefore, seem that it should not be difficult to find an exact definition. The quest has nevertheless baffled the most acute thinkers. Herbert Spencer devoted two chapters of his "Principles of Biology" to the discussion of the attempts at definition which had up to that date been proposed, and himself suggested another. But at the end of it all he is constrained to admit that no expression had been found which would embrace all the known manifestations of animate, and at the same time exclude those of admittedly inanimate, objects.

The ordinary dictionary definition of life is "the state of living." Dastre, following Claude Bernard, defines it as "the sum total of the phenomena common to all living beings."[1] Both of these definitions are, however, of the same character as Sydney Smith's definition of an archdeacon as "a person who performs archidiaconal functions." I am not myself proposing to take up your time by attempting to grapple with a task which has proved too great for the intellectual giants of philosophy, and I have the less disposition to do so because recent advances in knowledge have suggested the probability that the dividing line between animate and inanimate matter is less sharp than it has hitherto been regarded, so that the difficulty of finding an inclusive definition is correspondingly increased.

Life not Identical with Soul.

As a mere word "life" is interesting in the fact that it is one of those abstract terms which has no direct antithesis; although probably most persons would regard "death" in that light. A little consideration will show that this is not the case. "Death" implies the pre-existence of "life"; there are physiological grounds for regarding death as a phenomenon of life—it is the completion, the last act of life. We cannot speak of a non-living object as *possessing* death in the sense that we speak of a living object as *possessing* life. The adjective "dead" is, it is true, applied in a popular sense antithetically to objects which have never possessed life; as in the proverbial expression "as dead as a door-nail." But in the strict sense such application is not justifiable, since the use of the terms dead and living implies either in the past or in the present the possession of the recognised properties of living matter. On the other hand, the expressions *living* and *lifeless, animate* and *inanimate*, furnish terms which are undoubtedly

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[1] "La vie et la mort," English translation by W. J. Greenstreet, 1911, p. 54

antithetical. Strictly and literally, the words animate and inanimate express the presence or absence of "soul"; and not infrequently we find the terms "life" and "soul" erroneously employed as if identical. But it is scarcely necessary for me to state that the remarks I have to make regarding "life" must not be taken to apply to the conception to which the word "soul" is attached.

Problems of Life are Problems of Matter.

The fact that the formation of such a conception is only possible in connection with life, and that the growth and elaboration of the conception has only been possible as the result of the most complex processes of life in the most complex of living organisms, has doubtless led to a belief in the identity of life with soul. But unless the use of the expression "soul" is extended to a degree which would deprive it of all special significance, the distinction between these terms must be strictly maintained. For the problems of life are essentially problems of matter; we cannot conceive of life in the scientific sense as existing apart from matter. The phenomena of life are investigated, and can only be investigated, by the same methods as all other phenomena of matter, and the general results of such investigations tend to show that living beings are governed by laws identical with those which govern inanimate matter. The more we study the manifestations of life, the more we become convinced of the truth of this statement and the less we are disposed to call in the aid of a special and unknown form of energy to explain those manifestations.

Phenomena Indicative of Life: Movement.

The most obvious manifestation of life is "spontaneous" movement. We see a man, a dog, a bird move, and we know that they are alive. We place a drop of pond water under the microscope, and see numberless particles rapidly moving within it; we affirm that it swarms with "life." We notice a small mass of clear slime changing its shape, throwing out projections of its structureless substance, creeping from one part of the field of the microscope to another. We recognise that the slime is living; we give it a name—*Amoeba limax*—the slug amœba. We observe similar movements in individual cells of our own body; in the white corpuscles of our blood, in connective tissue cells, in growing nerve cells, in young cells everywhere. We denote the similarity between these movements and those of the amœba by employing the descriptive term "amœboid" for both. We regard such movements as indicative of the possession of "life"; nothing seems more justifiable than such an inference.

Similarity of Movements in Living and Non-living Matter.

But physicists[2] show us movements of a precisely similar character in substances which no one by any stretch of imagination can regard as living; movements of oil drops, of organic and inorganic mixtures, even of mercury globules, which are indistinguishable in their character from those of the living organisms we have been studying: movements which can only be described by the same term amœboid, yet obviously produced as the result of purely physical and chemical reactions causing changes in surface tension of the fluids under examination.[3] It is therefore certain that such movements are not specifically "vital," that their presence does not necessarily denote "life." And when we investigate closely even such active movements as those of a vibratile cilium or a phenomenon so closely identified with life as the contraction of a muscle, we find that these present so many analogies with amœboid movements as to render it certain that they are fundamentally of the same character and produced in much the same manner.[4] Nor can we for a moment doubt that the complex actions which are characteristic of the more highly differentiated organisms have been developed in the course of evolution from the simple movements characterising the activity of undifferentiated protoplasm; movements which can themselves, as we have seen, be perfectly imitated by non-living material. The chain of evidence regarding this particular manifestation of life—movement—is complete. Whether exhibited as the amœboid movement of the proteus animalcule or of the white corpuscle of our blood; as the ciliary motion of the infusorian or of the ciliated cell; as the contraction of a muscle under the governance of the will, or as the throbbing of the human heart responsive to every emotion of the mind, we cannot but conclude that it is alike subject to and produced in conformity with the general laws of matter, by agencies resembling those which cause movements in lifeless material.[5]

Assimilation and Disassimilation.

It will perhaps be contended that the resemblances between the movements of living and non-living matter may be only superficial, and that the conclusion regarding their identity to which we are led will be dissipated when we endeavour to penetrate more deeply into the working of living substance. For can we not recognise along with the possession of movement the presence of other phenomena which are equally characteristic of life and with which non-living material is not endowed? Prominent among the characteristic phenomena of life are the processes of assimilation and disassimilation, the taking in of food and its elaboration.[6] These, surely, it may be thought, are not shared by matter which is not endowed with life. Unfortunately for this argument, similar processes occur characteristically in situations which no one would think of associating with the presence of life. A striking example of this is afforded by the osmotic phenomena presented by solutions separated from one another by semipermeable membranes or films, a condition which is precisely that which is constantly found in living matter.[7]

Chemical Phenomena accompanying Life.

It is not so long ago that the chemistry of organic matter was thought to be entirely different from that of inorganic substances. But the line between inorganic and organic chemistry, which up to the middle of the last century appeared sharp, subsequently

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[2] G. Quincke, "Annal. d. Physik. v. Chem.," 1870 and 1888.

[3] The causation not only of movements but of various other manifestations of life by alterations in surface tension of living substance is ably dealt with by A. B. Macallum in a recent article in Asher and Spiro's "Ergebnisse der Physiologie," 1911. Macallum has described an accumulation of potassium salts at the more active surfaces of the protoplasm of many cells, and correlates this with the production of cell-activity by the effect of such accumulation upon the surface tension. The literature of the subject will be found in this article.

[4] G. F. Fitzgerald (Brit. Assoc. Reports, 1898, and Scient. Trans. Roy Dublin Society, 1898) arrived at this conclusion with regard to muscle from purely physical considerations.

[5] "Vital spontaneity, so readily accepted by persons ignorant of biology, is disproved by the whole history of science. Every vital manifestation is a response to a stimulus, a provoked phenomenon. It is unnecessary to say this is also the case with brute bodies, since that is precisely the foundation of the great principle of the inertia of matter. It is plain that it is also as applicable to living as to inanimate matter."—Dastre, *op. cit.*, p. 280.

[6] The terms "assimilation" and "disassimilation" express the physical and chemical changes which occur within protoplasm as the result of the intake of nutrient material from the circumambient medium and its ultimate transformation into waste products which are passed out again into that medium; the whole cycle of these changes being embraced under the term "metabolism."

[7] Leduc ("The Mechanism of Life," English translation by W. Deane Butcher, 1911) has given many illustrations of this statement. In the Report of the meeting of 1867 in Dundee is a paper by Dr. J. D. Heaton (On Simulations of Vegetable Growths by Mineral Substances) dealing with the same class of phenomena. The conditions of osmosis *in* cells have been especially studied by Hamburger ("Osmotischer Druck und Ionenlehre," (Wiesbaden, 1902–4).

became misty and has now disappeared. Similarly the chemistry of living organisms, which is now a recognised branch of organic chemistry, but used to be considered as so much outside the domain of the chemist that it could only be dealt with by those whose special business it was to study "vital" processes, is passing every day more out of the hands of the biologist and into those of the pure chemist.

The Colloid Constitution of Living Matter.—Identity of Physical and Chemical Processes in Living and Non-living Matter.

Somewhat more than half a century ago Thomas Graham published his epoch-making observations relating to the properties of matter in the colloidal state: observations which are proving all-important in assisting our comprehension of the properties of living substance. For it is becoming every day more apparent that the chemistry and physics of the living organism are essentially the chemistry and physics of nitrogenous colloids. Living substance or protoplasm always, in fact, takes the form of a colloidal solution. In this solution the colloids are associated with crystalloids (electrolytes), which are either free in the solution or attached to the molecules of the colloids. Surrounding and enclosing the living substance thus constituted of both colloid and crystalloid material is a film, probably also formed of colloid, but which may have a lipoid substratum associated with it (Overton). This film serves the purpose of an osmotic membrane, permitting of exchanges by diffusion between the colloidal solution constituting the protoplasm and the circumambient medium in which it lives. Other similar films or membranes occur in the interior of protoplasm. These films have in many cases specific characters, both physical and chemical, thus favouring the diffusion of special kinds of material into and out of the protoplasm and from one part of the protoplasm to another. It is the changes produced under these physical conditions, associated with those caused by active chemical agents formed within protoplasm and known as *enzymes*, that effect assimilation and disassimilation. Quite similar changes can be produced outside the body (*in vitro*) by the employment of methods of a purely physical and chemical nature. It is true that we are not yet familiar with all the intermediate stages of transformation of the materials which are taken in by a living body into the materials which are given out from it. But since the initial processes and the final results are the same as they would be on the assumption that the changes are brought about in conformity with the known laws of chemistry and physics, we may fairly conclude that all changes in living substance are brought about by ordinary chemical and physical forces.

Similarity of the Processes of Growth and Reproduction in Living and Non-living Matter.

Should it be contended that growth and reproduction are properties possessed only by living bodies and constitute a test by which we may differentiate between life and non-life, between the animate and inanimate creation, it must be replied that no contention can be more fallacious. Inorganic crystals grow and multiply and reproduce their like, given a supply of the requisite pabulum. In most cases for each kind of crystal there is, as with living organisms, a limit of growth which is not exceeded, and further increase of the crystalline matter results not in further increase in size but in multiplication of similar crystals. Leduc has shown that the growth and division of artificial colloids of an inorganic nature, when placed in an appropriate medium, present singular resemblances to the phenomena of the growth and division of living organisms. Even so complex a process as the division of a cell-nucleus by karyokinesis as a preliminary to the multiplication of the cell by division—a phenomenon which would *primâ facie* have seemed and has been commonly regarded as a distinctive manifestation of the life of the cell—can be imitated with solutions of a simple inorganic salt, such as chloride of sodium, containing a suspension of carbon particles; which arrange and rearrange themselves under the influence of the movements of the electrolytes in a manner indistinguishable from that adopted by the particles of chromatin in a dividing nucleus. And in the process of sexual reproduction, the researches of J. Loeb and others upon the ova of the sea-urchin have proved that we can no longer consider such an apparently vital phenomenon as the fertilisation of the egg as being the result of living material brought to it by the spermatozoon, since it is possible to start the process of the ovum and the resulting formation of cells, and ultimately of all the tissues and organs—in short, to bring about the development of the whole body—if a simple chemical reagent is substituted for the male element in the process of fertilisation. Indeed, even a mechanical or electrical stimulus may suffice to start development.

The Question of Vitalism and Vital Force.

Kurz und gut, as the Germans say, vitalism as a working hypothesis has not only had its foundations undermined, but most of the superstructure has toppled over, and if any difficulties of explanation still persist, we are justified in assuming that the cause is to be found in our imperfect knowledge of the constitution and working of living material. At the best vitalism explains nothing, and the term "vital force" is an expression of ignorance which can bring us no further along the path of knowledge. Nor is the problem in any way advanced by substituting for the term "vitalism" "neo-vitalism," and for "vital force" "biotic energy."[8] "New presbyter is but old priest writ large."

The Possibility of the Synthesis of Living Matter.

Further, in its chemical composition we are no longer compelled to consider living substance as possessing infinite complexity, as was thought to be the case when chemists first began to break up the proteins of the body into their simpler constituents. The researches of Miescher, which have been continued and elaborated by Kossel and his pupils, have acquainted us with the fact that a body so important for the nutritive and reproductive functions of the cell as the nucleus—which may be said indeed to represent the quintessence of cell-life—possesses a chemical constitution of no very great complexity; so that we may even hope some day to see the material which composes it prepared synthetically. And when we consider that the nucleus is not only itself formed of living substance, but is capable of causing other living substance to be built up; is, in fact, the directing agent in all the principal chemical changes which take place within the living cell, it must be admitted that we are a long step forward in our knowledge of the chemical basis of life. That it is the *form* of nuclear matter rather than its chemical and molecular structure which is the important factor in nuclear activity cannot be supposed. The form of nuclei, as every microscopist knows, varies infinitely, and there are numerous living organisms in which the nuclear matter is without form, appearing simply as granules distributed in the protoplasm. Not that the form assumed and the

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[8] B. Moore, in "Recent Advances in Physiology," 1906; Moore and Roaf, *ibid.*; and "Further Advances in Physiology," 1909 Moore lays especial stress on the transformations of energy which occur in protoplasm. See on the question of vitalism Gley (*Revue Scientifique*, 1911) and D'Arcy Thompson (Address to Section D at Portsmouth, 1911).

transformations undergone by the nucleus are without importance; but it is none the less true that even in an amorphous condition the material which in the ordinary cell takes the form of a "nucleus" may, in simpler organisms which have not in the process of evolution become complete cells, fulfil functions in many respects similar to those fulfilled by the nucleus of the more differentiated organism.

A similar anticipation regarding the probability of eventual synthetic production may be made for the proteins of the cell-substance. Considerable progress in this direction has indeed already been made by Emil Fischer, who has for many years been engaged in the task of building up the nitrogenous combinations which enter into the formation of the complex molecule of protein. It is satisfactory to know that the significance of the work both of Fischer and of Kossel in this field of biological chemistry has been recognised by the award to each of these distinguished chemists of a Nobel prize.

The Chemical Constitution of Living Substance.

The elements composing living substance are few in number. Those which are constantly present are carbon, hydrogen, oxygen, and nitrogen. With these, both in nuclear matter and also, but to a less degree, in the more diffuse living material which we know as protoplasm, phosphorus is always associated. "Ohne Phosphor kein Gedank" is an accepted aphorism; "Ohne Phosphor kein Leben" is equally true. Moreover, a large proportion, rarely less than 70 per cent., of water appears essential for any manifestation of life, although not in all cases necessary for its continuance, since organisms are known which will bear the loss of the greater part if not the whole of the water they contain without permanent impairment of their vitality. The presence of certain inorganic salts is no less essential, chief amongst them being chloride of sodium and salts of calcium, magnesium, potassium, and iron. The combination of these elements into a colloidal compound represents the chemical basis of life; and when the chemist succeeds in building up this compound it will without doubt be found to exhibit the phenomena which we are in the habit of associating with the term "life."[9]

Source of Life. The Possibility of Spontaneous Generation.

The above considerations seem to point to the conclusion that the possibility of the production of life—*i.e.*, of living material—is not so remote as has been generally assumed. Since the experiments of Pasteur, few have ventured to affirm a belief in the spontaneous generation of bacteria and monads and other micro-organisms, although before his time this was by many believed to be of universal occurrence. My esteemed friend Dr. Charlton Bastian is, so far as I am aware, the only scientific man of eminence who still adheres to the old creed, and Dr. Bastian, in spite of numerous experiments and the publication of many books and papers, has not hitherto succeeded in winning over any converts to his opinion. I am myself so entirely convinced of the accuracy of the results which Pasteur obtained—are they not within the daily and hourly experience of everyone who deals with the sterilisation of organic solutions?—that I do not hesitate to believe, if living torulæ or mycelia are exhibited to me in flasks which had been subjected to prolonged boiling after being hermetically sealed, that there has been some fallacy either in the premisses or in the carrying out of the operation. The appearance of organisms in such flasks would not furnish to my mind proof that they were the result of spontaneous generation. Assuming no fault in manipulation or fallacy in observation, I should find it simpler to believe that the germs of such organisms have resisted the effects of prolonged heat than that they became generated spontaneously. If spontaneous generation is possible, we cannot expect it to take the form of living beings which show so marked a degree of differentiation, both structural and functional, as the organisms which are described as making their appearance in these experimental flasks.[10] Nor should we expect the spontaneous generation of living substance of any kind to occur in a fluid the organic constituents of which have been so altered by heat that they can retain no sort of chemical resemblance to the organic constituents of living matter. If the formation of life—of living substance—is possible at the present day—and for my own part I see no reason to doubt it—a boiled infusion of organic matter—and still less of inorganic matter—is the last place in which to look for it. Our mistrust of such evidence as has yet been brought forward need not, however, preclude us from admitting the possibility of the formation of living from non-living substance.[11]

Life a Product of Evolution.

Setting aside, as devoid of scientific foundation, the idea of immediate supernatural intervention in the first production of life, we are not only justified in believing, but compelled to believe, that living matter must have owed its origin to causes similar in character to those which have been instrumental in producing all other forms of matter in the universe; in other words, to a process of gradual evolution.[12] But it has been customary of late amongst biologists to shelve the investigation of the mode of origin of life by evolution from non-living matter by relegating its solution to some former condition of the earth's history, when, it is assumed, opportunities were accidentally favourable for the passage of inanimate matter into animate; such opportunities, it is also assumed, having never since recurred and being never likely to recur.[13]

Various eminent scientific men have even supposed that life has not actually originated upon our globe, but has been brought to it from another planet or from another stellar system. Some of my audience may still remember the controversy that was excited when the theory of the origin of terrestial life by the intermediation of a meteorite was propounded by Sir William Thomson in his Presidential Address at the

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[9] The most recent account of the chemistry of protoplasm is that by Botazzi ("Das Cytoplasma u. die Körpersäfte") in Winterstein's "Handb. d. vergl. Physiologie," Bd. I., 1912. The literature is given in this article.

[10] It is fair to point out that Dr. Bastian suggests that the formation of ultramicroscopic living particles may precede the appearance of the microscopic organisms which he describes. "The Origin of Life," 1911, p. 65.

[11] The present position of the subject is succinctly stated by Dr. Chalmers Mitchell in his article on "Abiogenesis" in the "Encyclopædia Britannica." Dr. Mitchell adds: "It may be that in the progress of science it may yet be possible to construct living protoplasm from non-living material. The refutation of abiogenesis has no further bearing on this possibility than to make it probable that if protoplasm ultimately be formed in the laboratory, it will be by a series of steps, the earlier steps being the formation of some substance, or substances now unknown, which are not protoplasm. Such intermediate stages may have existed in the past." And Huxley in his Presidential Address at Liverpool in 1870 says: "But though I cannot express this conviction" (*i.e.*, of the impossibility of the occurrence of abiogenesis as exemplified by the appearance of organisms in hermetically sealed and sterilised flasks) "too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics and physiology yet in their infancy and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together."

[12] The arguments in favour of this proposition have been arrayed by Meldola in his Herbert Spencer Lecture, 1910, pp. 16–24. Meldola leaves the question open whether such evolution has occurred only in past years or is also taking place now. He concludes that whereas certain carbon compounds have survived by reason of possessing extreme stability, others—the precursors of living matter—survived owing to the possession of extreme lability and adaptability to variable conditions of environment. A similar suggestion was previously made by Lockyer, "Inorganic Evolution," 1900, pp. 169, 170.

[13] T. H. Huxley, Presidential Address, 1870; A. B. Macallum, "On the Origin of Life on the Globe," in Trans. Canadian Institute, VIII.

meeting of this Association in Edinburgh in 1871. To this "meteorite" theory[14] the apparently fatal objection was raised that it would take some sixty million years for a meteorite to travel from the nearest stellar system to our earth, and it is inconceivable that any kind of life could be maintained during such a period. Even from the nearest planet 150 years would be necessary, and the heating of the meteorite in passing through our atmosphere and at its impact with the earth would, in all probability, destroy any life which might have existed within it. A cognate theory, that of *cosmic panspermia*, assumes that life may exist and may have existed indefinitely in cosmic dust in the interstellar spaces (Richter, 1865; Cohn, 1872), and may with this dust fall slowly to the earth without undergoing the heating which is experienced by a meteorite. Arrhenius,[15] who adopts this theory, states that if living germs were carried through the ether by luminous and other radiations the time necessary for their transportation from our globe to the nearest stellar system would be only nine thousand years, and to Mars only twenty days!

But the acceptance of such theories of the arrival of life on the earth does not bring us any nearer to a conception of its actual mode of origin; on the contrary, it merely serves to banish the investigation of the question to some conveniently inaccessible corner of the universe and leaves us in the unsatisfactory position of affirming not only that we have no knowledge as to the mode of origin of life—which is unfortunately true—but that we never can acquire such knowledge—which it is to be hoped is not true.[16] Knowing what we know, and believing what we believe, as to the part played by evolution in the development of terestrial matter, we are, I think (without denying the possibility of the existence of life in other parts of the universe[17]), justified in regarding these cosmic theories as inherently improbable—at least in comparison with the solution of the problem which the evolutionary hypothesis offers.[18]

The Evolutionary Hypothesis as applied to the Origin of Life.

I assume that the majority of my audience have at least a general idea of the scope of this hypothesis, the general acceptance of which has within the last sixty years altered the whole aspect not only of biology, but of every other branch of natural science, including astronomy, geology, physics, and chemistry.[19] To those who have not this familiarity I would recommend the perusal of a little book by Prof. Judd entitled "The Coming of Evolution," which has recently appeared as one of the Cambridge manuals. I know of no similar book in which the subject is as clearly and succinctly treated. Although the author nowhere expresses the opinion that the actual origin of life on the earth has arisen by evolution from non-living matter, it is impossible to read either this or any similar exposition in which the essential unity of the evolutionary process is insisted upon without concluding that the origin of life must have been due to the same process, this process being, without exception, continuous, and admitting of no gap at any part of its course. Looking, therefore, at the evolution of living matter by the light which is shed upon it from the study of the evolution of matter in general, we are led to regard it as having been produced, not by a sudden alteration, whether exerted by natural or supernatural agency, but by a gradual process of change from material which was lifeless, through material on the borderland between inanimate and animate, to material which has all the characteristics to which we attach the term "life." So far from expecting a sudden leap from an inorganic, or at least an unorganised, into an organic and organised condition, from an entirely inanimate substance to a completely animate state of being, should we not rather expect a gradual procession of changes from inorganic to organic matter, through stages of gradually increasing complexity until material which can be termed living is attained? And in place of looking for the production of fully formed living organisms in hermetically sealed flasks, should we not rather search Nature herself, under natural conditions, for evidence of the existence, either in the past or in the present, of transitional forms between living and non-living matter?

The difficulty, nay, the impossibility, of obtaining evidence of such evolution from the past history of the globe is obvious. Both the hypothetical transitional material and the living material which was originally evolved from it may, as Macallum has suggested, have taken the form of diffused ultramicroscopic particles of living substance[20]; and even if they were not diffused but aggregated into masses, these masses could have been physically nothing more than colloidal watery slime which would leave no impress upon any geological formation. Myriads of years may have elapsed before some sort of skeleton in the shape of calcareous or siliceous spicules began to evolve itself, and thus enabled "life," which must already have possessed a prolonged existence, to make any sort of geological record. It follows that in attempting to pursue the evolution of living matter to its beginning in terrestrial history we can only expect to be confronted with a blank wall of nescience.

The problem would appear to be hopeless of ultimate solution if we are rigidly confined to the supposition that the evolution of life has only occurred once in the past history of the globe. But are we justified in assuming that at one period only, and as it were by a fortunate and fortuitous concomitation of substance and circumstance, living matter became evolved out of non-living matter—life became established? Is there any valid reason to conclude that at some previous period of its history our earth was more favourably circumstanced for the production of life than it is now?[21] I have vainly sought for such reason, and if none be forthcoming the conclusion forces itself upon us that the evolution of non-living into living substance has happened more than once—and we can be by no means sure that it may not be happening still.

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[14] First suggested, according to Dastre, by de Salles-Guyon (Dastre, *op. cit.*, p. 252). The theory received the support of Helmholtz.

[15] "Worlds in the Making," transl. by H. Borns, chap. viii., p. 221, 1908.

[16] "The history of science shows how dangerous it is to brush aside mysteries—*i e.*, unsolved problems—and to interpose the barrier placarded 'eternal—no thoroughfare.'"—R. Meldola, Herbert Spencer Lecture, 1910.

[17] Some authorities, such as Errera, contend, with much probability, that the conditions in interstellar space are such that life, as we understand it, could not possibly exist there.

[18] As Verworn points out, such theories would equally apply to the origin of any other chemical combination, whether inorganic or organic, which is met with on our globe, so that they lead directly to absurd conclusions.—"Allgemeine Physiologie," 1911.

[19] As Meldola insists, this general acceptance was in the first instance largely due to the writings of Herbert Spencer: "We are now prepared for evolution in every domain . . . As in the case of most great generalisations, thought had been moving in this direction for many years. . . . Lamarck and Buffon had suggested a definite mechanism of organic development, Kant and Laplace a principle of celestial evolution, while Lyell had placed geology upon an evolutionary basis. The principle of continuity was beginning to be recognised in physical science. . . . It was Spencer who brought these independent lines of thought to a focus, and who was the first to make any systematic attempt to show that the law of development expressed in its widest and most abstract form was universally followed throughout cosmical processes, inorganic, organic, and super-organic."—*Op. cit.*, p. 14.

[20] There still exist in fact forms of life which the microscope cannot show us (E. A. Minchin, Presidential Address to Quekett Club, 1911), and germs which are capable of passing through the pores of a Chamberland filter.

[21] Chalmers Mitchell (Article "Life," "Encycl. Brit.," eleventh editions writes as follows: "It has been suggested from time to time that condition) very unlike those now existing were necessary for the first appearance of life, and must be repeated if living matter is to be reconstituted artificially. No support for such a view can be derived from observations of the existing conditions of life."

It is true that up to the present there is no evidence of such happening; no process of transition has hitherto been observed. But, on the other hand, is it not equally true that the kind of evidence which would be of any real value in determining this question has not hitherto been looked for? We may be certain that if life is being produced from non-living substance, it will be life of a far simpler character than any that has yet been observed—in material which we shall be uncertain whether to call animate or inanimate, even if we are able to detect it at all, and which we may not be able to visualise physically even after we have become convinced of its existence.[22] But we can look with the mind's eye and follow in imagination the transformation which non-living matter may have undergone and may still be undergoing to produce living substance. No principle of evolution is better founded than that insisted upon by Sir Charles Lyell, justly termed by Huxley "the greatest geologist of his time," that we must interpret the past history of our globe by the present; that we must seek for an explanation of what has happened by the study of what is happening; that, given similar circumstances, what has occurred at one time will probably occur at another. The process of evolution is universal. The inorganic materials of the globe are continually undergoing transition. New chemical combinations are constantly being formed and old ones broken up; new elements are making their appearance and old elements disappearing.[23] Well may we ask ourselves why the production of living matter alone should be subject to other laws than those which have produced, and are producing, the various forms of non-living matter; why what has happened may not happen. If living matter has been evolved from lifeless in the past, we are justified in accepting the conclusion that its evolution is possible in the present and in the future. Indeed, we are not only justified in accepting this conclusion, we are forced to accept it. When or where such change from non-living to living matter may first have occurred, when or where it may have continued, when or where it may still be occurring, are problems as difficult as they are interesting, but we have no right to assume that they are insoluble.

Since living matter always contains water as its most abundant constituent, and since the first living organisms recognisable as such in the geological series were aquatic, it has generally been assumed that life must first have made its appearance in the depths of the ocean.[24] Is it, however, certain that the assumption that life originated in the sea is correct? Is not the land-surface of our globe quite as likely to have been the nidus for the evolutionary transformation of non-living into living material as the waters which surround it? Within this soil almost any chemical transformation may occur; it is subjected much more than matters dissolved in sea-water to those fluctuations of moisture, temperature, electricity, and luminosity which are potent in producing chemical changes. But whether life, in the form of a simple slimy colloid, originated in the depths of the sea or on the surface of the land, it would be equally impossible for the geologist to trace its beginnings, and were it still becoming evolved in the same situations, it would be almost as impossible for the microscopist to follow its evolution. We are therefore not likely to obtain direct evidence regarding such a transformation of non-living into living matter in nature, even if it is occurring under our eyes.

An obvious objection to the idea that the production of living matter from non-living has happened more than once is that, had this been the case, the geological record should reveal more than one palæontological series. This objection assumes that evolution would in every case take an exactly similar course and proceed to the same goal—an assumption which is, to say the least, improbable. If, as might well be the case, in any other palæontological series than the one with which we are acquainted the process of evolution of living beings did not proceed beyond Protista, there would be no obvious geological evidence regarding it; such evidence would only be discoverable by a carefully directed search made with that particular object in view.[25] I would not by any means minimise the difficulties which attend the suggestion that the evolution of life may have occurred more than once or may still be happening, but, on the other hand, it must not be ignored that those which attend the assumption that the production of life has occurred once only are equally serious. Indeed, had the idea of the possibility of a multiple evolution of living substance been first in the field, I doubt if the prevalent belief regarding a single fortuitous production of life upon the globe would have become established among biologists—so much are we liable to be influenced by the impressions we receive in scientific childhood!

Further Course of Evolution of Life.

Assuming the evolution of living matter to have occurred—whether once only or more frequently matters not for the moment—and in the form suggested, viz., as a mass of colloidal slime possessing the property of assimilation and therefore of growth, reproduction would follow as a matter of course. For all material of this physical nature—fluid or semifluid in character—has a tendency to undergo subdivision when its bulk exceeds a certain size. The subdivision may be into equal or nearly equal parts, or it may take the form of buds. In either case every separated part would resemble the parent in chemical and physical properties, and would equally possess the property of taking in and assimilating suitable material from its liquid environment, growing in bulk and reproducing its like by subdivision. *Omne vivum e vivo.* In this way from any beginning of living material a primitive form of life would spread, and would gradually people the globe. The establishment of life being once effected, all forms of organisation follow under the inevitable laws of evolution. *Ce n'est que le premier pas qui coûte.*

We can trace in imagination the segregation of a more highly phosphorised portion of the primitive living matter, which we may now consider to have become more akin to the protoplasm of organisms with which we are familiar. This more phosphorised portion might not for myriads of generations take the form of a definite nucleus, but it would be composed of material having a composition and qualities similar to those of the nucleus of a cell. Prominent among these qualities is that of catalysis—the func-

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[22] "Spontaneous generation of life could only be perceptually demonstrated by filling in the long terms of a series between the complex forms of inorganic and the simplest forms of organic substance. Were this done, it is quite possible that we should be unable to say (especially considering the vagueness of our definitions of life) where life began or ended."—K. Pearson, "Grammar of Science," second edition, 1900, p. 350.

[23] See on the production of elements, W. Crookes, Address to Section B, Brit. Assoc., 1886; T. Preston, NATURE, vol. lx., p. 180; J. J. Thomson, *Phil. Mag.*, 1897, p. 311; Norman Lockyer, *op. cit.*, 1900; G. Darwin, Pres. Addr. Brit. Association., 1905.

[24] For arguments in favour of the first appearance of life having been in the sea, see A. B. Macallum, "The Palæochemistry of the Ocean," Trans. Canad. Instit., 1903-4.

[25] Lankester (Art. "Protozoa," "Encycl. Brit.," tenth edition) conceives that the first protoplasm fed on the antecedent steps in its own evolution. F. J. Allen (Brit. Assoc. Reports, 1896) comes to the conclusion that living substance is probably constantly being produced, but that this fails to make itself evident owing to the substance being seized and assimilated by existing organisms. He believes that "in accounting for the first origin of life on this earth it is not necessary that, as Pflüger assumed, the planet should have been at a former period a glowing fire-ball." He "prefers to believe that the circumstances which support life would also favour its origin." And elsewhere: "Life is not an extraordinary phenomenon, not even an importation from some other sphere, but rather the actual outcome of circumstances on this earth."

tion of effecting profound chemical changes in other material in contact with it without itself undergoing permanent change. This catalytic function may have been exercised directly by the living substance or may have been carried on through the agency of the enzymes already mentioned, which are also of a colloid nature but of simpler constitution than itself, and which differ from the catalytic agents employed by the chemist in the fact that they produce their effects at a relatively low temperature. In the course of evolution special enzymes would become developed for adaptation to special conditions of life, and with the appearance of these and other modifications, a process of differentiation of primitive living matter into individuals with definite specific characters gradually became established. We can conceive of the production in this way from originally undifferentiated living substance of simple differentiated organisms comparable to the lowest forms of Protista. But how long it may have taken to arrive at this stage we have no means of ascertaining. To judge from the evidence afforded by the evolution of higher organisms it would seem that a vast period of time would be necessary for even this amount of organisation to establish itself.

Formation of the Nucleated Cell.

The next important phase in the process of evolution would be the segregation and moulding of the diffused or irregularly aggregated nuclear matter into a definite nucleus around which all the chemical activity of the organism will in future be centred. Whether this change were due to a slow and gradual process of segregation or of the nature of a jump, such as Nature does occasionally make, the result would be the advancement of the living organism to the condition of a complete nucleated cell: a material advance not only in organisation, but—still more important—in potentiality for future development. Life is now embodied in the cell, and every living being evolved from this will itself be either a cell or a cell-aggregate. *Omnis cellula e cellulâ.*

Establishment of Sexual Differences.

After the appearance of a nucleus—but how long after it is impossible to conjecture—another phenomenon appeared upon the scene in the occasional exchange of nuclear substance between cells. In this manner became established the process of sexual reproduction. Such exchange in the unicellular Protista might and may occur between any two cells forming the species, but in the multicellular Metazoa it became —like other functions—specialised in particular cells. The result of the exchange is rejuvenescence; associated with an increased tendency to subdivide and to produce new individuals. This is due to the introduction of a stimulating or catalytic chemical agent into the cell which is to be rejuvenated, as is proved by the experiments of Loeb already alluded to. It is true that the chemical material introduced into the germ-cell in the ordinary process of its fertilisation by the sperm-cell is usually accompanied by the introduction of definite morphological elements which blend with others already contained within the germ-cell, and it is believed that the transmission of such morphological elements of the parental nuclei is related to the transmission of parental qualities. But we must not be blind to the possibility that these transmitted qualities may be connected with specific chemical characters of the transmitted elements; in other words, that heredity also is one of the questions the eventual solution of which we must look to the chemist to provide.

Aggregate Life.

So far we have been chiefly considering life as it is found in the simplest forms of living substance, organisms for the most part entirely microscopic and neither distinctively animal nor vegetable, which were grouped together by Haeckel as a separate kingdom of animated nature—that of Protista. But persons unfamiliar with the microscope are not in the habit of associating the term "life" with microscopic organisms, whether these take the form of cells or of minute portions of living substance which have not yet attained to that dignity. We most of us speak and think of life as it occurs in ourselves and other animals with which we are familiar; and as we find it in the plants around us. We recognise it in these by the possession of certain properties—movement, nutrition, growth, and reproduction. We are not aware by intuition, nor can we ascertain without the employment of the microscope, that we and all the higher living beings, whether animal or vegetable, are entirely formed of aggregates of nucleated cells, each microscopic and each possessing its own life. Nor could we suspect by intuition that what we term our life is not a single indivisible property, capable of being blown out with a puff like the flame of a candle; but is the aggregate of the lives of many millions of living cells of which the body is composed. It is but a short while ago that this cell-constitution was discovered: it occurred within the lifetime, even within the memory, of some who are still with us. What a marvellous distance we have travelled since then in the path of knowledge of living organisms! The strides which were made in the advance of the mechanical sciences during the nineteenth century, which is generally considered to mark that century as an age of unexampled progress, are as nothing in comparison with those made in the domain of biology, and their interest is entirely dwarfed by that which is aroused by the facts relating to the phenomena of life which have accumulated within the same period. And not the least remarkable of these facts is the discovery of the cell-structure of plants and animals!

Evolution of the Cell-aggregate.

Let us consider how cell-aggregates came to be evolved from organisms consisting of single cells. Two methods are possible—viz. (1) the adhesion of a number of originally separate individuals; (2) the subdivision of a single individual without the products of its subdivision breaking loose from one another. No doubt this last is the manner whereby the cell-aggregate was originally formed, since it is that by which it is still produced, and we know that the life-history of the individual is an epitome of that of the species. Such aggregates were in the beginning solid; the cells in contact with one another and even in continuity: subsequently a space or cavity became formed in the interior of the mass, which was thus converted into a hollow sphere. All the cells of the aggregate were at first perfectly similar in structure and in function; there was no subdivision of labour. All would take part in effecting locomotion; all would receive stimuli from outside; all would take in and digest nutrient matter, which would then be passed into the cavity of the sphere to serve as a common store of nourishment. Such organisms are still found, and constitute the lowest types of Metazoa. Later one part of the hollow sphere became dimpled to form a cup; the cavity of the sphere became correspondingly altered in shape. With this change in structure, differentiation of function between the cells covering the outside and those lining the inside of the cup made its appearance. Those on the outside subserved locomotor functions and received and transmitted from cell to cell stimuli, physical or chemical, received by the organism; while those on the inside, being freed from such functions, tended to specialise in the direction of the inception and digestion of nutrient material; which, passing from them into the cavity of the invaginated sphere, served for the nourishment of all the cells composing the organism. The further course of evolution produced many changes of form and ever-increasing complexity of the cavity thus produced by simple invagination. Some of the cell-aggregates settled down to a sedentary life, becoming plant-like in appearance and to some extent in habit. Such organisms, complex in form but simple in structure, are the Sponges. Their several parts are not, as in the higher Metazoa, closely interdependent: the destruction of any one part, however extensive, does not either immediately or ultimately involve death of the rest: all parts function separately, although doubtless mutually benefiting by their conjunction, if only by slow diffusion of nutrient fluid throughout the mass. There is already some differentiation in these organisms, but the absence of a nervous system prevents any general coordination, and the individual cells are largely independent of one another.

Our own life, like that of all the higher animals, is an *aggregate life*; the life of the whole is the life of the individual cells. The life of some of these cells can be put an end to, the rest may continue to live. This is, in fact, happening every moment of our lives. The cells which cover the surface of our body, which form the scarf-skin and the hairs and nails, are constantly dying and the dead cells are rubbed off or cut away, their place being taken by others supplied from living layers beneath. But the death of these cells does not affect the vitality of the body as a whole. They serve merely as a protection, or an ornamental covering, but are otherwise not material to our existence. On the other hand, if a few cells, such as those nerve-cells under the influence of which respiration is carried on, are destroyed or injured, within a minute or two the whole living machine comes to a standstill, so that to the bystander the patient is dead; even the doctor will pronounce life to be extinct. But this pronouncement is correct only in a special sense. What has happened is that, owing to the cessation of respiration, the supply of oxygen to the tissues is cut off. And since the manifestations of life cease without this supply, the animal or patient appears to be dead. If, however, within a short period we supply the needed oxygen to the tissues requiring it, all the manifestations of life reappear.

It is only some cells which lose their vitality at the moment of so-called "general death." Many cells of the body retain their individual life in suitable circumstances long after the rest of the body is dead. Notable among these are muscle-cells. McWilliam showed that the muscle-cells of the blood-vessels give indications of life several days after an animal has been killed. The muscle-cells of the heart in mammals have been revived and caused to beat regularly and strongly many hours after apparent death. In man this result has been obtained by Kuliabko as many as eighteen hours after life had been pronounced extinct; in animals after days had elapsed. Waller has shown that indications of life can be elicited from various tissues many hours and even days after general death. Sherrington observed the white corpuscles of the blood to be active when kept in a suitable nutrient fluid weeks after removal from the blood-vessels. A French histologist, Jolly, has found that the white corpuscles of the frog, if kept in a cool place and under suitable conditions, show at the end of a year all the ordinary manifestations of life. Carrell and Burrows have observed activity and growth to continue for long periods in the isolated cells of a number of tissues and organs kept under observation in a suitable medium. Carrell has succeeded in substituting entire organs obtained after death from one animal for those of another of the same species, and has thereby opened up a field of surgical treatment the limit of which cannot yet be descried. It is a well-established fact that any part of the body can be maintained alive for hours isolated from the rest if the blood-vessels are perfused with an oxygenated solution of salts in certain proportions (Ringer). Such revival and prolongation of the life of separated organs is an ordinary procedure in laboratories of physiology. Like all the other instances enumerated, it is based on the fact that the individual cells of an organ have a life of their own which is largely independent, so that they will continue in suitable circumstances to live, although the rest of the body to which they belonged may be dead.

But some cells, and the organs which are formed of them, are more necessary to maintain the life of the aggregate than others, on account of the nature of the functions which have become specialised in them. This is the case with the nerve-cells of the respiratory centre, since they preside over the movements which are necessary to effect oxygenation of the blood. It is also true for the cells which compose the heart, since this serves to pump oxygenated blood to all other cells of the body: without such blood most cells soon cease to live. Hence we examine respiration and heart to determine if life is present: when one or both of these are at a standstill we know that life cannot be maintained. These are not the only organs necessary for the maintenance of life, but the loss of others can be borne longer, since the functions which they subserve, although useful or even essential to the organism, can be dispensed with for a time. The life of some cells is therefore more, of others less, necessary, for maintaining the life of the rest. On the other hand, the cells composing certain organs have in the course of evolution ceased to be necessary, and their continued existence may even be harmful. Wiedersheim has enumerated more than a hundred of these organs in the human body. Doubtless Nature is doing her best to get rid of them for us, and our descendants will some day have ceased to possess a vermiform appendix or a pharyngeal tonsil; until that epoch arrives we must rely for their removal on the more rapid methods of surgery!

The Maintenance of the Life of the Cell-aggregate in the Higher Animals.—Coordinating Mechanisms.

We have seen that in the simplest multicellular organisms, where one cell of the aggregate differs but little from another, the conditions for the maintenance of the life of the whole are nearly as simple as those for individual cells. But the life of a cell-aggregate such as composes the bodies of the higher animals is maintained not only by the conditions for the maintenance of the life of the individual cell being kept favourable, but also by the coordination of the varied activities of the cells which form the aggregate. Whereas in the lowest Metazoa all cells of the aggregate are alike in structure and function and perform and share everything in common, in higher animals (and for that matter in the higher plants also) the cells have become specialised, and each is only adapted for the performance of a particular function. Thus the cells of the gastric glands are only adapted for the secretion of gastric juice, the cells of the villi for the absorption of digested matters from the intestine, the cells of the kidney for the removal of waste products and superfluous water from the blood, those of the heart for pumping blood through the vessels. Each of these cells has its individual life and performs its individual functions. But unless there were some sort of cooperation and subordination to the needs of the body generally, there would be sometimes too little, sometimes too much gastric juice secreted; sometimes too tardy, sometimes too rapid an absorption from the intestine; sometimes too little, sometimes too much blood pumped into the arteries, and so on. As the result of such lack of cooperation the life of the whole would cease to be normal and would eventually cease to be maintained.

We have already seen what are the conditions which are favourable for the maintenance of life of the individual cell, no matter where situated. The principal condition is that it must be bathed by a nutrient fluid of suitable and constant composition. In higher animals this fluid is the lymph, which bathes the tissue elements and is itself constantly supplied with fresh nutriment and oxygen by the blood. Some tissue-cells are directly bathed by blood; and in invertebrates, in which there is no special system of lymph-vessels, all the tissues are thus nourished. All cells both take from and give to the blood, but not the same materials or to an equal extent. Some, such as the absorbing cells of the villi, almost exclusively give; others, such as the cells of the renal tubules, almost exclusively take. Nevertheless, the resultant of all the give and take throughout the body serves to maintain the composition of the blood constant in all circumstances. In this way the first condition of the maintenance of the life of the aggregate is fulfilled by insuring that the life of the individual cells composing it is kept normal.

The second essential condition for the maintenance of life of the cell-aggregate is the coordination of its parts and the due regulation of their activity, so that they may work together for the benefit of the whole. In the animal body this is effected in two ways: first, through the nervous system; and second, by the action of specific chemical substances which are formed in certain organs and carried by the blood to other parts of the body, the cells of which they excite to activity. These substances have received the general designation of "hormones" (ὁρμάω, to stir up), a term introduced by Prof. Starling. Their action, and indeed their very existence, has only been recognised of late years, although the part which they play in the physiology of animals appears to be only second in importance to that of the nervous system itself; indeed, maintenance of life may become impossible in the absence of certain of these hormones.

Part played by the Nervous System in the Maintenance of Aggregate Life.—Evolution of a Nervous System.

Before we consider the manner in which the nervous system serves to coordinate the life of the cell-aggregate, let us see how it has become evolved.

The first step in the process was taken when certain of the cells of the external layer became specially sensitive to stimuli from outside, whether caused by mechanical impressions (tactile and auditory stimuli) or impressions of light and darkness (visual stimuli) or chemical impressions. The effects of such impressions were probably at first simply communicated to adjacent cells and spread from cell to cell throughout the mass. An advance was made when the more impressionable cells threw out branching feelers amongst the other cells of the organism. Such feelers would convey the effects of stimuli with greater rapidity and directness to distant parts. They may at first have been retractile, in this respect resembling the long pseudopodia of certain Rhizopoda. When they became fixed they would be potential nerve-fibres and would represent the beginning of a nervous system. Even yet (as Ross Harrison has shown), in the course of development of nerve-fibres, each fibre makes its appearance as an amœboid cell-process which is at first retractile, but gradually grows into the position it is eventually to occupy and in which it will become fixed.

In the further course of evolution a certain number of these specialised cells of the external layer sank below the general surface, partly perhaps for protection, partly for better nutrition: they became nerve-cells. They remained connected with the surface by a prolongation which became an afferent or sensory nerve-fibre, and through its termination between the cells of the general surface continued to receive the effects of external impressions; on the other hand, they continued to transmit these impressions to other, more distant cells by their efferent prolongations. In the further course of evolution the nervous system thus laid down became differentiated into distinct *afferent, efferent,* and *intermediary* portions. Once established, such a nervous system, however simple, must dominate the organism, since it would furnish a mechanism whereby the individual cells would work together more effectually for the mutual benefit of the whole.

It is the development of the nervous system, although not proceeding in all classes along exactly the same lines, which is the most prominent feature of the evolution of the Metazoa. By and through it all impressions reaching the organism from the outside are translated into contraction or some other form of cell-activity. Its formation has been the means of causing the complete divergence of the world of animals from the world of plants, none of which possess any trace of a nervous system. Plants react, it is true, to external impressions, and these impressions produce profound changes and even comparatively rapid and energetic movements in parts distant from the point of application of the stimulus—as in the well-known instance of the sensitive plant. But the impressions are in all cases propagated directly from cell to cell—not through the agency of nerve-fibres; and in the absence of anything corresponding to a nervous system it is not possible to suppose that any plant can ever acquire the least glimmer of intelligence. In animals, on the other hand, from a slight original modification of certain cells has directly proceeded in the course of evolution the elaborate structure of the nervous system with all its varied and complex functions, which reach their culmination in the workings of the human intellect. "What a piece of work is a man! How noble in reason! How infinite in faculty! In form and moving how express and admirable! In action how like an angel! In apprehension how like a god!" But lest he be elated with his physical achievements, let him remember that they are but the result of the acquisition by a few cells in a remote ancestor of a slightly greater tendency to react to an external stimulus, so that these cells were brought into closer touch with the outer world; while, on the other hand, by extending beyond the circumscribed area to which their neighbours remained restricted, they gradually acquired a dominating influence over the rest. These dominating cells became nerve-cells; and now not only furnish the means for transmission of impressions from one part of the organism to another, but in the progress of time have become the seat of perception and conscious sensation, of the formation and association of ideas, of memory, volition, and all the manifestations of the mind!

Regulation of Movements by the Nervous System.—Voluntary Movements.

The most conspicuous part played by the nervous system in the phenomena of life is that which produces and regulates the general movements of the body—movements brought about by the so-called voluntary muscles. These movements are actually the result of impressions imparted to sensory or afferent nerves at the periphery, *e.g.* in the skin or in the several organs of special sense; the effect of these impressions may not be immediate, but can be stored for an indefinite time in certain cells of the nervous system. The regulation of movements—whether they occur instantly after reception of the peripheral impression or result after a certain lapse of time; whether they are accompanied by conscious sensation or are of a purely reflex and unconscious character—is an intricate process, and the conditions of their co-ordination are of a complex nature involving not merely the causation of contraction of certain muscles, but also the prevention of contraction of others. For our present knowledge of these conditions we are largely indebted to the researches of Prof. Sherrington.

Involuntary Movements.

A less conspicuous but no less important part played by the nervous system is that by which the contractions of involuntary muscles are regulated. In normal circumstances these are always independent of consciousness, but their regulation is brought about in much the same way as is that of the contractions of voluntary muscles—viz., as the result of impressions received at the periphery. These are transmitted by afferent fibres to the central nervous system, and from the latter other impulses are sent down, mostly along the nerves of the sympathetic or autonomic system of nerves, which either stimulate or prevent contraction of the involuntary muscles. Many involuntary muscles have a natural tendency to continuous or rhythmic contraction which is quite independent of the central nervous system; in this case the effect of impulses received from the latter is merely to increase or diminish the amount of such contraction. An example of this double effect is observed in connection with the heart, which—although it can contract regularly and rhythmically when cut off from the nervous system and even if removed from the body—is normally stimulated to increased activity by impulses coming from the central nervous system through the sympathetic, or to diminished activity by others coming through the vagus. It is due to the readiness by which the action of the heart is influenced in these opposite ways by the spread of impulses generated during the nerve-storms which we term "emotions" that in the language of poetry, and even of every day, the word "heart" has become synonymous with the emotions themselves.

Effects of Emotions.

The involuntary muscle of the arteries has its action similarly balanced. When its contraction is increased, the size of the vessels is lessened and they deliver less blood; the parts they supply accordingly become pale in colour. On the other hand, when the contraction is diminished the vessels enlarge and deliver more blood; the parts which they supply become correspondingly ruddy. These changes in the arteries, like the effects upon the heart, may also be produced under the influence of emotions. Thus "blushing" is a purely physiological phenomenon due to diminished action of the muscular tissue of the arteries, whilst the pallor produced by fright is caused by an increased contraction of that tissue. Apart, however, from these conspicuous effects, there is constantly proceeding a less apparent but not less important balancing action between the two sets of nerve-fibres distributed to heart and blood-vessels; which are influenced in one direction or another by every sensation which we experience and even by impressions of which we may be wholly unconscious, such as those which occur during sleep or anæsthesia, or which affect our otherwise insensitive internal organs.

Regulation of Secretion by the Nervous System.

A further instance of nerve-regulation is seen in secreting glands. Not all glands are thus regulated, at least not directly; but in those which are, the effects are striking. Their regulation is of the same general nature as that exercised upon involuntary muscle, but it influences the chemical activities of the gland-cells and the outpouring of secretion from them. By means of this regulation a secretion can be produced or arrested, increased or diminished. As with muscle, a suitable balance is in this way maintained, and the activity of the glands is adapted to the requirements of the organism. Most of the digestive glands are thus influenced, as are the skin-glands which secrete sweat.

Regulation of Body Temperature.

And by the action of the nervous system upon the skin-glands, together with its effect in increasing or diminishing the blood-supply to the cutaneous blood-vessels, the temperature of our blood is regulated and is kept at the point best suited for maintenance of the life and activity of the tissues.

Effects of Emotions on Secretion.

The action of the nervous system upon the secretion of glands is strikingly exemplified, as in the case of its action upon the heart and blood-vessels by the effects of the emotions. Thus an emotion of one kind—such as the anticipation of food—will cause saliva to flow—"the mouth to water"; whereas an emotion of another kind—such as fear or anxiety—will stop the secretion, causing the "tongue to cleave unto the roof of the mouth," and rendering speech difficult or impossible. Such arrest of the salivary secretion also makes the swallowing of dry food difficult: advantage of this fact is taken in the "ordeal by rice" which used to be employed in the East for the detection of criminals.

Regulation by Chemical Agents: Hormones.—Internal Secretions.

The activities of the cells constituting our bodies are controlled, as already mentioned, in another way than through the nervous system, viz., by chemical agents (hormones) circulating in the blood. Many of these are produced by special glandular organs, known as internally secreting glands. The ordinary secreting glands pour their secretions on the exterior of the body or on a surface communicating with the exterior; the internally secreting glands pass the materials which they produce directly into the blood. In this fluid the hormones are carried to distant organs. Their influence upon an organ may be essential to the proper performance of its functions or may be merely ancillary to it. In the former case removal of the internally secreting gland which produces the hormone, or its destruction by disease, may prove fatal to the organism.

Suprarenals.

This is the case with the suprarenal capsules: small glands which are adjacent to the kidneys, although having no physiological connection with these organs. A Guy's physician, Dr. Addison, in the middle of the last century showed that a certain affection, almost always fatal, since known by his name, is associated with disease of the suprarenal capsules. A short time after this observation a French physiologist, Brown-Séquard, found that animals from which the suprarenal capsules are removed rarely survive the operation for more than a few days. In the concluding decade of the last century interest in these bodies was revived by the discovery that they are constantly yielding to the blood a chemical agent (or hormone) which stimulates the contractions of the heart and arteries and assists in the promotion of every action which is brought about through the sympathetic nervous system (Langley). In this manner the importance of their integrity has been explained, although we have still much to learn regarding their functions.

Thyroid.

Another instance of an internally secreting gland which is essential to life, or at least to its maintenance in a normal condition, is the thyroid. The association of imperfect development or disease of the thyroid with disorders of nutrition and inactivity of the nervous system is well ascertained. The form of idiocy known as cretinism and the affection termed myxœdema are both associated with deficiency of its secretion: somewhat similar conditions to these are produced by the surgical removal of the gland. The symptoms are alleviated or cured by the administration of its juice. On the other hand, enlargement of the thyroid, accompanied by increase of its secretion, produces symptoms of nervous excitation, and similar symptoms are caused by excessive administration of glandular substance by the mouth. From these observations it is inferred that the juice contains hormones which help to regulate the nutrition of the body and serve to stimulate the nervous system, for the higher functions of which they appear to be essential. To quote M. Gley, to whose researches we owe much of our knowledge regarding the functions of this organ: "La genèse et l'exercice des plus hautes facultés de l'homme sont conditionnés par l'action purement chimique d'un produit de sécrétion. Que les psychologues méditent ces faits!"

Parathyroids.

The case of the parathyroid glandules is still more remarkable. These organs were discovered by Sandström in 1880. They are four minute bodies, each no larger than a pin's head, imbedded in the thyroid. Small as they are, their internal secretion possesses hormones which exert a powerful influence upon the nervous system. If they are completely removed, a complex of symptoms, technically known as "tetany," is liable to occur, which is always serious and may be fatal. Like the hormones of the thyroid itself, therefore, those of the parathyroids produce effects upon the nervous system, to which they are carried by the blood; although the effects are of a different kind.

Pituitary.

Another internally secreting gland which has evoked considerable interest during the last few years is the pituitary body. This is a small structure no larger than a cob-nut attached to the base of the brain. It is mainly composed of glandular cells. Its removal has been found (by most observers) to be fatal—often within two or three days. Its hypertrophy, when occurring during the general growth of the body, is attended by an undue development of the skeleton, so that the stature tends to assume gigantic proportions. When the hypertrophy occurs after growth is completed, the extremities—viz., the hands and feet, and the bones of the face—are mainly affected; hence the condition has been termed "acromegaly" (enlargement of extremities). The association of this condition with affections of the pituitary was pointed out in 1885 by a distinguished French physician, Dr. Pierre Marie. Both "giants" and "acromegalists" are almost invariably found to have an enlarged pituitary. The enlargement is generally confined to one part—the anterior lobe—and we conclude that this produces hormones which stimulate the growth of the body generally and of the skeleton in particular. The remainder of the pituitary is different in structure from the anterior lobe and has a different function. From it hormones can be extracted which, like those of the suprarenal capsule, although not exactly in the same manner, influence the contraction of the heart and arteries. Its extracts are also instrumental in promoting the secretion of certain glands. When injected into the blood they cause a free secretion of water from the kidneys and of milk from the mammary glands, neither of which organs are directly influenced (as most other glands are) through the nervous system. Doubtless under natural conditions these organs are stimulated to activity by hormones which are produced in the pituitary and which pass from this into the blood.

The internally secreting glands which have been mentioned (thyroid, parathyroid, suprarenal, pituitary) have, so far as is known, no other function than that of producing chemical substances of this character for the influencing of other organs, to which they are conveyed by the blood. It is interesting to observe that these glands are all of very small size, none being larger than a walnut, and some—the parathyroids—almost microscopic. In spite of this, they are essential to the proper maintenance of the life of the body, and the total removal of any of them by disease or operation is in most cases speedily fatal.

Pancreas.

There are, however, organs in the body yielding internal secretions to the blood in the shape of hormones, but exercising at the same time other functions. A striking instance is furnished by the pancreas, the secretion of which is the most important of the digestive juices. This—the pancreatic juice—forms the external secretion of the gland, and is poured into the intestine, where its action upon the food as it passes out from the stomach has long been recognised. It was, however, discovered in 1889 by von Mering and Minkowski that the pancreas also furnishes an internal secretion, containing a hormone which is passed from the pancreas into the blood, by which it is carried first to the liver and afterwards to the body generally. This hormone is essential to the proper utilisation of carbohydrates in the organism. It is well known that the carbohydrates of the food are converted into grape-sugar and circulate in this form in the blood, which always contains a certain amount; the blood conveys it to all the cells of the body, and they utilise it as fuel. If, owing to disease of the pancreas or as the result of its removal by surgical procedure, its internal secretion is not available, sugar is no longer properly utilised by the cells of the body and tends to accumulate in the blood; from the blood the excess passes off by the kidneys, producing diabetes.

Duodenum.

Another instance of an internal secretion furnished by an organ which is devoted largely to other functions is the "pro-secretin" found in the cells lining the duodenum. When the acid gastric juice comes into contact with these cells it converts their prosecretin into "secretin." This is a hormone which is passed into the blood and circulates with that fluid. It has a specific effect on the externally secreting cells of the pancreas, and causes the rapid outpouring of pancreatic juice into the intestine. This effect is similar to that of the hormones of the pituitary body upon the cells of the kidney and mammary gland. It was discovered by Bayliss and Starling.

Internal Secretions of the Reproductive Organs.

The reproductive glands furnish in many respects the most interesting example of organs which—besides their ordinary products, the germ- and sperm-cells (ova and spermatozoa)—form hormones which circulate in the blood and effect changes in cells of distant parts of the body. It is through these hormones that the secondary sexual characters, such as the comb and tail of the cock, the mane of the lion, the horns of the stag, the beard and enlarged larynx of a man, are produced, as well as the many differences in form and structure of the body which are characteristic of the sexes. The dependence of these so-called secondary sexual characters upon the state of development of the reproductive organs has been recognised from time immemorial, but has usually been ascribed to influences produced through the nervous system, and it is only in recent years that the changes have been shown to be brought about by the agency of internal secretions and hormones, passed from the reproductive glands into the circulating blood.[26]

Chemical Nature of Hormones.

It has been possible in only one or two instances to prepare and isolate the hormones of the internal secretions in a sufficient condition of purity to subject them to analysis, but enough is known about them to indicate that they are organic bodies of a not very complex nature, far simpler than proteins and even than enzymes. Those which have been studied are all dialysable, are readily soluble in water but insoluble in alcohol, and are not destroyed by boiling. One at least—that of the medulla of the suprarenal capsule—has been prepared synthetically, and when their exact chemical nature has been somewhat better elucidated it will probably not be difficult to obtain others in the same way.

From the above it is clear that not only is a coordination through the nervous system necessary in order that life shall be maintained in a normal condition, but a chemical coordination is no less essential. These may be independent of one another; but, on the other hand, they may react upon one another. For it can be shown that the production of some at least of the hormones is under the influence of the nervous system (Biedl, Asher, Elliott); whilst, as we have seen, some of the functions of the nervous system are dependent upon hormones.

Protective Chemical Mechanisms.—Toxins and Antitoxins.

Time will not permit me to refer in any but the briefest manner to the protective mechanisms which the cell-aggregate has evolved for its defence against disease, especially disease produced by parasitic micro-organisms. These, which belong with few exceptions to the Protista, are without doubt the most formidable enemies which the multicellular Metazoa, to which all the higher animal organisms belong, have to contend against. To such micro-organisms are due, *inter alia*, all diseases which are liable to become epidemic, such as anthrax and rinderpest in cattle, distemper in dogs and cats, smallpox, scarlet fever, measles, and sleeping sickness in man. The advances of modern medicine have shown that the symptoms of these diseases—the disturbances of nutrition, the temperature, the lassitude or excitement, and other nervous disturbances—are the effects of chemical poisons (*toxins*) produced by the micro-organisms and acting deleteriously upon the tissues of the body. The tissues, on the other hand, endeavour to counteract these effects by producing other chemical substances destructive to the micro-organisms or antagonistic to their action: these are known as *anti-bodies*. Sometimes the protection takes the form of a subtle alteration in the living substance of the cells which renders them for a long time, or even permanently, insusceptible (immune) to the action of the poison. Sometimes certain cells of the body, such as the white corpuscles of the blood, eat the invading micro-organisms and destroy them bodily by the action of chemical agents within their protoplasm. The result of an illness thus depends upon the result of the struggle between these opposing forces—the micro-organisms on the one hand and the cells of the body on the other—both of which fight with chemical weapons. If the cells of the body do not succeed in destroying the invading organisms, it is certain that the invaders will in the long run destroy them, for in this combat no quarter is given. Fortunately we have been able, by the aid of animal experimentation, to acquire some knowledge of the manner in which we are attacked by micro-organisms and of the methods which the cells of our body adopt to repel the attack, and the knowledge is now extensively utilised to assist our defence.

Parasitic Nature of Diseases.

For this purpose protective serums or antitoxins, which have been formed in the blood of other animals, are employed to supplement the action of those which our own cells produce. It is not too much to assert that the knowledge of the parasitic origin of so many diseases and of the chemical agents which on the one hand cause, and on the other combat, their symptoms, has transformed medicine from a mere art practised empirically into a real science based upon experiment. The transformation has opened out an illimitable vista of possibilities in the direction not only of cure, but, more important still, of prevention. It has taken place within the memory of most of us who are here present. And only last February the world was mourning the death of one of the greatest of its benefactors—a former President of this Association[27]—who, by applying this knowledge to the practice of surgery, was instrumental, even in his own lifetime, in saving more lives than were destroyed in all the bloody wars of the nineteenth century!

Senescence and Death.

The question has been debated whether, if all accidental modes of destruction of the life of the cell could be eliminated, there would remain a possibility of individual cell life, and even of aggregate cell life, continuing indefinitely; in other words, Are the phenomena of senescence and death a natural and necessary sequence to the existence of life? To most of my audience it will appear that the subject is not open to debate. But some physiologists (*e.g.* Metchnikoff) hold that the condition of senescence is itself abnormal; that old age is a form of disease or is due to disease, and, theoretically at least, is capable of being eliminated. We have already seen that individual cell life, such as that of the white blood-corpuscles and of the cells of many tissues, can under suitable conditions be prolonged for days or weeks or months after general death. Unicellular organisms kept under suitable conditions of nutrition have been observed to carry on their functions normally for prolonged periods and to show no degeneration such as would accompany senescence. They give rise by division to others of the same kind, which also, under favourable conditions, continue to live, to all appearance indefinitely. But these instances, although they indicate that in the simplest forms of organisation existence may be greatly extended without signs of decay, do not furnish conclusive evidence of indefinite

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[26] The evidence is to be found in F. H. A. Marshall, "The Physiology of Reproduction," 1911.

[27] Lord Lister was President at Liverpool in 1896.

prolongation of life. Most of the cells which constitute the body, after a period of growth and activity, sometimes more, sometimes less prolonged, eventually undergo atrophy and cease to perform satisfactorily the functions which are allotted to them. And when we consider the body as a whole, we find that in every case the life of the aggregate consists of a definite cycle of changes which, after passing through the stages of growth and maturity, always leads to senescence, and finally terminates in death. The only exception is in the reproductive cells, in which the processes of maturation and fertilisation result in rejuvenescence, so that instead of the usual downward change towards senescence, the fertilised ovum obtains a new lease of life, which is carried on into the new-formed organism. The latter again itself ultimately forms reproductive cells, and thus the life of the species is continued. It is only in the sense of its propagation in this way from one generation to another that we can speak of the indefinite continuance of life: we can only be immortal through our descendants!

Average Duration of Life and Possibility of its Prolongation.

The individuals of every species of animal appear to have an average duration of existence.[28] Some species are known the individuals of which live only for a few hours, whilst others survive for a hundred years.[29] In man himself the average length of life would probably be greater than the three-score and ten years allotted to him by the Psalmist if we could eliminate the results of disease and accident; when these results are included it falls far short of that period. If the terms of life given in the purely mythological part of the Old Testament were credible, man would in the early stages of his history have possessed a remarkable power of resisting age and disease. But, although many here present were brought up to believe in their literal veracity, such records are no longer accepted even by the most orthodox of theologians, and the nine hundred odd years with which Adam and his immediate descendants are credited, culminating in the nine hundred and sixty-nine of Methuselah, have been relegated, with the accounted of Creation and the Deluge, to their proper position in literature. When we come to the Hebrew patriarchs, we notice a considerable diminution to have taken place in what the insurance offices term the "expectation of life." Abraham is described as having lived only to 175 years, Joseph and Joshua to 110, Moses to 120; even at that age "his eye was not dim nor his natural force abated." We cannot say that under ideal conditions all these terms are impossible; indeed, Metchnikoff is disposed to regard them as probable; for great ages are still occasionally recorded, although it is doubtful if any as considerable as these are ever substantiated. That the expectation of life was better then than now would be inferred from the apologetic tone adopted by Jacob when questioned by Pharaoh as to his age: "The days of the years of my pilgrimage are a hundred and thirty years; few and evil have the days of the years of my life been, and have not attained unto the days of the years of the life of my fathers in the days of their pilgrimage." David, to whom, before the advent of the modern statistician, we owe the idea that seventy years is to be regarded as the normal period of life,[30] is himself merely stated to have "died in a good old age." The periods recorded for the Kings show a considerable falling-off as compared with the Patriarchs; but not a few were cut off by violent deaths, and many lived lives which were not ideal. Amongst eminent Greeks and Romans few very long lives are recorded, and the same is true of historical persons in mediæval and modern history. It is a long life that lasts much beyond eighty; three such linked together carry us far back into history. Mankind is in this respect more favoured than most mammals, although a few of these surpass the period of man's existence.[31] Strange that the brevity of human life should be a favourite theme of preacher and poet when the actual term of his "erring pilgrimage" is greater than that of most of his fellow creatures!

The End of Life.

The modern applications of the principles of preventive medicine and hygiene are no doubt operating to lengthen the average life. But even if the ravages of disease could be altogether eliminated, it is certain that at any rate the fixed cells of our body must eventually grow old and ultimately cease to function; when this happens to cells which are essential to the life of the organism, general death must result. This will always remain the universal law, from which there is no escape. "All that lives must die, passing through nature to eternity."

Such natural death unaccelerated by disease—is not death by disease as unnatural as death by accident?—should be a quiet, painless phenomenon, unattended by violent change. As Dastre expresses it, "The need of death should appear at the end of life, just as the need of sleep appears at the end of the day." The change has been led gradually up to by an orderly succession of phases, and is itself the last manifestation of life. Were we all certain of a quiet passing—were we sure that there would be "no moaning of the bar when we go out to sea"—we could anticipate the coming of death after a ripe old age without apprehension. And if ever the time shall arrive when man will have learned to regard this change as a simple physiological process, as natural as the oncoming of sleep, the approach of the fatal shears will be as generally welcomed as it is now abhorred. Such a day is still distant; we can scarcely say that its dawning is visible. Let us at least hope that, in the manner depicted by Dürer in his well-known etching, the sunshine which science irradiates may eventually put to flight the melancholy which hovers, bat-like, over the termination of our lives, and which even the anticipation of a future happier existence has not hitherto succeeded in dispersing.

SECTION A.

MATHEMATICS AND PHYSICS.

Opening Address by Prof. H. L. Callendar, LL.D., F.R.S., President of the Section.

My first duty on taking the chair is to say a few words in commemoration of the distinguished members whom we have lost since the last meeting.

George Chrystal, Professor of Mathematics in the University of Edinburgh for more than thirty years, officiated as President of this section in the year 1885, and took a prominent part in the advancement of science as secretary of the Royal Society of Edinburgh since 1901. Of his brilliant mathematical work and his ability in developing the school at Edinburgh, I am not competent to speak, but I well remember as a student his admirable article on "Electricity and

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[28] This was regarded by Buffon as related to the period of growth, but the ratio is certainly not constant. The subject is discussed by Ray Lankester in an early work: "On Comparative Longevity in Man and Animals," 1870.

[29] The approximate regular periods of longevity of different species of animals furnishes a strong argument against the theory that the decay of old age is an accidental phenomenon, comparable with disease.

[30] The expectation of life of a healthy man of fifty is still reckoned at about twenty years.

[31] "Hominis ævum cæterorum animalium omnium superat præter admodum paucorum."—Francis Bacon, "Historia vitæ et mortis," 1637.

Magnetism" contributed to the "Encyclopædia Britannica," which formed at that time the groundwork of our studies at Cambridge under Sir J. J. Thomson. It would be difficult to find a more complete and concise statement of the mathematical theory at the time when that article was written. One can well understand the value of such a teacher, and sympathise with his university in the loss they have sustained.

John Brown, F.R.S., who acted as local secretary for the Association at Belfast in 1902, will be remembered for his work on the Volta contact effect between metals, which he showed to be in the main dependent on chemical action, and to be profoundly affected by the nature of the gas or other medium in which the plates were immersed. Although the theory of this difficult subject may not yet be completely elucidated, there can be little doubt that his work takes the first rank on the experimental side.

William Sutherland, D.Sc., who at one time acted as Professor of Physics at Melbourne, is best known for his familiar papers on the subject of molecular physics in *The Philosophical Magazine*. His work was always remarkable for its wide range and boldness of imagination. Many of his hypotheses cannot yet be weighed in the balance of experiment, but some have already been substantiated. For instance, his theory of the variation of viscosity of gases with temperature has been generally accepted, and results are now commonly expressed in terms of Sutherland's constant.

Osborne Reynolds, the first Professor of Engineering at Owens College, was President of Section G in 1887, but belongs almost as much to mathematics and physics, in which his achievements are equally memorable. It would be scarcely possible for me to enumerate his important contributions to the science of engineering, which will be more fittingly commemorated elsewhere. His mastery of mathematical and physical methods, while contributing greatly to his success as a pioneer in the engineering laboratory, enabled him to attack the most difficult problems in physics, such as the theory of the radiometer and the thermal transpiration of gases. His determination of the mechanical equivalent of heat is a most striking example of accurate physical measurement carried out on an engineering scale. His last great work, on the "Submechanics of the Universe," is so original in its ideas and methods that its value cannot yet be fully appreciated. While it differs so radically from our preconceived ideas that it fails to carry immediate conviction, it undoubtedly represents possibilities of truth which subsequent workers in the same field cannot afford to ignore.

The present year has been one of remarkable activity in the world of mathematical and physical science if we may measure activity by the number and importance of scientific gatherings like the present for the interchange of ideas and the general advancement of science. The celebration of the 250th anniversary of the foundation of the Royal Society brought to our shores a number of distinguished delegates from all parts of the world, to promote the ever-growing fellowship among men of science which is one of the surest guarantees of international progress. The Congress of Universities of the Empire brought other guests from distant British dominions, and considered, as one of the principal points in its programme, the provision of facilities for the interchange of students between different universities, which will doubtless prove particularly advantageous to the scientific student in the higher branches of research. In the special branches of knowledge more particularly associated with this section, the International Congress of Mathematics at Cambridge, while it affords to Cambridge men like myself a most gratifying recognition of our *alma mater* as one of the leading schools of mathematics in the world, has given us the opportunity of meeting here a number of distinguished foreign mathematicians whose presence and personality cannot be otherwise than inspiring to our proceedings, and will compensate for any deficiency in our own mathematical programme. The Optical Convention held this year in London, by the importance of the papers contributed for discussion, and by its admirable exhibition of British instruments, has revealed the extent of our optical industry and talent, and has done much to dispel the impression, fostered by an unfortunate trade regulation, that the majority of optical instruments were "made elsewhere." The Radio-Telegraphic Conference, held under the auspices of the British Government, has formulated recommendations for regulating and extending the application of the discoveries of modern physics for saving life and property at sea. The work of this Conference will be fittingly supplemented on the scientific side by the discussion on wireless telegraphy which has been arranged to take place in this section in conjunction with Section G.

It would be impossible, even if it were not out of place, for me to attempt to review in detail the important work of these congresses, a full account of which will shortly be available in their several reports of proceedings now in course of publication. In the present age of specialisation and rapid publication it would be equally impossible to give any connected account in the time at my disposal of recent developments in those branches of science which come within the range of our section. The appropriate alternative, adopted by the majority of my predecessors in this chair, is to select some theory or idea, sufficiently fundamental to be of general interest, and to discuss it in the light of recent experimental evidence. It may sometimes be advantageous to take stock of our fundamental notions in this way, and to endeavour to determine how far they rest on direct experiment, and how far they are merely developments of some dynamical analogy, which may represent the results of experiment up to a certain point, but may lead to erroneous conclusions if pushed too far. With this object I propose to consider on the present occasion some of our fundamental ideas with regard to the nature of heat, and in particular to suggest that we might with advantage import into our modern theory some of the ideas of the old caloric or material theory which has for so long a time been forgotten and discredited. In so doing I may appear to many of you to be taking a retrograde step, because the caloric theory is generally represented as being fundamentally opposed to the kinetic theory and to the law of the conservation of energy. I would, therefore, remark at the outset that this is not necessarily the case, provided that the theory is rightly interpreted and applied in accordance with experiment. Mistakes have been made on both theories, but the method commonly adopted of selecting all the mistakes made in the application of the caloric theory and contrasting them with the correct deductions from the kinetic theory has created an erroneous impression that there is something fundamentally wrong about the caloric theory, and that it is in the nature of things incapable of correctly representing the facts. I shall endeavour to show that this fictitious antagonism between the two theories is without real foundation. They should rather be regarded as different ways of describing the same phenomena. Neither is complete without the other. The kinetic theory is generally preferable for elementary exposition, and has come to be almost exclusively adopted for this purpose; but in many cases the caloric theory would have the advantage of emphasising at the outset the importance of fundamental facts which are too often obscured in the prevailing method of treatment.

The explanation of the development of heat by friction was one of the earliest difficulties encountered by the caloric theory. One explanation, maintained by Cavendish and others, was simply that caloric was generated *de novo* by friction in much the same way as electricity. Another explanation, more commonly adopted, was that the fragments of solid, abraded in such operations as boring cannon, had a smaller capacity for heat than the original material. Caloric already existing in the substance was regarded as being squeezed or ground out of it without any fresh caloric being actually generated. The probability of the second explanation was negatived by the celebrated experiments of Rumford and Davy, who concluded that friction did not diminish the capacities of bodies for heat, and that it could not be a material substance because the supply obtainable by friction appeared to be inexhaustible. Rumford also showed that no increase of weight in a body when heated could be detected by the most delicate apparatus available in his time. Caloric evidently did not possess to any marked extent the properties of an ordinary ponderable fluid; but, if it had any real existence and was not merely a convenient mathematical fiction, it must be something of the same nature as the electric fluids, which had already played so useful a part in the description of phenomena, although their actual existence as physical entities had not then been demonstrated. Heat, as Rumford and Davy maintained, might be merely a mode of motion or a vibration of the ultimate particles of matter, but the idea in this form was too vague to serve as a basis of measurement or calculation. The simple conception of caloric, as a measurable quantity of something, sufficed for many purposes, and led in the hands of Laplace and others to correct results for the ratio of the specific heats, the adiabatic equation of gases, and other fundamental points of theory, though many problems in the relations of heat and work remained obscure.

The greatest contribution of the caloric theory to thermodynamics was the production of Carnot's immortal "Reflections on the Motive Power of Heat." It is one of the most remarkable illustrations of the undeserved discredit into which the caloric theory has fallen, that this work, the very foundation of modern thermodynamics, should still be misrepresented, and its logic assailed, on the ground that much of the reasoning is expressed in the language of the caloric theory. In justice to Carnot, even at the risk of wearying you with an oft-told tale, I cannot refrain from taking this opportunity of reviewing the essential points of his reasoning, because it affords incidentally the best introduction to the conception of caloric, and explains how a quantity of caloric is to be measured.

At the time when Carnot wrote, the industrial importance of the steam-engine was already established, and the economy gained by expansive working was generally appreciated. The air-engine, and a primitive form of the internal-combustion engine, had recently been invented. On account of the high value of the latent heat of steam, it was confidently expected that more work might be obtained from a given quantity of heat or fuel by employing some other working substance, such as alcohol or ether, in place of steam. Carnot set himself to investigate the conditions under which motive-power was obtainable from heat, how the efficiency was limited, and whether other agents were preferable to steam. These were questions of immediate practical importance to the engineer, but the answer which Carnot found embraces the whole range of science in its ever-widening scope.

In discussing the production of work from heat it is necessary, as Carnot points out, to consider a complete series or cycle of operations in which the working substance, and all parts of the engine, are restored on completion of the cycle to their initial state. Nothing but heat, or its equivalent fuel, may be supplied to the engine. Otherwise part of the motive power obtained might be due, not to heat alone, but to some change in the working substance, or in the disposition of the mechanism. Carnot here assumes the fundamental axiom of the cycle, which he states as follows:—"*When a body has undergone any changes, and, after a certain number of transformations, it is brought back identically to its original state, considered relatively to density, temperature, and mode of aggregation, it must contain the same quantity of heat as it contained originally.*" This does not limit the practical application of the theory, because all machines repeat a regular series of operations, which may be reduced in theory to an equivalent cycle in which everything is restored to its initial state.

The most essential feature of the working of all heat-engines, considered apart from details of mechanism, is the production of motive power by alternate expansion or contraction, or heating and cooling of the working substance. This necessitates the existence of a difference of temperature, produced by combustion or otherwise, between two bodies, such as the boiler and condenser of a steam-engine, which may be regarded as the source and sink of heat respectively. Wherever a difference of temperature exists, it may be made a source of motive-power, and conversely, without difference of temperature, no motive-power can be obtained from heat by a cyclical or continuous process. From this consideration Carnot deduces the simple and sufficient rule for obtaining the maximum effect:—"*In order to realise the maximum effect, it is necessary that, in the process employed, there should not be any direct interchange of heat between bodies at sensibly different temperatures.*" Direct transference of heat between bodies at sensibly different temperatures would be equivalent to wasting a difference of temperature which might have been utilised for the production of motive-power. Equality of temperature is here assumed as the limiting condition of thermal equilibrium, such that an infinitesimal difference of temperature will suffice to determine the flow of heat in either direction. An engine satisfying Carnot's rule will be reversible so far as the thermal operations are concerned. Carnot makes use of this property of reversibility in deducing his formal proof that an engine of this type possesses the maximum efficiency. If in the usual or direct method of working such an engine takes a quantity of heat Q from the source, rejects heat to the condenser, and gives a balance of useful work W per cycle, when the engine is reversed and supplied with motive-power W per cycle it will in the limit take the same quantity of heat from the condenser as it previously rejected, and return to the source the same quantity of heat Q as it took from it when working direct. All such engines must have the same efficiency (measured by the ratio W/Q of the work done to the heat taken from the source) whatever the working substance, provided that they work between the same temperature limits. For, if this were not the case, it would be theoretically possible, by employing the most efficient to drive the least efficient reversible engine backwards, to restore to the source all the heat taken from it, and to obtain a balance of useful work without the consumption of fuel; a result sufficiently improbable to serve as the basis of a formal proof. Carnot thus deduces his famous principle, which he states as follows:—"*The motive power obtainable from heat is independent of the agents set at work to realise it. Its quantity is fixed* *solely by the temperatures between which in the limit the transfer of heat takes place.*"

Objection is commonly taken to Carnot's proof, on the ground that the combination which he imagines might produce a balance of useful work without infringing the principle of conservation of energy, or constituting what we now understand as perpetual motion of the ordinary kind in mechanics. It has become the fashion to introduce the conservation of energy in the course of the proof, and to make a final appeal to some additional axiom. Any proof of this kind must always be to some extent a matter of taste; but since Carnot's principle cannot be deduced from the conservation of energy alone, it seems a pity to complicate the proof by appealing to it. For the particular object in view, the absurdity of a heat-engine working without fuel appears to afford the most appropriate improbability which could be invoked. The final appeal must be to experiment in any case. At the present time the experimental verification of Carnot's principle in its widest application so far outweighs the validity of any deductive proof, that we might well rest content with the logic that satisfied Carnot instead of confusing the issue by disputing his reasoning.

Carnot himself proceeded to test his principle in every possible way by comparison with experiment so far as the scanty data available in his time would permit. He also made several important deductions from it, which were contrary to received opinion at the time, but have since been accurately verified. He appears to have worked out these results analytically in the first instance, as indicated by his footnotes, and to have translated his equations into words in the text for the benefit of his non-mathematical readers. In consequence of this, some of his most important conclusions appear to have been overlooked or attributed to others. Owing to want of exact knowledge of the properties of substances over extended ranges of temperature, he was unable to apply his principle directly in the general form for any temperature limits. We still labour to a less extent under the same disability at the present day. He showed, however, that a great simplification was effected in its application by considering a cycle of infinitesimal range at any temperature t. In this simple case the principle is equivalent to the assertion that the work obtainable from a unit of heat per degree fall (or per degree range of the cycle) at a temperature t, is some function $F't$ of the temperature (generally known as Carnot's function), which must be the same for all substances at the same temperature. From the rough data then available for the properties of steam, alcohol, and air, he was able to calculate the numerical values of this function in kilogrammetres of work per kilocalorie of heat at various temperatures between 0° and 100° C., and to show that it was probably the same for different substances at the same temperature within the limits of experimental error. For the vapour of alcohol at its boiling-point, 78·7° C., he found the value $F't = 1{\cdot}230$ kilogrammetres per kilocalorie per degree fall. For steam at the same temperature he found nearly the same value, namely, $F't = 1{\cdot}212$. Thus no advantage in point of efficiency could be gained by employing the vapour of alcohol in place of steam. He was also able to show that the work obtainable from a kilocalorie per degree fall probably diminished with rise of temperature, but his data were not sufficiently exact to indicate the law of the variation.

The equation which Carnot employed in deducing the numerical values of his function from the experimental data for steam and alcohol is simply the direct expression of his principle as applied to a saturated vapour. It is now generally known as Clapeyron's equation, because Carnot did not happen to give the equation itself in algebraic form, although the principle and details of the calculation were most minutely and accurately described. In calculating the value of his function for air, Carnot made use of the known value of the difference of the specific heats at constant pressure and volume. He showed that this difference must be the same for equal volumes of all gases measured under the same temperature and pressure, whereas it had always previously been assumed that the ratio (not the difference) of the specific heats was the same for different gases. He also gave a general expression for the heat absorbed by a gas in expanding at constant temperature, and showed that it must bear a constant ratio to the work of expansion. These results were verified experimentally some years later, in part by Dulong, and more completely by Joule, but Carnot's theoretical prediction has generally been overlooked, although it was of the greatest interest and importance. The reason of this neglect is probably to be found in the fact that Carnot's expressions contained the unknown function $F't$ of the temperature, the form of which could not be deduced without making some assumptions with regard to the nature of heat and the scale on which temperature should be measured.

It was my privilege to discover a few years ago that Carnot himself had actually given the correct solution of this fundamental problem in one of his most important footnotes, where it had lain buried and unnoticed for more than eighty years. He showed by a most direct application of the caloric theory that if temperature was measured on the scale of a perfect gas (which is now universally adopted) the value of his function $F't$ on the caloric theory would be the same at all temperatures, and might be represented simply by a numerical constant A (our "mechanical equivalent") depending on the units adopted for work and heat. In other words, the work W done by a quantity of caloric Q in a Carnot cycle of range T to T_0 on the gas scale would be represented by the simple equation:

$$W = AQ(T - T_0).$$

It is at once obvious that this solution, obtained by Carnot from the caloric theory, so far from being inconsistent with the mechanical theory of heat, is a direct statement of the law of conservation of energy as applied to the Carnot cycle. If the lower limit T_0 of the cycle is taken at the absolute zero of the gas-thermometer, we observe that the maximum quantity of work obtainable from a quantity of caloric Q at a temperature T is simply AQT, which represents the absolute value of the energy carried by the caloric taken from the source at the temperature T. The energy of the caloric rejected at the temperature T_0 is AQT_0. The external work done is equal to the difference between the quantities of heat energy supplied and rejected in the cycle.

The analogy which Carnot himself employed in the interpretation of this equation was the oft-quoted analogy of the waterfall. Caloric might be regarded as possessing motive-power or energy in virtue of elevation of temperature just as water may be said to possess motive-power in virtue of its head or pressure. The limit of motive-power obtainable by a reversible motor in either case would be directly proportional to the head or fall measured on a suitable scale. Caloric itself was not motive-power, but must be regarded simply as the vehicle or carrier of energy, the production of motive-power from caloric depending essentially (as Carnot puts it) not on the actual consumption of caloric, but on the fall of temperature available. The measure of a quantity of caloric is the work done per degree fall, which corresponds with the measure of a quantity of water by weight, *i.e.* in kilogrammetres per metre fall.

That Carnot did not pursue the analogy further, and deduce the whole mechanical theory of heat from the caloric theory, is scarcely to be wondered at if we remember that no applications of the energy principle had then been made in any department of physics. He appears, indeed, at a later date to have caught a glimpse of the general principle when he states that "motive-power [his equivalent for work or energy] changes its form but is never annihilated." It is clear from the posthumous notes of his projected experimental work that he realised how much remained to be done on the experimental side, especially in relation to the generation of caloric by friction, and the waste of motive-power by conduction of heat, which appeared to him (in 1824) "almost inexplicable in the present state of the theory of heat."

One of the points which troubled him most in the application of the theoretical result that the work obtainable from a quantity of caloric was simply proportional to the fall of temperature available, was that it required that the specific heat of a perfect gas should be independent of the pressure. This was inconsistent with the general opinion prevalent at the time, and with one solitary experiment by Delaroche and Bérard, which appeared to show that the specific heat of a gas diminished with increase of pressure, and which had been explained by Laplace as a natural consequence of the caloric theory. Carnot showed that this result did not necessarily follow from the caloric theory, but the point was not finally decided in his favour until the experiments of Regnault, first published in 1852, established the correct values of the specific heat of gases, and proved that they were practically independent of the pressure.

Another point which troubled Carnot was that, according to his calculations, the motive-power obtainable from a kilocalorie of heat per degree fall appeared to diminish with rise of temperature, instead of remaining constant. This might have been due to experimental errors, since the data were most uncertain. But, if he had lived to carry out his projected experiments on the quantity of motive-power required to produce one unit of heat, and had obtained the result, 424 kilogrammetres per kilocalorie, subsequently found by Joule, he could scarcely have failed to notice that this was the same (within the limits of experimental error) as the maximum work AQT obtainable from the kilocalorie according to his equation. (This is seen to be the case when the values calculated by Carnot per degree fall at different temperatures were multiplied by the absolute temperature in each case. *E.g.* 1·212 kilogrammetres per degree fall with steam at 79° C. or 352° Abs. $1·212 \times 352 = 426$ kilogrammetres.) The origin of the apparent discrepancy between theory and experiment lay in the tacit assumption that the quantity of caloric in a kilocalorie was the same at different temperatures. There were no experiments at that time available to demonstrate that the caloric measure of heat as work per degree fall, implied in Carnot's principle, or more explicitly stated in his equation, was not the same as the calorimetric measure obtained by mixing substances at different temperatures. Even when the energy principle was established its exponents failed to perceive exactly where the discrepancy between the two theories lay. In reality both were correct, if fairly interpreted in accordance with experiment, but they depended on different methods of measuring a quantity of heat, which, so far from being inconsistent, were mutually complementary.

The same misconception, in a more subtle and insidious form, is still prevalent in such common phrases as the following: "We now know that heat is a form of energy and not a material fluid." The experimental fact underlying this statement is that our ordinary methods of measuring quantities of heat in reality measure quantities of thermal energy. When two substances at different temperatures are mixed, the quantity remaining constant, provided that due allowance is made for external work done and for external loss of heat, is the total quantity of energy. Heat is a form of energy merely because the thing we measure and call heat is really a quantity of energy. Apart from considerations of practical convenience, we might equally well have agreed to measure a quantity of heat in accordance with Carnot's principle, by the external work done in a cycle per degree fall. Heat would then not be a form of energy, but would possess all the properties postulated for caloric. The caloric measure of heat follows directly from Carnot's principle, just as the energy measure follows from the law of conservation of energy. But the term *heat* has become so closely associated with the energy measure that it is necessary to employ a different term, *caloric*, to denote the simple measure of a quantity of heat as opposed to a quantity of heat energy. The measurement of heat as caloric is precisely analogous to the measure of electricity as a quantity of electric fluid. In the case of electricity, the quantity measure is more familiar than the energy measure, because it is generally simpler to measure electricity by its chemical and magnetic effects as a quantity of fluid than as a quantity of energy. The units for which we pay by electric meter, however, are units of energy, because the energy supplied is the chief factor in determining the cost of production, although the actual quantity of fluid supplied has a good deal to do with the cost of distribution. Both methods of measurement are just as important in the theory of heat, and it seems a great pity that the natural measure of heat quantity is obscured in the elementary stages of exposition by regarding heat simply as so much energy. The inadequacy of such treatment makes itself severely felt in the later stages.

Since Carnot's principle was adopted without material modification into the mechanical theory of heat, it was inevitable that Carnot's caloric, and his solution for the work done in a finite cycle, should sooner or later be rediscovered. Caloric reappeared first as the "thermodynamic function" of Rankine, and as the "equivalence-value of a transformation" in the equations of Clausius; but it was regarded rather as the quotient of heat energy by temperature than as possessing any special physical significance. At a later date, when its importance was more fully recognised, Clausius gave it the name of *entropy*, and established the important property that its total quantity remained constant in reversible heat exchanges, but always increased in an irreversible process. Any process involving a decrease in the total quantity of entropy was impossible. Equivalent propositions with regard to the possibility or impossibility of transformations had previously been stated by Lord Kelvin in terms of the dissipation of available energy. But, since Carnot's solution had been overlooked, no one at the time seems to have realised that entropy was simply Carnot's caloric under another name, that heat could be measured otherwise than as energy, and that the increase of entropy in any irreversible process was the most appropriate measure of the quantity of heat generated. Energy so far as we know must always be associated with something of a material nature acting as carrier, and there is no reason to believe that heat energy is an exception to this rule. The tendency of the kinetic theory has always been to regard entropy as a purely abstract mathematical function, relating to the distribution of the energy, but having no physical existence. Thus it is not a quantity of anything in the kinetic theory of gases, but merely the logarithm of the probability of an arrangement. In a similar way, some twenty years ago the view was commonly held that electric phenomena were due merely to strains in the æther, and that the electric fluids had no existence except as a convenient means of mathematical expression. Recent discoveries have enabled us to form a more concrete conception of a charge of electricity, which has proved invaluable as a guide to research. Perhaps it is not too much to hope that it may be possible to attach a similar conception with advantage to caloric as the measure of a quantity of heat.

It has generally been admitted in recent years that some independent measure of heat quantity as opposed to heat energy is required, but opinions have differed widely with regard to the adoption of entropy as the quantity factor of heat. Many of these objections have been felt rather than explicitly stated, and are therefore the more difficult to answer satisfactorily. Others arise from the difficulty of attaching any concrete conception of a quantity of something to such a vague and shadowy mathematical function as entropy. The answer to the question "What is caloric?" must necessarily be of a somewhat speculative nature. But it is so necessary for the experimentalist to reason by analogy from the seen to the unseen, that almost any answer, however crude, is better than none at all. The difficulties experienced in regarding entropy as a measure of heat quantity are more of an academic nature, but may be usefully considered as a preliminary in attempting to answer the more fundamental question.

The first difficulty felt by the student in regarding caloric as the measure of heat quantity is that when two portions of the same substance, such as water, at different temperatures are mixed, the quantity of caloric in the mixture is greater than the sum of the quantities in the separate portions. The same difficulty was encountered by Carnot from the opposite point of view. The two portions at different temperatures represented a possible source of motive-power. The question which he asked himself may be put as follows:—"If the total quantity of caloric remained the same when the two portions at different temperatures were simply mixed, what had become of the motive-power wasted?" The answer is that caloric is generated, and that the quantity generated is such that its energy is the precise equivalent of the motive-power which might have been obtained if the transfer of heat had been effected by means of a perfect engine working without generation of caloric. The caloric generated in wasting a difference of temperature is the necessary and appropriate measure of the quantity of heat obtained by the degradation of available motive-power into the less available or transformable variety of heat energy.

The processes by which caloric is generated in mixing substances at different temperatures, or in other cases where available motive-power is allowed to run to waste, are generally of so turbulent a character that the steps of the process cannot be followed, although the final result can be predicted under given conditions from the energy principle. Such processes could not be expected *a priori* to throw much light on the nature of caloric. The familiar process of conduction of heat through a body the parts of which are at different temperatures, while equally leading to the generation of a quantity of caloric equivalent to the motive-power wasted, affords better promise of elucidating the nature of caloric, owing to the comparative simplicity and regularity of the phenomena, which permit closer experimental study. The earliest measurements of the relative conducting powers of the metals for heat and electricity showed that the ratio of the thermal to the electric conductivity was nearly the same for all the pure metals, and suggested that, in this case, the carriers of heat and electricity were the same. Later and more accurate experiments showed that the ratio of the conductivities was not constant, but varied nearly as the absolute temperature. At first sight this might appear to suggest a radical difference between the two conductivities, but it results merely from the fact that heat is measured as energy in the definition of thermal conductivity, whereas electricity is measured as a quantity of fluid. If thermal conductivity were defined in terms of caloric or thermal fluid, the ratio of the two conductivities would be constant with respect to temperature almost, if not quite, within the limits of error of experiment. On the hypothesis that the carriers are the same for electricity and heat, and that the kinetic energy of each carrier is the same as that of a gas molecule at the same temperature, it becomes possible, on the analogy of the kinetic theory of gases, to calculate the actual value of the ratio of the conductivities. The value thus found agrees closely in magnitude with that given by experiment, and may be regarded as confirming the view that the carriers are the same, although the hypotheses and analogies invoked are somewhat speculative.

When the electrons or corpuscles of negative electricity were discovered it was a natural step to identify them with the carriers of energy, and to imagine that a metal contained a large number of such corpuscles, moving in all directions, and colliding with each other and with the metallic atoms, like the molecules of a gas on the kinetic theory. If the mass of each carrier were 1/1700 of that of an atom of hydrogen, the velocity at 0° C. would be about sixty miles a second, and would be of the right order of magnitude to account for the observed values of the conductivities of good conductors, on the assumption that the number of negative corpuscles was the same as the number of positive metallic atoms, and that the mean free path of each corpuscle was of the same order as the distance between the atoms. The same hypothesis served to give a qualitative account of thermo-electric phenomena, such as the Peltier and Thomson effects, and of radiation and absorption of heat, though in a less satisfactory manner. When extended to give a consistent account of *all* the related phenomena, it would appear that the number of free corpuscles required is too large to be reconciled, for instance, with the observed values of the specific heat, on the assumption that each corpuscle possesses energy of translation equal to that of a gas molecule at the same temperature.

Sir J. J. Thomson has accordingly proposed and discussed another possible theory of metallic conduction, in which the neutral electric doublets present in the metal are supposed to be continually interchanging corpuscles at a very high rate. Under ordinary condition these interchanges take place indifferently in all directions, but under the action of an electric field the axes of the doublets are supposed to become more or less oriented, as in the Grotthus-chain hypothesis of electrolytic conduction, producing a general drift or current proportional to the field. This hypothesis, though fundamentally different from the preceding or more generally accepted view, appears to lead to practically the same relations, and is in some ways preferable, as suggesting possible explanations of difficulties encountered by the first theory in postulating so large a number of free negative corpuscles. On the other hand, the second theory requires that each neutral doublet should be continually ejecting corpuscles at the rate of about 10^{15} per second. There are probably elements of truth in both theories, but, without insisting too much on the exact details of the process, we may at least assert with some confidence that the corpuscles of caloric which constitute a current of heat in a metal are very closely related to the corpuscles of electricity, and have an equal right to be regarded as constituting a material fluid possessing an objective physical existence.

If I may be allowed to speculate a little on my own account (as we are all here together in holiday mood, and you will not take anything I may say too seriously), I should prefer to regard the molecules of caloric, not as being identical with the corpuscles of negative electricity, but as being neutral doublets formed by the union of a positive and negative corpuscle, in much the same way as a molecule of hydrogen is formed by the union of two atoms. Nothing smaller than a hydrogen atom has yet, so far as I know, been discovered with a positive charge. This may be merely a consequence of the limitations of our experimental methods, which compel us to employ metals to so large an extent as electrodes. In the symmetry of nature it is almost inconceivable that the positive corpuscle should not exist, if only as the other end of the Faraday-tube or vortex-filament representing a chemical bond. Prof. Bragg has identified the X or γ rays with neutral corpuscles travelling at a high velocity, and has maintained this hypothesis with brilliant success against the older view that these rays are not separate entities, but merely thin, spreading pulses in the æther produced by the collisions of corpuscles with matter. I must leave him to summarise the evidence, but if neutral corpuscles exist, or can be generated in any way, it should certainly be much easier to detach a neutral corpuscle from a material atom or molecule than to detach a corpuscle with a negative charge from the positive atom with which it is associated. We should therefore expect neutral corpuscles to be of such exceedingly common and universal occurrence that their very existence might be overlooked, unless they happened to be travelling at such exceptionally high velocities as are associated with the γ rays. According to the pulse theory, it is assumed that all γ rays travel with the velocity of light, and that the enormous variations observed in their penetrative power depend simply on the thickness of the pulse transmitted. On the corpuscular theory, the penetrative power, like that of the α and β rays, is a question of size, velocity, and electric charge. Particles carrying electric charges, like the α and β rays, lose energy in producing ions by their electric field, perhaps without actual collision. Neutral or γ rays do not produce ions directly, but dislodge either γ rays or β rays from atoms by direct collisions, which are comparatively rare. The β rays alone, as C. T. R. Wilson's photographs show, are responsible for the ionisation. Personally, I have long been a convert to Prof. Bragg's views on the nature of X rays, but even if we regard the existence of neutral corpuscles as not yet definitely proved, it is, I think, permissible to assume their existence for purposes of argument, in order to see whether the conception may not be useful in the interpretation of physical phenomena.

If, for instance, we assume that the neutral corpuscles or molecules of caloric exist in conductors and metallic bodies in a comparatively free state of solution, and are readily dissociated into positive and negative electrons owing to the high specific inductive capacity of the medium, the whole theory of metallic conduction follows directly on the analogy of conduction in electrolytic solutions. But, whereas in electrolytes the ions are material atoms moving through a viscous medium with comparatively low velocities, the ions in metallic conductors are electric corpuscles moving with high velocities more after the manner postulated in the kinetic theory of gases. It is easy to see that this theory will give similar numerical results to the electronic theory when similar assumptions are made in the course of the work. But it has the advantage of greater latitude in explaining the vagaries of sign of the Hall effect, and many other peculiarities in the variation of resistance and thermo-electric power with temperature. For good conductors, like the pure metals, we may suppose, on the electrolytic analogy, that the dissociation is practically complete, so that the ratio of the conductivities will approach the value calculated on the assumption that all the carriers of heat are also carriers of electricity. But in bad conductors the dissociation will be far from complete, and it is possible to see why, for instance, the electric resistance of cast-iron should be nearly ten times that of pure iron, although there is comparatively little difference in their thermal conductivities. The numerical magnitude of the thermo-electric effect, which is commonly quoted in explanation of the deviation of alloys from the electronic theory, is far too small to produce the required result; and there is little or no correspondence between the thermo-electric properties of the constituents of alloys and the variations of their electric conductivities.

One of the oldest difficulties of the material theory of heat is to explain the process of the production of heat by friction. The application of the general principle of the conservation of energy leads to the undoubted conclusion that the thermal energy generated is the equivalent of the mechanical work spent in friction, but throws little or no light on the steps of the process, and gives no information with regard to the actual nature of the energy produced in the form of heat. It follows from the energy principle that the quantity of caloric generated in the process is such that its total energy at the final temperature is equal to the work spent. If a quantity of caloric represents so many neutral molecules of electricity, one cannot help asking where they came from, and how they were produced. It is certain that in most cases of friction, wherever slip occurs, some molecules are torn apart, and the work spent is represented in the first instance by the separation of electric ions. Some of these ions are permanently separated as frictional electricity, and can be made to perform useful work; but the majority recombine before they can be effectively separated, leaving only their equivalent in thermal energy. The recombination of two ions is generally regarded simply as reconstituting the original molecule at a high temperature, but in the light of recent discoveries we may perhaps go a step further. It is generally admitted that X or γ rays are produced by the sudden stoppage of a charged corpuscle, and Lorentz, in his electron theory of radiation, has assumed that such is the case however low the velocity of the electron. A similar effect must occur in the sudden stoppage of a pair of ions rushing together under the influence of their mutual attraction. Rays produced in this way would be of an exceedingly soft or absorbable character, but they would not differ in kind from those produced by electrons except that their energy, not exceeding that of a pair of ions, would be too small to produce ionisation, so that they could not be detected in the usual way. If the X rays are corpuscular in their nature, we cannot logically deny the corpuscular character even to the slowest moving rays. We know that X rays continually produce other X rays of lower velocity. The final stage is probably reached when the average energy of an X corpuscle or molecule of caloric is the same as that of a gas molecule at the same temperature, and the number of molecules of caloric generated is such that their total energy is equal to the work originally spent in friction.

In this connection it is interesting to note that Sir J. J. Thomson, in a recent paper on ionisation by moving particles, has arrived, on other grounds, at the conclusion that the character of the radiation emitted during the recombination of the ions will be a series of pulses, each pulse containing the same amount of energy and being of the same type as very soft X rays. If the X rays are really corpuscular, these definite units or quanta of energy generated by the recombination of the ions bear a close resemblance to the hypothetical molecules of caloric.

It may be objected that in many cases of friction, such as internal or viscous friction in a fluid, no electrification or ionisation is observable, and that the generation of caloric cannot in this case be attributed to the recombination of ions. It must, however, be remarked that the generation of a molecule of caloric requires less energy than the separation of two ions; that, just as the separation of two ions corresponds with the breaking of a chemical bond, so the generation of one or more molecules of caloric may correspond with the rupture of a physical bond, such as the separation of a molecule of vapour from a liquid or solid. The assumption of a molecular constitution for caloric follows almost of necessity from the molecular theories of matter and electricity, and is not inconsistent with any well-established experimental facts. On the contrary, the many relations which are known to exist between the specific heats of similar substances, and also between latent heats, would appear to lead naturally to a molecular theory of caloric. For instance, it has often been noticed that the molecular latent heats of vaporisation of similar compounds at their boiling-points are proportional to the absolute temperature. It follows that the molecular latent caloric of vaporisation is the *same* for all such compounds, or that they require the same number of molecules of caloric to effect the same change of state, irrespective of the absolute temperatures of their boiling-points. From this point of view one may naturally regard the liquid and gaseous states as conjugate solutions of caloric in matter and matter in caloric respectively. The proportion of caloric to matter varies regularly with pressure and temperature, and there is a definite saturation limit of solubility at each temperature.

One of the most difficult cases of the generation of caloric to follow in detail is that which occurs whenever there is exchange of heat by radiation between bodies at different temperatures. If radiation is an electro-magnetic wave-motion, we must suppose that there is some kind of electric oscillator or resonator in the constitution of a material molecule which is capable of responding to the electric oscillations. If the natural periods of the resonators correspond sufficiently closely with those of the incident radiation the amplitude of the vibration excited may be sufficient to cause the ejection of a corpuscle of caloric. It is generally admitted that the ejection of an electron may be brought about in this manner, but it would evidently require far less energy to produce the emission of a neutral corpuscle, which ought therefore to be a much more common effect. On this view, the conversion of energy of radiation into energy of caloric is a discontinuous process taking place by definite molecular increments, but the absorption or emission of radiation itself is a continuous process. Prof. Planck, by a most ingenious argument based on the probability of the distribution of energy among a large number of similar electric oscillators (in which the entropy is taken as the logarithm of the probability, and the temperature as the rate of increase of energy per unit of entropy), has succeeded in deducing his well-known formula for the distribution of energy in full radiation at any temperature; and has recently, by a further extension of the same line of argument, arrived at the remarkable conclusion that, while the absorption of radiation is continuous, the emission of radiation is discontinuous, occurring in discrete elements or quanta. Where an argument depends on so many intricate hypotheses and analogies the possible interpretations of the mathematical formulæ are to some extent uncertain; but it would appear that Prof. Planck's equations are not necessarily inconsistent with the view above expressed that both emission and absorption of radiation are continuous, and that his *elementa quanta*, the energy of which varies with their frequency, should rather be identified with the molecules of caloric, representing the conversion of the electro-magnetic energy of radiation into the form of heat, and possessing energy in proportion to their temperature.

Among the difficulties felt, rather than explicitly stated, in regarding entropy or caloric as the measure of heat quantity is its awkward habit of becoming infinite, according to the usual approximate formulæ, at extremes of pressure or temperature. If caloric is to be regarded as the measure of heat quantity, the quantity existing in a finite body must be finite, and must vanish at the absolute zero of temperature. In reality there is no experimental foundation for any other conclusion. According to the usual gas formulæ it would be possible to extract an infinite quantity of caloric from a finite quantity of gas by compressing it at constant temperature. It is true that (even if we assumed the law of gases to hold up to infinite pressures, which is far from being the case) the quantity of caloric extracted would be of an infinitely low order of infinity as compared with the pressure required. But, as a matter of fact, experiment indicates that the quantity obtainable would be finite, although its exact value cannot be calculated owing to our ignorance of the properties of gases at infinite pressures. In a similar way, if we assume that the specific heat as ordinarily measured remains constant, or approaches a finite limit at the absolute zero of temperature, we should arrive at the conclusion that an infinite quantity of caloric would be required to raise the temperature of a finite body from 0° to 1° absolute. The tendency of recent experimental work on specific heats at low temperatures, by Tilden, Nernst, Lindemann, and others, is to show, on the contrary, that the specific heats of all substances tend to vanish as the absolute zero is approached, and that it is the specific capacity for caloric which approaches a finite limit. The theory of the variation of the specific heats of solids at low temperatures is one of the most vital problems in the theory of heat at the present time, and is engaging the attention of many active workers. Prof. Lindemann, one of the leading exponents of this work, has kindly consented to open a discussion on the subject in our section. We are very fortunate to have succeeded in securing so able an exponent, and shall await his exposition with the greatest interest. For the present I need only add that the obvious conclusion of the caloric theory bids fair to be completely justified.

A most interesting question, which early presented itself to Rumford and other inquirers into the caloric theory of heat, was whether caloric possessed *weight*. While a positive answer to this question would be greatly in favour of a material theory, a negative answer, such as that found by Rumford, or quite recently by Profs. Poynting and Phillips, and by Mr. L. Southerns working independently, would not be conclusively against it. The latter observers found that the change in weight, if any, certainly did not exceed 1 in 10^8 per 1° C. If the mass of a molecule of caloric were the same as that generally attributed to an electron, the change of weight, in the cases tested, should have been of the order of 1 in 10^7 per 1° C., and should not have escaped detection. It is generally agreed, however, that the mass of the electron is entirely electro-magnetic. Any such statement virtually assumes a particular distribution of the electricity in a spherical electron of given size. But if electricity itself really consists of electrons, an argument of this type would appear to be so perfectly circular that it is questionable how much weight should be attached to it. If the equivalent mass of an electron in motion arises slowly from the electro-magnetic field produced by its motion, a neutral corpuscle of caloric should not possess mass or energy of translation as a whole, though it might still possess energy of vibration or rotation of its separate charges. For the purpose of mental imagery we might picture the electron as the free or broken end of a vortex filament, and the neutral corpuscle as a vortex ring produced when the positive and negative ends are united; but a mental picture of this kind does not carry us any further than the sphere coated with electricity, except in so far as either image may suggest points for experimental investigation. In our ignorance of the exact mechanism of gravity it is even conceivable that a particle of caloric might possess mass without possessing weight, though, with the possible exception of the electron, nothing of the kind has yet been demonstrated. In any case it would appear that the mass, if any, associated with a quantity of caloric must be so small that we could not hope to learn much about it by the direct use of the balance.

The fundamental property of caloric, that its total quantity cannot be diminished by any known process and that it is not energy but merely the vehicle or carrier of energy, is most simply represented in thought by imagining it to consist of some indestructible form of matter. The further property, that it is always generated in any turbulent or irreversible process, appears at first sight to conflict with this idea, because it is difficult to see how anything indestructible can be so easily generated. When, however, we speak of caloric as being generated, what we really mean is that it becomes associated with a material body in such a way that we can observe and measure its quantity by the change of state produced. The caloric may have existed previously in a form in which its presence could not be detected. In the light of recent discoveries we might suppose the caloric generated to arise from the disintegration of the atoms of matter. No doubt some caloric is produced in this way, but those corpuscles that are so strongly held as to be incapable of detection by ordinary physical methods require intense shocks to dislodge them. A more probable source of caloric is the æther, which, so far as we know, may consist entirely of neutral corpuscles of caloric. The hypothesis of a continuous æther has led to great difficulties in the electro-magnetic theory of light and in the kinetic theory of gases. A molecular, or cellular-vortex, structure appears to be required. According to the researches of Kelvin, Fitzgerald, and Hicks, such an æther can be devised to satisfy the requirements of the electro-magnetic theory without requiring it to possess a density many times greater than that of platinum. So far as the properties of caloric are concerned, a neutral pair of electrons would appear to constitute the simplest type of molecule, though without more exact knowledge of the ultimate nature of an electric charge it would be impossible to predict all its properties. Whether an æther composed of such molecules would be competent to discharge satisfactorily all the onerous functions expected from it, may be difficult to decide, but the inquiry, in its turn, would probably throw light on the ultimate structure of the molecule.

Without venturing too far into the regions of metaphysical speculation, or reasoning in vicious circles about the nature of an electric charge, we may at least assert with some degree of plausibility that material bodies under ordinary conditions probably contain a number of discrete physical entities, similar in kind to X rays or neutral corpuscles, which are capable of acting as carriers of energy, and of preserving the statical equilibrium between matter and radiation at any temperature in virtue of their interchanges with electrons. If we go a step further and identify these corpuscles with the molecules of caloric, we shall certainly come in conflict with some of the fundamental dogmas of the kinetic theory, which tries to express everything in terms of energy, but the change involved is mainly one of point of view or expression. The experimental facts remain the same, but we describe them differently. Caloric has a physical existence, instead of being merely the logarithm of the probability of a complex ion. In common with many experimentalists, I cannot help feeling that we have everything to gain by attaching a material conception to a quantity of caloric as the natural measure of a quantity of heat as opposed to a quantity of heat energy. In the time at my disposal I could not pretend to offer you more than a suggestion of a sketch, an apology for the possibility of an explanation, but I hope I may have succeeded in conveying the impression that a caloric theory of heat is not so entirely unreasonable in the light of recent experiment as we are sometimes led to imagine.

NOTES.

DR. G. T. BEILBY, F.R.S., has been appointed a member of the Royal Commission on Oil Fuel in succession to the late Dr. H. Owen Jones.

THE death is announced, at eighty years of age, of Prof. T. Gomperz, of the University of Vienna, distinguished by his studies in philology and philosophy, and well known by his work "Greek Thinkers," of which an English translation appeared several years ago.

As previously announced, the autumn meeting of the Institute of Metals will be held in London on Wednesday and Thursday, September 25 and 26. The following are among the papers that are expected to be submitted:—Autogenous welding by means of oxygen and acetylene of copper and its principal alloys, and of aluminium, Prof. F. Carnevali; the effect of other metals on the structure of the beta constituent in copper-zinc alloys, Prof. H. C. H. Carpenter; the effect of temperatures higher than atmospheric on tensile tests of copper and its alloys, Prof. A. K. Huntington; the influence of oxygen on the properties of metals and alloys, E. F. Law; the annealing of coinage alloys, Dr. T. Kirke Rose; intercrystalline cohesion in metals (with an appendix on the formation of twinned crystals in silver), Dr. W. Rosenhain and D. Ewen; oxygen in brass, Prof. T. Turner.

WE regret to announce that Prof. T. Winter, professor of agriculture in University College of North Wales, Bangor, died on Sunday, September 1, at forty-six years of age. Prof. Winter was educated at Darlington Grammar School and Edinburgh University, where he graduated in arts. He afterwards became assistant lecturer on agriculture at the University College of North Wales. Later he was appointed lecturer in agriculture at the University of Leeds; and in 1894 he returned to the University College of North Wales as head of the agricultural department. He took an active part in agricultural matters in Wales, and was widely known and respected throughout the country.

WE are glad to see that progress is gradually being made with the synchronisation of clocks, thanks largely to the enterprise of private companies. Last year a committee of the British Science Guild presented a valuable report upon the position of the subject and the system employed by the General Post Office, and an instructive account of synchronisation and the importance of correct time is given by Major O'Meara in an address printed in this year's report of the Guild. The committee recommended that, as a beginning, it would probably be well to have a few large public clocks in London synchronised, and that these should be set apart and considered as "standard time clocks." An electric clock which may be used for the purpose suggested by the committee has just been built by the Silent Electric Clock Co., 192 Goswell Road, London, E.C., on the new mills of the Hovis Bread Co., Vauxhall Bridge Road. We understand that this electric clock, with its four faces each 9 ft. 6 in. diameter, is not only the largest electric clock in London, but is also to be controlled by a master clock directly synchronised from Greenwich. The clock thus represents an up-to-date form of public timekeeper which is likely to be extensively adopted in the future.

A LOCAL society which possesses such a creditable record of work as the Royal Cornwall Polytechnic Society does well to commemorate worthies who were members of their body. In the first part of its Proceedings for 1912 it publishes portraits and lives of three of its most eminent members, Sir C. Lemon, F.R.S., first president (1833–67), who did good service to science by his attempt to found a school of mines at Truro, a project which was in advance of the times when it was proposed, but has been since realised; Lord de Dunstanville, first patron, scholar and politician; and last, but not least, Davies Gilbert, who succeeded Sir Humphry Davy as president of the Royal Society, an accomplished botanist and distinguished in other branches of science. In the annual report the council takes occasion to congratulate the Rev. Philip Carlyon, a former vice-president of the society, on attaining the age of a hundred years in December last.

VOLUME xi. of the Zoological Publications of the Field Museum is devoted to an account of the mammals of Illinois and Wisconsin, comprising 502 pages of text and a large number of illustrations. "Keys" to the various genera and their species are given.

IN the report of the Field Museum of Natural History, Chicago, for 1911, the director refers to the acquisition by the trustees of a site for a new building in Jackson Park, immediately to the north of the present structure. The plans for the new building have been approved, and the specifications for the contracts drawn up. The report is illustrated with photographs of bird groups and other interesting exhibits recently added to the museum.

THE Meteorological Service of Canada has issued a very useful pamphlet on the comparison of the Ångström pyrheliometer and the Callendar sunshine recorder, and the determination of the proportion of heat received on a horizontal surface from the diffuse radiation from the sky to that received from the sun. The International Union for Cooperation in Solar Research at its Oxford conference recommended (1) the adoption of the former instrument, and (2) comparisons between it and other standard instruments, but except at laboratories and the larger observatories little is yet generally known about its working. The paper in question, prepared by Mr. J. Patterson, under the direction of Mr. R. F. Stupart, gives a very clear idea of the construction and action of both the above instruments. The following are among the noteworthy features shown by their comparison: (1) the maximum intensity of radiation measured by the Ångström instrument occurred at apparent noon, and by the Callendar recorder about forty minutes later. (2) The Ångström instrument gave slightly higher values in the afternoon than in the morning, and the Callendar recorder much higher values. (3) In the early morning and late afternoon the Callendar instrument gave higher readings than the Ångström. (4) Excluding the morning readings the greatest percentage difference occurred between 9h. and 10h. a.m.; from about 1h. to 3h. p.m. the change in percentage was very slight.

SIR T. L. HEATH has now supplied an English edition (Cambridge Press, 2*s.* 6*d.*) of the "Method" of Archimedes, discovered by Heiberg in 1906. This tract is of very great interest, because it gives mechanical discussions of geometrical problems based upon the principle of the lever. Thus we have the rule for the quadrature of a parabolic segment, which Archimedes elsewhere proves by the method of exhaustion. Archimedes expressly says that the "Method" does not supply demonstrations; he does not give any reasons, but no doubt he had in mind what we should call the theory of infinitesimals of different orders. For example, a triangular lamina may be roughly, but not exactly, replaced by a set of parallel rectangular strips; to find the centroid of the triangle we must find the *limiting* position of the centroid of the system of strips. Among other noteworthy points it may be observed that Archimedes arrived at the formula for the volume of a sphere before he discovered that for its area; and that he attributes to Democritus the discovery of the theorem that pyramids of equal bases and altitudes are of equal volume. The first proof, allowed to be rigorous, he assigns (as elsewhere) to Eudoxus. As usual, the editor's task is performed with great learning and thoroughness; his introduction in particular will be found extremely useful by those who are not familiar with Greek mathematics beyond the elementary stage.

The Central—the journal of the City and Guilds Engineering College—for August contains an article advocating the use of direct rather than alternating currents in electric traction by Mr. L. Calisch, an account of some recent improvements in vacuum evaporation by Mr. W. A. Davis, and a description of the Boncourt system of gaseous combustion by one of its inventors, Mr. C. D. McCourt. The feature of the system is the combustion of the gas and air mixture as it is passing with the requisite velocity through the interstices of a granular refractory material. A steam boiler fired in this way evaporates 16 lbs. of water per hour per square foot of heating surface. The old student notes occupy fifteen pages. Referring to work in the drawing office of a French engineering firm, Mr. K. C. Barnaby writes:—". . . there is the delightful metric system. I cannot imagine anyone who has worked and calculated in a Continental office who would not wish our antiquated system of weights and measures—well, where parallels meet."

THE fifty-seventh annual exhibition of the Royal Photographic Society, which was opened last Monday, will remain open until the 21st inst. at the Gallery of the Royal Society of British Artists, Suffolk Street, Pall Mall. In the scientific and technical sections four exhibits have been awarded medals. The first consists of examples of a new photo-mechanical process by Mr. A. E. Bawtree, who has found a method of transferring the pigment of an impression from an engraved plate, whether it is old or new, to a sheet of glass, so producing a more perfect transparency than any camera method can yield. He claims that not a grain of the pigment is lost. From this transparency copies of the original may be made by various photographic or photo-mechanical methods as is well known. He can then retransfer the pigment from the glass to paper without the loss of even the finest detail. The method of transfer is so easy that the author does not yet describe it, because it enables facsimiles of bank-notes and such documents to be prepared with a very moderate outlay for apparatus. Dr. D. H. Hutchinson's series of photomicrographs of the ova of the Mexican Axolotl show the development of the embryo from the first day after the egg has been laid up to the time of its escape from the egg. This, and Mr. Farren's series of photographs of the little egret, and Mr. G. Busby's autochrome landscape, well deserve the medals that have been awarded them. Among the numerous other exhibits we may perhaps direct special attention to the radiographs of Dr. Hall-Edwards, which show the effect of bismuth salts and iodoform in indicating details with great clearness, Dr. Thurstan Holland's "plastic" radiographs, Dr. T. W. Butcher's high-power photomicrographs, and Dr. Rodman's stereo-photomicrographs of the scales on the wings of moths and butterflies and the hairs on the leaves of plants, though it seems almost invidious to do so where so much good work is shown illustrating many different branches of work.

PARTS ii. and iii. of the Subject List of Works on Mineral Industries in the Library of the Patent Office have just been published at the office, 25 Southampton Buildings, Chancery Lane, W.C., price sixpence each. Part ii. contains classified titles of works on iron manufacture, alloys, and metallography, and part iii. those relating to metallurgy (non-ferrous and general), assaying, and fuel combustion. The lists, like others in the same series, are most helpful guides to the contents of a very valuable library.

OUR ASTRONOMICAL COLUMN.

THE SPECTRUM OF BROOKS'S COMET, 1911*c*.—Some excellent spectrograms of comet 1911*c* are reproduced and their special features discussed by MM. de la Baume Pluvinel and Baldet in the September number of *L'Astronomie*. The spectrographs employed were mounted at the Juvisy Observatory, and an examination of the complete series of plates shows very markedly the spectral changes which took place as the comet approached the sun; between August and the end of October a number of "unknown" radiations between λ 4100 and λ 4000 suffered a considerable diminution of intensity as compared with other radiations. The wave-lengths of these lines, considered precise to 1 Å, are 4099, 4074, 4065, 4051, 4041, 4032, and 4016. These radiations were peculiar to the nucleus of the comet, being found neither in the coma nor the tail, and as they became fainter the tail radiations became strong; it was also noted that in the later spectra the tail radiations extended well to the front of the comet's head, showing that in active comets, such as this one and Morehouse's, the tail matter is expelled in all directions. In Kiess's comet it appeared to escape from one point only. Altogether 47 monochromatic images of the nucleus were counted on the Juvisy plates, but the kathode spectrum of nitrogen was not recognised among them.

THE CORONA AT THE TOTAL SOLAR ECLIPSE OF APRIL 17.—A drawing of the corona, made by Señor J. Comas Solá, at Barco de Valdeorras (Galicia), on April 17, appears in No. 4597 of the *Astronomische Nachrichten*. Although observers at other stations were uncertain as to the definite apparition of the corona, Señor Comas Solá saw it well extended, and on his drawing depicts it extending equatorially to about 2½ solar diameters on either side of the sun. The drawing, given principally to show the general form, represents a corona distinctly of the minimum type. The same observer also describes his spectrum observations, while many others give the results of observations of the contacts, &c.

THE DIAMETER OF NEPTUNE.—An interesting paper by Dr. G. Abetti, discussing the various measures of Neptune made since 1846, appears in No. 8, vol. i. (second series), of the *Memorie della Società degli Spettroscopisti Italiani*. He shows that the measured diameter has, in general, tended to become less as the aperture and magnification employed have increased. Using only the results from apertures of more than 40 c.m. and magnifications greater than 620, the mean values being 76 c.m. and 794 respectively, the diameter at unit distance comes out as 69·04″ for the mean aperture, and 68·98″ for the mean power; other considerations show that the true value differs but little from 69″. Using this value, he then calculates the true diameter as 5×10^4 km., the density (earth = 1) as 0·29 or (water = 1) 1·6, and the superficial gravity as 1·12, that at the earth's equator being taken as 1·0. As seen from the earth, the apparent diameter ranges between 2·39″ and 2·20″.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

BIRMINGHAM.—The University has suffered a severe loss by the death of the Vice-Chancellor, Alderman Charles Gabriel Beale, at the early age of 69. Alderman Beale, who was a graduate of Trinity College, Cambridge, was one of the most prominent citizens of Birmingham, having been elected to the mayoral chair no fewer than four times. He was mainly instrumental in carrying to a successful conclusion the great scheme for supplying the city with water from the Welsh mountains. He was, from the outset, a most energetic supporter of the movement for establishing a University in Birmingham, and was largely responsible for the working-out of the scheme, for which his legal training and experience qualified him in an unusual degree. When the University became an accomplished fact in 1900, his services to the cause were fittingly recognised by his appointment as the first Vice-Chancellor. His ideas were on a large scale, and he believed in the importance of associating the University with buildings which by their imposing size and appearance should appeal to local patriotism and serve to keep before the inhabitants of a great industrial centre the claims of higher education. Within the University he was known to the undergraduates for his special interest in their social welfare.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 26.—M. A. Bassot in the chair.—Édouard **Heckel**: The cultural bud mutation of *Solanum tuberosum*. An account of experiments in the cultivation of wild potato plants from Chile, Bolivia, and Peru. The tubers produced from the cultivated plants were edible, and contained a greater amount of starch than the wild plants. The tubercles from Bolivia showed the characters of mutation; those from other sources appeared to be in course of mutation.—W. H. **Young**: The summability of a function of which the Fourier's series is given.—B. **Bianu** and L. **Wertenstein**: An ionising radiation, attributable to the radio-active recoil, emitted by polonium. It was found to be necessary to use a polonium film in these experiments not exceeding $10\mu\mu$ in thickness. The curves obtained with a silver disc covered with this thin polonium layer, in presence of a transversal magnetic field of 1100 units, were analogous with those obtained in the case of radium C, and show clearly the existence of an absorbable radiation.—J. **Bougault**: Benzylpyruvic acid. The acid was prepared by the action of alkaline solutions on phenyl-α-oxycrotonamide. The yields of benzylpyruvic acid were good. The condensation products of this acid with itself and with acetone were also studied.—H. **Vincent**: The active immunisation of man against typhoid fever. Details of five cases are given which show that inoculations of typhovaccin have a preventive power not only against subsequent absorption of typhoid cultures, but also against a recent infection anterior to the inoculation.—Charles **Nicolle**, L. **Blaizot**, and E. **Conseil**: The conditions of transmission of recurrent fever by the flea. The evidence is against the assumption of hereditary transmission in the flea. Details are given of studies in the necessary conditions for infection.—J. **Wolff**: The stimulating action of alkalies and of ammonia in particular on peroxydase.—P. **Chaussé**: The vitality of the tubercle bacillus tested by inoculation and by inhalation.

BOOKS RECEIVED.

Notes on Algebra. By A. F. van der Heyden. Pp. viii+133. (Middlesbrough: W. Appleyard and Sons, Ltd.) 2*s*. 6*d*.

Exercises in Modern Arithmetic. By H. S. Jones. Pp. x+336. (London: Macmillan and Co., Ltd.) 2*s*. 6*d*.

British Rainfall, 1911. By Dr. H. R. Mill. Pp. 388. (London: E. Stanford, Ltd.) 10*s*.

Life Understood from a Scientific and Religious Point of View, &c. By F. L. Rawson. Pp. xv+660. (London: The Crystal Press, Ltd.) 7*s*. 6*d*. net.

Identification of the Economic Woods of the United States. By Prof. S. J. Record. Pp. vii+117+6 plates. (New York: J. Wiley and Sons; London: Chapman and Hall, Ltd.) 5*s*. 6*d*. net.

Forestry in New England. By Profs. R. C. Hawley and A. F. Hawes. Pp. xv+479. (New York: J. Wiley and Sons; London: Chapman and Hall, Ltd.) 15*s*. net.

Dove Marine Laboratory, Cullercoats, Northumberland. Report for the year ending June 30, 1912. New Series. I. Edited by Prof. A. Meek. (Newcastle-on-Tyne: Cail and Sons.) 5*s*.

Catalogue of the Periodical Publications including the Serial Publications of Societies and Governments in the Library of University College, London. By L. Newcombe. Pp. vii+269. (Oxford: H. Hart.)

Catalogue of the Periodical Publications in the Library of the Royal Society of London. Pp. viii+455. (London: H. Frowde.)

Results of the Magnetical and Meteorological Observations made at the Royal Alfred Observatory, Mauritius, in the year 1902. Pp. xxii+lxxviii+5 plates. Ditto, 1903. Pp. xxi+lxxiv+7 plates. Ditto, 1908. Pp. xxv+lxxxviii+6 plates. (Mauritius.)

An Introduction to the Study of the Protozoa, with special reference to the Parasitic Forms. By Prof. E. A. Minchin. Pp. xi+520. (London: E. Arnold.) 21*s*. net.

Eugenics and Public Health. By Prof. K. Pearson. Pp. 34. (London: Dulau and Co., Ltd.) 1*s*. net.

Darwinism, Medical Progress, and Eugenics. The Cavendish Lecture, 1912. By Prof. K. Pearson. Pp. 29+7 plates. (London: Dulau and Co., Ltd.) 1*s*. net.

Instinct and Experience. By Prof. C. Lloyd Morgan. Pp. xvii+299. (London: Methuen and Co., Ltd.) 5*s*. net.

Lebensbild eines Naturforschers. By E. du Bois-Reymond. Zweite Auflage. Pp. 50. (Brackwede i.W: Dr. W. Breitenbach.) 80 pfennigs.

Grundriss der Biochemie für Studierende und Aerzte. By Prof. C. Oppenheimer. Pp. vii+399. (Leipzig: G. Thieme.) 9 marks.

The Boy's Playbook of Science. By J. H. Pepper. Revised, &c., by Dr. J. Mastin. Pp. x+680. (London: G. Routledge and Sons, Ltd.) 5*s*.

Dana's Manual of Mineralogy. Thirteenth edition. By Prof. W. E. Ford. Pp. viii+460. (New York: J. Wiley and Sons; London: Chapman and Hall, Ltd.) 8*s*. 6*d*. net.

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