THURSDAY, SEPTEMBER 12, 1895.

A NEW STANDARD DICTIONARY.

A Standard Dictionary of the English Language. Vol. ii. Prepared under the supervision of Dr. I. K. Funk, Dr. F. A. March, and Dr. D. S. Gregory. (New York and London: Funk and Wagnall Co., 1895.)

SINCE the appearance of the first volume of this work, noticed in NATURE, vol. l. p. 146, we have often had occasion to refer to it, and have formed opinions as to its merits and faults. In many respects the dictionary is a very good one within its compass, though it does not contain much that is really new.

Before going further, it may be well to state briefly the magnitude of the work, and to give a general idea of its characteristics. The two volumes run into 2338 pages and contain 301,865 vocabulary terms, embellished by 5000 illustrations. A point upon which great stress is put is that more than two hundred editors and specialists have assisted in the production of the work, though it is not clear to what extent this assistance was given. Their services, with those of the five hundred readers for quotations, who are said to have been engaged upon this work, have helped to bring the cost up to one million dollars! Considering how little there is in the dictionary that is not in the "Century," "International," and other American dictionaries, one wonders where the money has gone. This, however, is by the way, and we only mention the matter because the large amount stated to have been spent in the production of the dictionary is put forward as a claim to favour.

A few definitions from the work will be the best means of indicating its merits. A whole column of the dictionary is taken up with definitions, and examples, of the use of the word science and its synonyms. The first two of the six definitions given are as follows:—

Science.—(1) Knowledge gained and verified by exact observation and correct thinking, especially as methodically formulated and arranged in a rational system; also, the sum of universal knowledge.

(2) Any department of knowledge in which the results of investigation have been worked out and systematised; an exact and systematic statement of knowledge concerning some subject or group of subjects; especially, a system of ascertained facts and principles covering and attempting to give adequate expression to a great natural group or division of knowledge.

The sciences are divided in the dictionary into (1) the mathematical, treating of quantity; (2) the physical, treating of matter and its properties; (3) the biological, treating of the phenomena of life; (4) the anthropological, treating of man; and (5) the theological, treating of the Deity. All the divisions are fully treated under their respective heads. Thus, under physical sciences, the classification of them as sciences of energy is given; the biological sciences are fully tabulated and their relation to one another shown with all their sub-divisions, and anthropology is made to embrace all the sciences relating to man. The departments of anthropology presented in the dictionary are (*a*) Somatology, (*b*) Ethnology, (*c*) Archæology. It is worth while printing the definition of the third of these for the benefit of unscientific archæologists.

Archæology.—The science of antiquities; in its widest sense, the branch of anthropology, embracing archæography, concerned with the systematic investigation of the relics of man and of his industries, and the classification and treatment of ancient remains and records of any or every kind, whether historic or prehistoric, of ancient places, customs, arts, &c.

In popular signification, archæology refers mainly to the collection or investigation of the materials from which a knowledge of the particular country under investigation may be obtained, which materials may be divided into *written*, *monumental*, and *traditional*. Scientific archæology is (1) general, including (*a*) the geology of the epoch of man and (*b*) the prehistoric ages; and (2) special, including the study of separate nations and areas.

These examples, which could be multiplied many times, are sufficient to show the generally trustworthy character, and the fulness, of the definitions, so far as science is concerned. The work has an attractive appearance, and offers every facility for consultation, and is altogether a desirable addition to a library.

THE CHEMISTRY OF LIGHTING.

Chemical Technology, or Chemistry in its Applications to Arts and Manufactures. Edited by C. E. Groves, F.R.S., and W. Thorp, B.Sc. Vol. ii. Lighting. (London: J. & A. Churchill, 1895.)

THE second volume of this important work possesses great intrinsic worth. Section i., dealing with fats and oils, by W. Y. Dent, contains much information concisely and clearly expressed. It may be noted that, in connection with the determination of specific gravity, the Sprengel tube is described, but no mention is made of the modification of this apparatus having the capillary arms at right angles and provided with expansion bulbs, although the latter form would always be used where accuracy combined with ease of manipulation were desired. When specific gravities are given to four significant figures, correction to a vacuum is necessary, or the fourth figure has no meaning. No mention is made of this in the text, and the specific gravities given are termed densities, a misuse of the latter term which occurs much too often.

The second Section, on stearine, by J. McArthur, puts forth the main processes for the decomposition of fats in a very explicit form. The writer wisely confines the term "saponification" to decomposition by means of a base.

The account of the candle manufacture, by L. and F. A. Field, given in Section iii., is highly interesting, and will be read with profit by many who have no connection with such matters, as well as by specialists. Producers of gas may well believe that their product will be in increasing demand when the candle industry flourishes in spite of the introduction of later forms of lighting. Doubtless candles owe their present hold on the public favour largely to the great improvements in quality effected by recent advances in the methods of manufacture. How great these advances are may be gathered from even a rapid perusal of the pages before us.

The description, in Section iv., of the petroleum industry, by Boverton Redwood, is both graphic and complete. It forms the best monograph on the subject yet written. The origin of petroleum is so treated as to present the various theories put forward to account for its occurrence; necessarily, no authoritative decision can be given on this very debatable question. Concerning the occurrence of sulphur in the petroleums from Ohio and Canada, those interested would do well to supplement the bare mention of the fact here given by reference to the July number of the *Journal* of the Franklin Institute, where C. F. Mabery gives an account in which the subject is treated as its importance requires. Warren is stated by Mr. Redwood to have isolated hydrocarbons of the C_nH_{2n} series, termed naphthenes by Markownikoff. Mr. Mabery shows that the Ohio and Canadian petroleums do not yield the naphthenes of Markownikoff and Ogloblin, but give hydrocarbons of the C_nH_{2n+2} series of similar boiling points. This writer also proves conclusively the presence of benzene, toluene, and xylenes in these petroleums.

The manufacture of shale oil gives yet another instance of the application of continuous processes; the text contains very lucid descriptions of these, well and sufficiently illustrated. Few of the general public can have any adequate conception of the number and variety of lamps in existence for use with oils. An exhaustive account is given of these, and the advantages or disadvantages characteristic of the main types of oil-lamps are dwelt upon at sufficient length to enable an intelligent judgment to be formed as to the suitability of any particular lamp for the work required from it.

The Section on safety-lamps, with which this volume concludes, has been contributed by D. A. Louis, in conjunction with Boverton Redwood. It gives by no means the least interesting reading. Although the excellent account of the lamp-indication of fire-damp is highly technical, and calculated to be eminently useful to specialists, the general reader will find no difficulty in grasping the principles involved, and will much appreciate the clearness with which this important subject is treated.

It may be hoped that the high standard exhibited in this volume will be maintained in volume iii., announced as to appear shortly. The editors are certainly to be congratulated on the excellent production now before us.

W. T.

OUR BOOK SHELF.

Science Readers. By Vincent T. Murché. Book iv. Pp. 216. (London: Macmillan and Co., 1895.)

THE conversational method of instruction, which used to be so general in school books, is not one that leads to pleasant memories. Mr. Murché has created two boy prodigies in his "Science Readers," and they ask and answer questions of a teacher whose laudable ambition is to elicit and impart all kinds of scientific knowledge upon every suitable or unsuitable occasion. We reverence that teacher for his patience and for his ability to find texts in everything. The pity of it is, that lessons given in this way on all and sundry topics lack the quality which lies at the base of all true scientific knowledge, viz. the orderly arrangement of facts. A lesson on solids, liquids, and gases precedes one on our bodies, another on gravity precedes a lesson on vertebrates and invertebrates. A lesson on the classification of invertebrates is wedged between two on hydrostatic pressure, and so on throughout the book. Possibly the variety is introduced to charm the youthful mind, but it is not a desirable attribute of the book; for the method must result in the acquisition of unconnected information, and such knowledge has little to commend it. In the matter of illustration, and simplicity of language, the book leaves little to be desired.

A Garden of Pleasure. By E. V. B. Pp. 220. (London: Elliot Stock, 1895.)

A FEW chapters fresh with the fragrance of common country flowers, and breathing the life of "lustrous woodland." Here and there the authoress lapses into sentiment, but, taken as a whole, her language is attractive in its simplicity. The changes that go on in organic nature from month to month are drawn with careful touch, and many students of botany would derive benefit from the contemplation of the sketches.

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.*]

The "4026·5" Line and D_3.

MAY I call attention to the fact that the line at 4026·5, now recognised as belonging to the spectrum of helium, and conspicuous in the Orion stars, is also prominent in the spectrum of the solar chromosphere. Although not given in the catalogue of chromosphere lines (which dates from 1872), it was observed and published as long ago as 1883 (*Am. Jour. Sci. and Art.*, November 1883), in connection with another line at 4092, seen at the same time. Since then the 4026 line has been observed repeatedly, and might be given a place in the catalogue with a relative frequency of about 15, and a brightness of 3 or 4. Like the other helium lines it has no dark analogue in the ordinary solar spectrum. The 4092 line falls upon a strong double line shown upon Rowland's map, but I am not sure to which of the two components it belongs; it is faint, and seldom seen.

While D_3 rarely appears as a dark line upon the solar spectrum, yet in the course of over twenty years I am able to count up a considerable number of instances; certainly not less than twenty or thirty. The phenomenon occurs usually in the penumbral region of an active sun-spot, which in its nucleus reverses the lines of hydrogen, magnesium, and sodium, and sometimes D_3 itself. By a slight motion of the telescope as one passes away from the nucleus, it crosses regions where D_3 appears as a smoky shade: on page 130 of "The Sun" I have figured a typical case.

I have not yet been fortunate enough to see the duplicity of D_3 myself, but Prof. Reed has observed it on several occasions.

Hanover, N.H., August 26. C. A. YOUNG.

On the Temperature Variation of the Thermal Conductivity of Rocks.

NATURE reproduces the results obtained by Lord Kelvin, P.R.S., and J. R. Erskine Murray, a paper read at the Royal Society, May 30, "On the Temperature Variation of the Thermal Conductivity of Rocks." These gentlemen arrived at the following results: "(§ 13). . . . that for slate with lines of fluor parallel to cleavage planes, the mean conductivity in the range from 123° C. to 202° C. is 91 per cent. of the mean conductivity in the range from 50° C. to 123° C., and for granite the mean conductivity in the range from 145° C. to 214° C. is 88 per cent. of the mean conductivity in the range from 81° C. to 145° C."

These results are so widely different from those I obtained by another method, and which Lord Kelvin had the kindness to publish in NATURE, March 7, 1895, p. 439, that I must be allowed to introduce here a word of objection.

It seems to me that details of experimental dispositions are important enough, and should be trustworthy. It is however, not opportune to discuss them minutely now.

The experimenters based their work on the case of Fourier's "indefinite wall," which is characterised by the fact that temperature differences in the parallel planes are exactly proportional to the distances of these planes. According to the experiment, they get the result that this proportionality does not exist, and that conductibility varies much according to temperature.

In my opinion, this absence of proportionality arrived at, proves rather that the experimental conditions were defective, and are in contradiction with the hypothesis of the "indefinite wall" case.

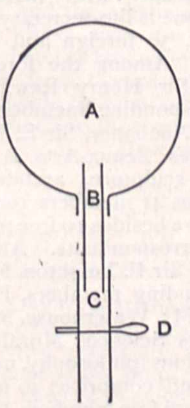
I admit, in principle, the employed method, but I think it should be modified until—for the same temperature of the bath, the said proportionality should be obtained; then, in a new experiment, the temperature of the bath being higher, it should be verified if the proportionality and the conductibility remain, or if the last increases or diminishes with the temperature.

Neuchâtel, August. ROBERT WEBER.

Experimental Mountain-building.

PROF. JOHANNES WALTHER, of Jena, requests me to communicate to you the following details regarding an interesting experiment which he has recently devised for teaching purposes: it is intended to explain mountain-formation.

He compares the system of folds on the surface of our slowly-ageing earth to the wrinkles which form on the skin of a drying apple, and points out that the height of our mountain-chains in relation to the mass of the globe is precisely comparable to the wrinkles on the skin of the apple. In order to demonstrate the formation of these folds, he takes an indiarubber balloon (A), and attaches to it a bit of glass tubing (B). On to this is stretched a piece of indiarubber tubing (C), which is pinched close by the stopcock (D). When the indiarubber balloon is blown out to its full capacity, it is spread over with a layer of flour-paste two millimetres thick, and is then dipped and twirled round and round in dry wheaten flour until a perfectly smooth crust, three to four millimetres in thickness, covers the whole sphere. The balloon is then placed on a tripod, so that the indiarubber tubing (C) dips exactly into a glass of water standing below. Thereupon the stopcock is turned open, and the air is allowed to escape in single bubbles; the volume of the ball is lessened, and lateral pressure makes itself immediately felt in the paste-crust. Small folds gradually grow bigger, single folds unite to form systems of folds, flat areas of depression sink deeper and deeper, and the neighbouring folds twirl and cross over the depression. The features of the Cordilleras, of the Jura, and many other well-known tectonic relations are thus reproduced with striking accuracy. Whenever it is desired to repeat the experiment, one need simply blow the balloon out again, smooth out the folded surface by dipping and twirling in dry flour, and all is ready for another demonstration.



London, August 26. L. BELINFANTE.

Joseph Thomson.

IN Mr. Gregory's sympathetic notice (NATURE, p. 440) of Joseph Thomson, he hardly does justice to the memory of the deceased traveller in relation to the scientific results of his expeditions; at least so far as botany is concerned. During his too short career Thomson presented three considerable collections of dried plants to Kew. The first, which appears to have been made on his own initiative, chiefly between Lake Nyassa and Lake Tanganyika, was secured for Kew in 1880, through the instrumentality of the late Colonel J. A. Grant, F.R.S. This was not the subject of a special paper; yet it contained a number of interesting novelties, some of which have from time to time been published in Hooker's "Icones Plantarum" and elsewhere. Before going out again Thomson carefully studied the means by which his collecting opportunities might be turned to the greatest advantage. Armed with this knowledge he collected even more successfully in the Kilimanjaro and other mountains of Eastern Equatorial Africa. This second collection reached Kew in September 1884, and proved of the greatest scientific importance, being the first adequate illustration of the mountain flora of that region. It contained scarcely 150 species; but the specimens were selected with admirable judgment, and were sufficient for all purposes. It was worked out by Sir Joseph D. Hooker and Prof. D. Oliver, and the very important results recorded in the twenty-first volume of the *Journal* of the Linnean Society. This paper and Thomson's collection will always rank among the classical documents for the study of the phytogeography of Central Africa. Subsequently Mr. Thomson sent to Kew the botanical fruits of his journey to the Atlas Mountains, and although they contained very few previously unknown plants, they were none the less instructive as a sample of the flora of that comparatively little-known part of the world. Had he preserved his health Thomson might have taken his place in the first rank of botanical explorers. He had acquired the rare gift of selection in collecting; of knowing what to secure and what to neglect.

W. BOTTING HEMSLEY.

Late Nestlings.

TO-DAY I observed nests of the house-martin underneath the eaves of the clock-tower at Lamlash Pier, on the south and west sides. The parents were busy feeding their young, whose cries I heard. Surely this is a late date for a migratory bird. How are these nestlings to get across the ocean? JAS. SHAW.

Barrhead, September 7.

THE INSTITUTE OF FRANCE.

IN a few weeks, at the end of October, the Institut National de France is to celebrate its first centenary. Some words concerning its origin and organisation may be of interest at the present moment.

The Institute is the outcome of a previous scientific society, entirely due to individual initiative. During the first half of the seventeenth century, a few men, between whom love of science was a firm bond, agreed to meet at regular intervals at the house of one of their number, informally, in order to exchange views, to keep each other posted up on their various researches, and to make up an unconventional assembly of congenial spirits. It was more of a temporary or intermittent club than a real society, as we understand the latter now. These men were mostly mathematicians and physicists—for at that time natural science was more in the *werden* than in the *sein* state—and Mersenne, Descartes, Blaise Pascal, Gassendi, are some of them. Their meetings soon attracted public attention, and the great Colbert, anxious for the development of the arts of peace after the Pyrenees treaty had put an end to the war, considered them as being of sufficient importance and utility to take an interest in them, and to support the incipient society officially.

Colbert even made out a full plan of what was to be realised 200 years later; what he organised was a body of scientific men who were to meet at regular intervals, and were divided into three classes—historical scholars, literary men, and, finally, scientific men. The private society of mathematicians and physicists grew into the Académie des Sciences, and each of the three academies met separately in the Bibliothèque du Roi, at Colbert's own residence. The king, as a sign of his approval, gave some money for experiments, and some pensions.

Among the members (no one knows how they were appointed) were Huyghens, Mariotte, Pecquet, Picard, Robertval. The Academy of Sciences, the Académie Française, and the Académie des Inscriptions et Médailles thus lived in harmony, each having its particular pursuits. The history of these academies would take too much space; it is enough to have shown how they originated. They lived on till the Revolution, when they were organised on a new basis, and the Institute came into existence. The whole constitution of France being altered, that of the academies had also to be changed.

The Institute was founded in 1795. Article 298 of the *Constitution du* 5 *Fructidor, an* iii. (August 22, 1795) gave it the mission of "registering discoveries, and perfecting arts and sciences," while later laws provided for the details of the scheme, that of the 3 *Brumaire, an* iv., *i.e.* October 25, 1795. According to this law, the Institut National—a new name applied to, practically, an old thing—was divided into three classes—scientific (10 sections); moral and political (6 sections); literary and artistic (8 sections). Bonaparte (3 *Pluviose, an* xi., January 23, 1803) altered this plan, and added a fourth class, so that the Institute comprised the class of mathematical and physical sciences, with 11 sections; that of French language and literature (no sections); that of ancient language and literature (no sections); and that of fine arts (5 sections). In 1816, upon the return of monarchy, the general plan was respected, but in 1832 a fifth class was added: that of moral and political sciences, which had disappeared in 1803. Those five classes still exist, under the names of Académie Française, Académie des Sciences, Académie des Beaux-Arts, Académie des Sciences Morales et Politiques, Académie des Inscriptions et Belles Lettres. They still dwell in the Palais des Quatre Nations on the Seine, where Bonaparte housed them in 1805.

At present, the Institute is a society of men of eminence, divided into five distinct sub-societies, or academies, each member being at the same time, and as a matter of course, member of this or that particular academy, and of the Institute as a whole. Each academy has its definite purpose, and meets each week on fixed and different days; the Institute, as a whole, meets once a year, in October.

As a whole, the Institute is regulated by a committee of delegates, elected by, and in, the five academies, while each academy has its own president and secretary.

Two points must be noticed in reference to the academies. The one is that the Académie de Medécine has nothing at all to do with the Institute; it is a separate society (of medical men only) quite distinct, without the slightest relationship to any of the above-mentioned academies, or to the Institute. The other is that there is no connection whatever between the Institute or academies which make up the Institute, and the title of Officier d'Académie. To be Officier d'Académie is to have received from the Department of Public Instruction a special decoration (of the *Palmes Académiques*) which is, theoretically at least, more specially destined to persons who serve the cause of education and instruction. The Officiers d'Académie are thousands in number; they have nothing at all to do with the Academies.

Now, as to the membership of the latter.

New members are always elected by the members of each academy. A man considers himself as eligible for such or such academy; all he has to do, when a vacancy occurs in the academy or in the section to which he should belong, considering his previous work, is to declare himself a candidate by a letter addressed to the president of the latter, and to prepare a pamphlet in which he gives the list of his scientific or literary titles, of his works, of his researches or discoveries, of the functions he has occupied, &c.; and this pamphlet he sends or carries to each of the members of the academy. It is customary for every candidate to pay a visit to each of the latter, and then he waits for the result; in the meantime canvassing, in order to secure this or that member's vote when things do not seem to run smoothly. A very amusing book might be written of the anecdotes which are current upon the devices suggested to the candidates by what is called the "green fever," *la fièvre verte*, the fever which takes hold of a man anxious to wear the green-laced uniform which the members of the Institute wear upon official occasions. But such a book could be published only after the death of the author and of those concerned. Generally speaking, however, the Académie des Sciences would contribute little to the making of this book. Each election must be approved by the President of the Republic, and is approved as a matter of course. Each member receives a small *indemnité* of £60 a year.

Each academy has a limited number of members, but in most academies there are different classes of membership. The Académie Française, for literary men, comprises 40 immortals all told, one of whom is perpetual (life) secretary. It has no associates nor corresponding members, and while the members have little or nothing to do as members, save the preparation of a dictionary, and examining works which compete for various prizes, it is the custom for each new member to deliver a very elaborate speech concerning his predecessor, and one of the members answers this *discours de réception* by a speech concerning the works of the new-comer.

The Académie des Inscriptions et Belles Lettres, for men who deal specially with history, comprises 40 members (of whom one is life-secretary), 10 free members, 8 foreign associates, 30 foreign and 20 national corresponding members. Among the foreign associates are Prof. Max Müller, Sir Henry Rawlinson, W. Stokes; among foreign corresponding members, Mr. R. S. Poole, Sir J. Evans, M. A. Neubauer, Sir E. M. Thompson.

The Académie des Beaux-Arts is divided into five sections (painting, sculpture, architecture, engraving, music), and comprises 41 members (one of whom is life-secretary). There are besides 10 free members, 10 foreign associates, and 50 correspondents. Among the associates are Sir J. E. Millais, Sir F. Leighton, Mr. Alma Tadema; among the corresponding members, Prof. H. Herkomer, Sir E. Burne-Jones, Mr. Waterhouse, Mr. R. W. Macbeth.

The Académie des Sciences Morales et Politiques is divided into five sections (philosophy, morals, law, political economy, history), and comprises 40 members (of whom one is life-secretary), 16 free members, 6 foreign associates, 48 corresponding members. Among the foreign associates are Right Hon. W. E. Gladstone and Mr. Henry Reeve; Mr. Robert Flint, Right Hon. J. Bryce, Sir Fredk. Pollock, Right Hon. G. J. Goschen, Bishop Stubbs, and Mr. Lecky are corresponding members.

Last, but by no means least, comes the Académie des Sciences, which certainly exerts the largest influence, and is the most highly considered in public opinion. Divided into eleven sections, it comprises 68 members (of whom two are life-secretaries), 10 free members, 8 foreign associates, 100 corresponding members. Lord Kelvin, Sir Joseph Lister, and Dr. E. Frankland are among the associates. The British Correspondants are as follows:—Mathematical sciences—geometry: Prof. J. J. Sylvester, Rev. Prof. Salmon; astronomy: Dr. J. R. Hind, Mr. Norman Lockyer, Dr. W. Huggins; geography and navigation: Sir George Henry Richards; general physics: Sir G. G. Stokes, Lord Rayleigh. Physical sciences—chemistry: Prof. A. W. Williamson, Sir Henry Roscoe, Prof. W. Ramsay; mineralogy: Dr. J. Prestwich, Sir A. Geikie; botany: Sir Joseph D. Hooker, Dr. Maxwell Masters; rural economy: Sir J. B. Lawes, Sir J. H. Gilbert; anatomy and zoology: Sir W. Flower;

medicine and surgery: Sir James Paget. Prof. Huxley was a corresponding member also.

Each academy has more or less money left to it in order to distribute prizes for different subject-matters; the Académie des Sciences and Académie Française are the richest. The Duke d'Aumale has agreed to leave the splendid residence of Chantilly, with the books and collections it contains, to the Institute, and this handsome gift is accompanied by a sum of money to help to keep the castle in good order. It is estimated that, all paid, the Institute will be 100,000 francs richer each year for this gift.

English corresponding members and associates will have a good opportunity of visiting the fine chateau of Chantilly, for on October 26 the Duke opens the doors to all members of the Institute, and bids them welcome. The celebration of the centenary, to which *all* members of each Academy, *all* corresponding members and associates in every country have been, or are being, invited, will last four days. The programme has been given in NATURE (August 8) in full. The only new feature I can introduce, is the programme of the afternoon performance at the Comédie Française, where the best actors of the best theatre in France will play *Les Horaces* (Corneille), *Les Femmes Savantes* (Molière), and recite a piece of poetry by Sully-Prudhomme. The railway fares will be reduced 50 per cent. for all foreigners invited.

All may be sure to receive a hearty welcome. If the Institut de France does not contain *all* our "best men" in the different departments of knowledge or art, it contains only men of recognised authority. They are men whose aims are noble, and their feelings can but be most cordial towards those whose aims are the same towards their fellow-workers, whatever language they speak, whatever country they come from, towards all whose work and character are high enough to have secured for them the highest recognition French science can award.

HENRY DE VARIGNY.

THE IPSWICH MEETING OF THE BRITISH ASSOCIATION.

COMING after the Oxford year, the meeting at Ipswich is in numbers a comparatively small one; but, from a scientific point of view, everything augurs well. The papers promise to be of more than usual interest, and are so numerous that most of the Sections will have to sit early and late in order to get through all the work before them.

We have previously referred at some length to the work proposed for Sections A, B, C, D, G, and H.

Section D is this year reserved entirely to zoology and animal physiology, under the presidency of Prof. W. A. Herdman.

Prof. A. C. Haddon will read a paper on the exploration of the isles of the Pacific. Dr. Bashford Deane, of New York, is to read two papers—one on an apparatus for catching oyster spat, the other on the ganoids of North America. Prof. McIntosh will open a discussion on British fisheries. A paper will be read by the Rev. T. R. R. Stebbing, on zoological nomenclature and publication. Special interest is likely to be taken in a paper by the President and Prof. Boyce on the subject of oysters and typhoid, by those who propose to join in the excursion to the Colne Oyster Fishery (Colchester), which has just been added to the programme for Wednesday. It is intended to make a large use of the lantern for illustrating papers in the Section.

The provisional programme in Section E (Geography) makes it evident that the Section is, as usual, to be a popular one. After the address of the President, Mr. H. J. Mackinder, an account will be given, by Mr. H. S. Cowper, of a journey over Tarhuna and Gharian in Tripoli; and Mr. J. Batalka-Reis will discuss how to consider and write the history of the discovery of the world. On Friday, the papers will be given by Mr. C. E. Borchgrevink, describing his voyage to the Antarctic Sea; by Mr. H. N. Dickson, on oceanographical research in the North Sea; by Mr. W. B. Blaikie, on the cosmosphere; and by Mr. John Dodd, on Formosa. On Monday, Mr. E. G. Ravenstein will present a report on the climate of tropical Africa; and there will be papers by Mr. G. F. Scott Elliot, on Ruwenzori and East Africa; by Captain S. L. Hinde, on the Congo State; by Mr. J. T. P. Keatly, on the port of the Upper Nile in relation to the highways of commerce; and by Mr. J. L. Myres, on the maps of Herodotus. On Tuesday, Mr. Weston will deal with the New Zealand Alps, and Mr. J. L. Myres with Asia Minor, whilst Mr. A. Trevor Battye will give an account of Kolguev.

In Section F (Economic Science and Statistics), over which Mr. L. L. Price presides, bimetallism appears early on the scene, the arrangement being to devote Friday morning to a monetary discussion, in which representatives of the Bimetallic League and of the Gold Standard Defence Association, and others, are expected to take part. Monday will be given up to a discussion on the state of agriculture, on which question Captain E. G. Pretyman, M.P., will read a paper from the landlord's point of view, and Mr. Herman Biddell one from the tenant's point of view. This discussion has unfortunately been fixed for the same day as the discussion on the relation of chemistry to agriculture in Section B, but it is hoped that by an arrangement of the hours the two discussions may not clash. Other contributions in Section F will be by Mr. H. W. Wolff, on land banks; Mr. H. Moore, on co-operation in agriculture; Mr. E. Cannan, on population; Mr. H. Higgs, on the climbing ratio; and Rev. Frome Wilkinson, on the State and the labourer.

In Section H (Anthropology), in which Prof. W. M. Flinders Petrie presides, ethnology is to play a prominent part. The Section will, therefore, feel all the more the absence of Mr. E. W. Brabrook, who is unable to come to Ipswich on account of the very sad bereavement he has so recently suffered. It has been arranged that the Section shall sit each morning till 12.30 or 1, and then reassemble at 2, on each day except Saturday for a lecture illustrated by the lantern.

Botany is sitting for the first time as a distinct Section (K), under the presidency of Mr. W. T. Thiselton-Dyer. Amongst the papers will be one on Sporangia by Prof. F. O. Bower. Dr. D. H. Scott will speak on fossil botany, with special reference to the researches of the late Prof. Williamson. A paper on fossil botany will also be read by Prof. Solms-Laubach, of Strasbourg. Prof. E. C. Hansen, of Copenhagen, promises a paper on the variation of yeast cells, and Mr. A. C. Seward one on the Wealden Flora. Amongst other foreign botanists attending the meeting is Dr. T. M. Treub, of Java. A special botanical excursion, not figuring as one of the regular excursions, is being arranged.

INAUGURAL ADDRESS BY SIR DOUGLAS GALTON, K.C.B., D.C.L., F.R.S., PRESIDENT.

MY first duty is to convey to you, Mr. Mayor, and to the inhabitants of Ipswich, the thanks of the British Association for your hospitable invitation to hold our sixty-fifth meeting in your ancient town, and thus to recall the agreeable memories of the similar favour which your predecessors conferred on the Association forty-four years ago.

In the next place I feel it my duty to say a few words on the great loss which science has recently sustained—the death of the Right Hon. Thomas Henry Huxley. It is unnecessary for me to enlarge, in the presence of so many to whom his personality was known, upon his charm in social and domestic life; but upon the debt which the Association owes to him for the assistance which he rendered in the promotion of science I cannot well be silent. Huxley was pre-eminently qualified to assist in sweeping away the obstruction by dogmatic authority, which in the early days of the Association fettered progress in certain branches of science. For, whilst he was an eminent leader in biological research, his intellectual power, his original and intrepid mind, his vigorous and masculine English, made him a writer who explained the deepest subject with transparent clearness. And as a speaker his lucid and forcible style was adorned with ample and effective illustration in the lecture-room; and his energy and wealth of argument in a more public arena largely helped to win the battle of evolution, and to secure for us the right to discuss questions of religion and science without fear and without favour.

It may, I think, interest you to learn that Huxley first made the acquaintance of Tyndall at the meeting of the Association held in this town in 1851.

About forty-six years ago I first began to attend the meetings of the British Association; and I was elected one of your general secretaries about twenty-five years ago.

It is not unfitting, therefore, that I should recall to your minds the conditions under which science was pursued at the formation of the Association, as well as the very remarkable position which the Association has occupied in relation to science in this country.

Between the end of the sixteenth century and the early part of the present century several societies had been created to develop various branches of science. Some of these societies were established in London, and others in important provincial centres.

In 1831, in the absence of railways, communication between different parts of the country was slow and difficult. Science was therefore localised; and in addition to the universities in England, Scotland, and Ireland, the towns of Birmingham, Manchester, Plymouth and York each maintained an important nucleus of scientific research.

ORIGIN OF THE BRITISH ASSOCIATION.

Under these social conditions the British Association was founded in September 1831.

The general idea of its formation was derived from a migratory society which had been previously formed in Germany; but whilst the German society met for the special occasion on which it was summoned, and then dissolved, the basis of the British Association was continuity.

The objects of the founders of the British Association were enunciated in their earliest rules to be:—

"To give a stronger impulse and a more systematic direction to scientific inquiry; to promote the intercourse of those who cultivated science in different parts of the British Empire with one another, and with foreign philosophers; to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress."

Thus the British Association for the Advancement of Science based its utility upon the opportunity it afforded for combination.

The first meeting of the Association was held at York with 353 members.

As an evidence of the want which the Association supplied, it may be mentioned that at the second meeting, which was held at Oxford, the number of members was 435. The third meeting, at Cambridge, numbered over 900 members, and at the meeting at Edinburgh in 1834 there were present 1298 members.

At its third meeting, which was held at Cambridge in 1833, the Association, through the influence it had already acquired, induced the Government to grant a sum of £500 for the reduction of the astronomical observations of Baily. And at the same meeting the General Committee commenced to appropriate to scientific research the surplus from the subscriptions of its members. The committees on each branch of science were desired "to select definite and important objects of science, which they may think most fit to be advanced by an application of the funds of the society, either in compensation for labour, or in defraying the expense of apparatus, or otherwise, stating their reasons for their selection, and, when they may think proper, designating individuals to undertake the desired investigations."

The several proposals were submitted to the Committee of Recommendations, whose approval was necessary before they could be passed by the General Committee. The regulations then laid down still guide the Association in the distribution of its grants. At that early meeting the Association was enabled to apply £600 to these objects.

I have always wondered at the foresight of the framers of the constitution of the British Association, the most remarkable feature of which is the lightness of the tie which holds it together. It is not bound by any complex central organisation. It consists of a federation of Sections, whose youth and energy are yearly renewed by a succession of presidents and vice-presidents, whilst in each Section some continuity of action is secured by the less movable secretaries.

The governing body is the General Committee, the members of which are selected for their scientific work; but their controlling power is tempered by the law that all changes of rules, or of constitution, should be submitted to, and receive the approval of, the Committee of Recommendations. This committee may be described as an ideal Second Chamber. It consists of the most experienced members of the Association.

The administration of the Association in the interval between annual meetings is carried on by the Council, an executive body, whose duty it is to complete the work of the annual meeting (*a*) by the publication of its proceedings; (*b*) by giving effect to resolutions passed by the General Committee; (*c*) it also appoints the Local Committee and organises the *personnel* of each Section for the next meeting.

I believe that one of the secrets of the long-continued success and vitality of the British Association lies in this purely democratic constitution, combined with the compulsory careful consideration which must be given to suggested organic changes.

The Association is now in the sixty-fifth year of its existence. In its origin it invited the philosophical societies dispersed throughout Great Britain to unite in a co-operative union.

Within recent years it has endeavoured to consolidate that union.

At the present time almost all important local scientific societies scattered throughout the country, some sixty-six in number, are in correspondence with the Association. Their delegates hold annual conferences at our meetings. The Association has thus extended the sphere of its action: it places the members of the local societies engaged in scientific work in relation with each other, and brings them into co-operation with members of the Association and with others engaged in original investigations, and the papers which the individual societies publish annually are catalogued in our Report. Thus by degrees a national catalogue will be formed of the scientific work of these societies.

The Association has, moreover, shown that its scope is coterminous with the British Empire by holding one of its annual meetings at Montreal, and we are likely soon to hold a meeting in Toronto.

CONDITION OF CERTAIN SCIENCES AT THE FORMATION OF THE BRITISH ASSOCIATION.

The Association, at its first meeting, began its work by initiating a series of reports upon the then condition of the several sciences.

A rapid glance at some of these reports will not only show the enormous strides which have been made since 1831 in the investigation of facts to elucidate the laws of nature, but it may afford a slight insight into the impediments offered to the progress of investigation by the mental condition of the community, which has been for so long satisfied to accept assumptions without undergoing the labour of testing their truth by ascertaining the real facts. This habit of mind may be illustrated by two instances selected from the early reports made to the Association. The first is afforded by the report made in 1832, by Mr. Lubbock, on "Tides."

This was a subject necessarily of importance to England as a dominant power at sea. But in England records of the tides had only recently been commenced at the dockyards of Woolwich, Sheerness, Portsmouth, and Plymouth, on the request of the Royal Society, and no information had been collected upon the tides on the coasts of Scotland and Ireland.

The British Association may feel pride in the fact that within three years of its inception, viz. by 1834, it had induced the Corporation of Liverpool to establish two tide gauges, and the

Government to undertake tidal observations at 500 stations on the coasts of Britain.

Another cognate instance is exemplified by a paper read at the second meeting, in 1832, upon the State of Naval Architecture in Great Britain. The author contrasts the extreme perfection of the carpentry of the internal fittings of the vessels with the remarkable deficiency of mathematical theory in the adjustment of the external form of vessels, and suggests the benefit of the application of refined analysis to the various practical problems which ought to interest shipbuilders—problems of capacity, of displacement, of stowage, of velocity, of pitching and rolling, of masting, of the effects of sails and of the resistance of fluids; and, moreover, suggests that large-scale experiments should be made by Government, to afford the necessary data for calculation.

Indeed, when we consider how completely the whole habit of mind of the populations of the Western world has been changed, since the beginning of the century, from willing acceptance of authority as a rule of life to a universal spirit of inquiry and experimental investigation, is it not probable that this rapid change has arisen from society having been stirred to its foundations by the causes and consequences of the French Revolution?

One of the earliest practical results of this awakening in France was the conviction that the basis of scientific research lay in the accuracy of the standards by which observations could be compared; and the following principles were laid down as a basis for their measurements of length, weight, and capacity: viz. (1) that the unit of linear measure applied to matter in its three forms of extension, viz. length, breadth, and thickness, should be the standard of measures of length, surface, and solidity; (2) that the cubic contents of the linear measure in decimetres of pure water at the temperature of its greatest density should furnish at once the standard weight and the measure of capacity.[1] The metric system did not come into full operation in France till 1840; and it is now adopted by all countries on the continent of Europe except Russia.

The standards of length which were accessible in Great Britain at the formation of the Association were the Parliamentary standard yard lodged in the Houses of Parliament (which was destroyed in 1834 in the fire which burned the Houses of Parliament); the Royal Astronomical Society's standard; and the 10-foot bar of the Ordnance Survey.

The first two were assumed to afford exact measurements at a given temperature. The Ordnance bar was formed of two bars on the principle of a compensating pendulum, and afforded measurements independent of temperature. Standard bars were also disseminated throughout the country, n possession of the corporations of various towns.

The British Association early recognised the importance of uniformity in the record of scientific facts, as well as the necessity for an easy method of comparing standards and for verifying differences between instruments and apparatus required by various observers pursuing similar lines of investigation. At its meeting at Edinburgh in 1834 it caused a comparison to be made between the standard bar at Aberdeen, constructed by Troughton, and the standard of the Royal Astronomical Society, and reported that the scale "was exceedingly well finished; it was about $\frac{1}{500}$th of an inch shorter than the 5-feet of the Royal Astronomical Society's scale, but it was evident that a great number of minute, yet important, circumstances have hitherto been neglected in the formation of such scales, without an attention to which they cannot be expected to accord with that degree of accuracy which the present state of science demands." Subsequently, at the meeting at Newcastle in 1863, the Association appointed a committee to report on the best means of providing for a uniformity of weights and measures with reference to the interests of science. This committee recommended the metric decimal system—a recommendation which has been endorsed by a committee of the House of Commons in the last session of last Parliament.

British instrument-makers had been long conspicuous for accuracy of workmanship. Indeed, in the eighteenth century practical astronomy had been mainly in the hands of British observers; for although the mathematicians of France and other countries on the continent of Europe were occupying the foremost place in mathematical investigation, means of astronomical observation had been furnished almost exclusively by English artisans.

The sectors, quadrants, and circles of Ramsden, Bird, and Cary were inimitable by continental workmen.

But the accuracy of the mathematical-instrument maker had not penetrated into the engineer's workshop. And the foundation of the British Association was coincident with a rapid development of mechanical appliances.

At that time a good workman had done well if the shaft he was turning, or the cylinder he was boring, "was right to the $\frac{1}{32}$nd of an inch." This was, in fact, a degree of accuracy as fine as the eye could usually distinguish.

Few mechanics had any distinct knowledge of the method to be pursued for obtaining accuracy; nor, indeed, had practical men sufficiently appreciated either the immense importance or the comparative facility of its acquisition.

The accuracy of workmanship essential to this development of mechanical progress required very precise measurements of length, to which reference could be easily made. No such standards were then available for the workshops. But a little before 1830 a young workman named Joseph Whitworth realised that the basis of accuracy in machinery was the making of a true plane. The idea occurred to him that this could only be secured by making three independent plane surfaces; if each of these would lift the other, they must be planes, and they must be true.

The true plane rendered possible a degree of accuracy beyond the wildest dreams of his contemporaries in the construction of the lathe and the planing machine, which are used in the manufacture of all tools.

His next step was to introduce an exact system of measurement, generally applicable in the workshop.

Whitworth felt that the eye was altogether inadequate to secure this, and appealed to the sense of touch for affording a means of comparison. If two plugs be made to fit into a round hole, they may differ in size by a quantity imperceptible to the eye, or to any ordinary process of measurement, but in fitting them into the hole the difference between the larger and the smaller is felt immediately by the greater ease with which the smaller one fits. In this way a child can tell which is the larger of two cylinders differing in thickness by no more than $\frac{1}{5000}$th of an inch.

Standard gauges, consisting of hollow cylinders with plugs to fit, but differing in diameter by the $\frac{1}{1000}$th or the $\frac{1}{10000}$th of an inch, were given to his workmen, with the result that a degree of accuracy inconceivable to the ordinary mind became the rule of the shop.

To render the construction of accurate gauges possible, Whitworth devised his measuring machine, in which the movement was effected by a screw; by this means the distance between two true planes might be measured to the one-millionth of an inch.

These advances in precision of measurement have enabled the degree of accuracy which was formerly limited to the mathematical-instrument maker to become the common property of every machine shop. And not only is the latest form of steam-engine, in the accuracy of its workmanship, little behind the chronometer of the early part of the century, but the accuracy in the construction of experimental apparatus which has thus been introduced has rendered possible recent advances in many lines of research.

Lord Kelvin said, in his Presidential Address at Edinburgh, "Nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient, long-continued labour in the sifting of numerical results." The discovery of argon, for which Lord Rayleigh and Prof. Ramsay have been awarded the Hodgkin prize by the Smithsonian Institution, affords a pregnant illustration of the truth of this remark. Indeed, the provision of accurate standards not only of length, but of weight, capacity, temperature, force, and energy, are amongst the foundations of scientific investigation.

In 1842, the British Association obtained the opportunity of extending its usefulness in this direction.

In that year the Government gave up the Royal Observatory at Kew, and offered it to the Royal Society, who declined it. But the British Association accepted the charge. Their first object was to continue Sabine's valuable observations upon the vibrations of a pendulum in various gases, and to promote pendulum observations in different parts of the world. They subsequently extended it into an observatory for comparing and verifying the various instruments which recent discoveries in physical science had suggested for continuous meteorological and magnetic observations, for observations and experiments on atmospheric electricity, and for the study of solar physics.

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[1] The litre is the volume of a kilogramme of pure water at its maximum density, and is slightly less than the litre was intended to be, viz. one cubic decimetre. The weight of a cubic decimetre of pure water is 1·000013 kilogrammes.

This new departure afforded a means for ascertaining the advantages and disadvantages of the several varieties of scientific instruments; as well as for standardising and testing instruments, not only for instrument-makers, but especially for observers by whom simultaneous observations were then being carried on in different parts of the world; and also for training observers proceeding abroad on scientific expeditions.

Its special object was to promote original research, and expenditure was not to be incurred on apparatus merely intended to exhibit the necessary consequences of known laws.

The rapid strides in electrical science had attracted attention to the measurement of electrical resistances, and in 1859 the British Association appointed a special committee to devise a standard. The standard of resistance proposed by that committee became the generally accepted standard, until the requirements of that advancing science led to the adoption of an international standard.

In 1866 the Meteorological Department of the Board of Trade entered into close relations with the Kew Observatory.

And in 1871 Mr. Gassiot transferred £10,000 upon trust to the Royal Society for the maintenance of the Kew Observatory, for the purpose of assisting in carrying on magnetical, meteorological, and other physical observations. The British Association thereupon, after having maintained this Observatory for nearly thirty years, at a total expenditure of about £12,000, handed the Observatory over to the Royal Society.

The *Transactions* of the British Association are a catalogue of its efforts in every branch of science, both to promote experimental research and to facilitate the application of the results to the practical uses of life.

But probably the marvellous development in science which has accompanied the life-history of the Association will be best appreciated by a brief allusion to the condition of some of the branches of science in 1831 as compared with their present state.

GEOLOGICAL AND GEOGRAPHICAL SCIENCE.

Geology.

At the foundation of the Association geology was assuming a prominent position in science. The main features of English geology had been illustrated as far back as 1821, and, among the founders of the British Association, Murchison and Phillips, Buckland, Sedgwick and Conybeare, Lyell and De la Beche, were occupied in investigating the data necessary for perfecting a geological chronology by the detailed observations of the various British deposits, and by their co-relation with the continental strata. They were thus preparing the way for those large generalisations which have raised geology to the rank of an inductive science.

In 1831 the Ordnance maps published for the southern counties had enabled the Government to recognise the importance of a geological survey by the appointment of Mr. De la Beche to affix geological colours to the maps of Devonshire and portions of Somerset, Dorset and Cornwall; and in 1835, Lyell, Buckland and Sedgwick induced the Government to establish the Geological Survey Department, not only for promoting geological science, but on account of its practical bearing on agriculture, mining, the making of roads, railways, and canals, and on other branches of national industry.

Geography.

The Ordnance Survey appears to have had its origin in a proposal of the French Government to make a joint-measurement of an arc of the meridian. This proposal fell through at the outbreak of the Revolution; but the measurement of the base for that object was taken as a foundation for a national survey. In 1831, however, the Ordnance Survey had only published the 1-inch map for the southern portion of England, and the great triangulation of the kingdom was still incomplete.

In 1834 the British Association urged upon the Government that the advancement of various branches of science was greatly retarded by the want of an accurate map of the whole of the British Isles; and that, consequently, the engineer and meteorologist, the agriculturist and the geologist, were each fettered in their scientific investigations by the absence of those accurate data which now lie ready to his hand for the measurement of length, of surface, and of altitude.

Yet the first decade of the British Association was coincident with a considerable development of geographical research. The Association was persistent in pressing on the Government the scientific importance of sending the expedition of Ross to the Antarctic and of Franklin to the Arctic regions. We may trust that we are approaching a solution of the geography of the North Pole; but the Antarctic regions still present a field for the researches of the meteorologist, the geologist, the biologist, and the magnetic observer, which the recent voyage of M. Borchgrevink leads us to hope may not long remain unexplored.

In the same decade the question of an alternative route to India by means of a communication between the Mediterranean and the Persian Gulf was also receiving attention, and in 1835 the Government employed Colonel Chesney to make a survey of the Euphrates valley in order to ascertain whether that river would enable a practicable route to be formed from Iskanderoon, or Tripoli, opposite Cyprus, to the Persian Gulf. His valuable surveys are not, however, on a sufficiently extensive scale to enable an opinion to be formed as to whether a navigable waterway through Asia Minor is physically practicable, or whether the cost of establishing it might not be prohibitive.

The advances of Russia in Central Asia have made it imperative to provide an easy, rapid, and alternative line of communication with our Eastern possessions, so as not to be dependent upon the Suez Canal in time of war. If a navigation cannot be established, a railway between the Mediterranean and the Persian Gulf has been shown by the recent investigations of Messrs. Hawkshaw and Hayter, following on those of others, to be perfectly practicable and easy of accomplishment; such an undertaking would not only be of strategical value, but it is believed it would be commercially remunerative.

Speke and Grant brought before the Association, at its meeting at Newcastle in 1863, their solution of the mystery of the Nile basin, which had puzzled geographers from the days of Herodotus; and the efforts of Livingstone and Stanley and others have opened out to us the interior of Africa. I cannot refrain here from expressing the deep regret which geologists and geographers, and indeed all who are interested in the progress of discovery, feel at the recent death of Joseph Thomson. His extensive, accurate, and trustworthy observations added much to our knowledge of Africa, and by his premature death we have lost one of its most competent explorers.

CHEMICAL, ASTRONOMICAL AND PHYSICAL SCIENCE.

Chemistry.

The report made to the Association on the state of the chemical sciences in 1832, says that the efforts of investigators were then being directed to determining with accuracy the true nature of the substances which compose the various products of the organic and inorganic kingdoms, and the exact ratios by weight which the different constituents of these substances bear to each other.

But since that day the science of chemistry has far extended its boundaries. The barrier has vanished which was supposed to separate the products of living organisms from the substances of which minerals consist, or which could be formed in the laboratory. The number of distinct carbon compounds obtainable from organisms has greatly increased; but it is small when compared with the number of such compounds which have been artificially formed. The methods of analysis have been perfected. The physical, and especially the optical, properties of the various forms of matter have been closely studied, and many fruitful generalisations have been made. The form in which these generalisations would now be stated may probably change, some, perhaps, by the overthrow or disuse of an ingenious guess at nature's workings, but more by that change which is the ordinary growth of science—namely, inclusion in some simpler and more general view.

In these advances the chemist has called the spectroscope to his aid. Indeed, the existence of the British Association has been practically coterminous with the comparatively newly developed science of spectrum analysis, for though Newton,[1] Wollaston, Fraunhofer, and Fox Talbot had worked at the subject long ago, it was not till Kirchhoff and Bunsen set a seal on the prior labours of Stokes, Ångström, and Balfour Stewart that the spectra of terrestrial elements have been mapped out and grouped; that by its help new elements have been discovered,

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[1] Joannes Marcus Marci, of Kronland in Bohemia, was the only predecessor of Newton who had any knowledge of the formation of a spectrum by a prism. He not only observed that the coloured rays diverged as they left the prism, but that a coloured ray did not change in colour after transmission through a prism. His book, *Thaumantias, liber de arcu cœlesti deque colorum apparentium natura*, Prag. 1648, was, however, not known to Newton, and had no influence upon future discoveries.

and that the idea has been suggested that the various orders of spectra of the same element are due to the existence of the element in different molecular forms—allotropic or otherwise—at different temperatures.

But great as have been the advances of terrestrial chemistry through its assistance, the most stupendous advance which we owe to the spectroscope lies in the celestial direction.

Astronomy.

In the earlier part of this century, whilst the sidereal universe was accessible to investigators, many problems outside the solar system seemed to be unapproachable.

At the third meeting of the Association, at Cambridge, in 1833, Dr. Whewell said that astronomy is not only the queen of science, but the only perfect science, which was "in so elevated a state of flourishing maturity that all that remained was to determine with the extreme of accuracy the consequences of its rules by the profoundest combinations of mathematics; the magnitude of its data by the minutest scrupulousness of observation."

But in the previous year, viz. 1832, Airy, in his report to the Association on the progress of astronomy, had pointed out that the observations of the planet Uranus could not be united in one elliptic orbit; a remark which turned the attention of Adams to the discovery of Neptune. In his report on the position of optical science in 1832, Brewster suggested that with the assistance of adequate instruments "it would be possible to study the action of the elements of material bodies upon rays of artificial light, and thereby to discover the analogies between their affinities and those which produce the fixed lines in the spectra of the stars; and thus to study the effects of the combustions which light up the suns of other systems."

This idea has now been realised. All the stars which shine brightly enough to impress an image of the spectrum upon a photographic plate have been classified on a chemical basis. The close connection between stars and nebulæ has been demonstrated; and while on the one hand the modern science of thermodynamics has shown that the hypothesis of Kant and Laplace on stellar formation is no longer tenable, inquiry has indicated that the true explanation of stellar evolution is to be found in the gradual condensation of meteoritic particles, thus justifying the suggestions put forward long ago by Lord Kelvin and Prof. Tait.

We now know that the spectra of many of the terrestrial elements in the chromosphere of the sun differ from those familiar to us in our laboratories. We begin to glean the fact that the chromospheric spectra are similar to those indicated by the absorption going on in the hottest stars, and Lockyer has not hesitated to affirm that these facts would indicate that in those localities we are in the presence of the actions of temperatures sufficiently high to break up our chemical elements into finer forms. Other students of these phenomena may not agree in this view, and possibly the discrepancies may be due to default in our terrestrial chemistry. Still, I would recall to you that Dr. Carpenter, in his Presidential Address at Brighton in 1872, almost censured the speculations of Frankland and Lockyer in 1868 for attributing a certain bright line in the spectrum of solar prominences (which was not identifiable with that of any known terrestrial source of light) to a hypothetical new substance which they proposed to call "helium," because "it had not received that verification which, in the case of Crookes' search for thallium, was afforded by the actual discovery of the new metal." Ramsay has now shown that this gas is present in dense minerals on earth; but we have now also learned from Lockyer that it and other associated gases are not only found with hydrogen in the solar chromosphere, but that these gases, with hydrogen, form a large percentage of the atmospheric constituents of some of the hottest stars in the heavens.

The spectroscope has also made us acquainted with the motions and even the velocities of those distant orbs which make up the sidereal universe. It has enabled us to determine that many stars, single to the eye, are really double, and many of the conditions of these strange systems have been revealed. The rate at which matter is moving in solar cyclones and winds is now familiar to us. And I may also add that quite recently this wonderful instrument has enabled Prof. Keeler to verify Clerk Maxwell's theory that the rings of Saturn consist of a marvellous company of separate moons—as it were, a cohort of courtiers revolving round their queen—with velocities proportioned to their distances from the planet.

Physics.

If we turn to the sciences which are included under physics, the progress has been equally marked.

In optical science, in 1831, the theory of emission as contrasted with the undulatory theory of light was still under discussion.

Young, who was the first to explain the phenomena due to the interference of the rays of light as a consequence of the theory of waves, and Fresnel, who showed the intensity of light for any relative position of the interference-waves, both had only recently passed away.

The investigations into the laws which regulate the conduction and radiation of heat, together with the doctrine of latent and of specific heat, and the relations of vapour to air, had all tended to the conception of a material heat, or caloric, communicated by an actual flow and emission.

It was not till 1834 that improved thermometrical appliances had enabled Forbes and Melloni to establish the polarisation of heat, and thus to lay the foundation of an undulatory theory for heat similar to that which was in progress of acceptation for light.

Whewell's report, in 1832, on magnetism and electricity shows that these branches of science were looked upon as cognate, and that the theory of two opposite electric fluids was generally accepted.

In magnetism, the investigations of Hansteen, Gauss, and Weber in Europe, and the observations made under the Imperial Academy of Russia over the vast extent of that Empire, had established the existence of magnetic poles, and had shown that magnetic disturbances were simultaneous at all the stations of observation.

At their third meeting the Association urged the Government to establish magnetic and meteorological observatories in Great Britain and her colonies and dependencies in different parts of the earth, furnished with proper instruments, constructed on uniform principles, and with provisions for continued observations at those places.

In 1839 the British Association had a large share in inducing the Government to initiate the valuable series of experiments for determining the intensity, the declination, the dip, and the periodical variations of the magnetic needle which were carried on for several years, at numerous selected stations over the surface of the globe, under the directions of Sabine and Lefroy.

In England systematic and regular observations are still made at Greenwich, Kew, and Stonyhurst. For some years past similar observations by both absolute and self-recording instruments have also been made at Falmouth—close to the home of Robert Were Fox, whose name is inseparably connected with the early history of terrestrial magnetism in this country—but under such great financial difficulties that the continuance of the work is seriously jeopardised. It is to be hoped that means may be forthcoming to carry it on. Cornishmen, indeed, could found no more fitting memorial of their distinguished countryman, John Couch Adams, than by suitably endowing the magnetic observatory in which he took so lively an interest.

Far more extended observation will be needed before we can hope to have an established theory as to the magnetism of the earth. We are without magnetic observations over a large part of the southern hemisphere. And Prof. Rücker's recent investigations tell us that the earth seems as it were alive with magnetic forces, be they due to electric currents or to variations in the state of magnetised matter; that the disturbances affect not only the diurnal movement of the magnet, but that even the small part of the secular change which has been observed, and which has taken centuries to accomplish, is interfered with by some slower agency. And, what is more important, he tells us that none of these observations stand as yet upon a firm basis, because standard instruments have not been in accord; and much labour, beyond the power of individual effort, has hitherto been required to ascertain whether the relations between them are constant or variable.

In electricity, in 1831, just at the time when the British Association was founded, Faraday's splendid researches in electricity and magnetism at the Royal Institution had begun with his discovery of magneto-electric induction, his investigation of the laws of electro-chemical decomposition, and of the mode of electrolytical action.

But the practical application of our electrical knowledge was then limited to the use of lightning-conductors for buildings and ships. Indeed, it may be said that the applications of electricity to the use of man have grown up side by side with the British Association.

One of the first practical applications of Faraday's discoveries was in the deposition of metals and electro-plating, which has developed into a large branch of national industry; and the dissociating effect of the electric arc, for the reduction of ores, and in other processes, is daily obtaining a wider extension.

But probably the application of electricity which is tending to produce the greatest change in our mental, and even material condition, is the electric telegraph and its sister, the telephone. By their agency not only do we learn, almost at the time of their occurrence, the events which are happening in distant parts of the world, but they are establishing a community of thought and feeling between all the nations of the world which is influencing their attitude towards each other, and, we may hope, may tend to weld them more and more into one family.

The electric telegraph was introduced experimentally in Germany in 1833, two years after the formation of the Association. It was made a commercial success by Cooke and Wheatstone in England, whose first attempts at telegraphy were made on the line from Euston to Camden Town in 1837, and on the line from Paddington to West Drayton in 1838.

The submarine telegraph to America, conceived in 1856, became a practical reality in 1861 through the commercial energy of Cyrus Field and Pender, aided by the mechanical skill of Latimer Clark, Gooch, and others, and the scientific genius of Lord Kelvin. The knowledge of electricity gained by means of its application to the telegraph largely assisted the extension of its utility in other directions.

The electric light gives, in its incandescent form, a very perfect hygienic light. Where rivers are at hand the electrical transmission of power will drive railway trains and factories economically, and might enable each artisan to convert his room into a workshop, and thus assist in restoring to the labouring man some of the individuality which the factory has tended to destroy.

In 1843 Joule described his experiments for determining the mechanical equivalent of heat. But it was not until the meeting at Oxford, in 1847, that he fully developed the law of the conservation of energy, which, in conjunction with Newton's law of the conservation of momentum, and Dalton's law of the conservation of chemical elements, constitutes a complete mechanical foundation for physical science.

Who, at the foundation of the Association, would have believed some far-seeing philosopher if he had foretold that the spectroscope would analyse the constituents of the sun and measure the motions of the stars; that we should liquefy air and utilise temperatures approaching to the absolute zero for experimental research; that, like the magician in the "Arabian Nights," we should annihilate distance by means of the electric telegraph and the telephone; that we should illuminate our largest buildings instantaneously, with the clearness of day, by means of the electric current; that by the electric transmission of power we should be able to utilise the Falls of Niagara to work factories at distant places; that we should extract metals from the crust of the earth by the same electrical agency to which, in some cases, their deposition has been attributed?

These discoveries and their applications have been brought to their present condition by the researches of a long line of scientific explorers, such as Dalton, Joule, Maxwell, Helmholtz, Herz, Kelvin, and Rayleigh, aided by vast strides made in mechanical skill. But what will our successors be discussing sixty years hence? How little do we yet know of the vibrations which communicate light and heat! Far as we have advanced in the application of electricity to the uses of life, we know but little even yet of its real nature. We are only on the threshold of the knowledge of molecular action, or of the constitution of the all-pervading æther. Newton, at the end of the seventeenth century, in his preface to the "Principia," says: "I have deduced the motions of the planets by mathematical reasoning from forces; and I would that we could derive the other phenomena of nature from mechanical principles by the same mode of reasoning. For many things move me, so that I somewhat suspect that all such may depend on certain forces by which the particles of bodies, through causes not yet known, are either urged towards each other according to regular figures, or are repelled and recede from each other; and these forces being unknown, philosophers have hitherto made their attempts on nature in vain."

In 1848 Faraday remarked: "How rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develop more and more their importance! "A few years ago magnetism was an occult force, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relation with electricity, heat, chemical action, light, crystallisation; and through it the forces concerned in cohesion. We may feel encouraged to continuous labours, hoping to bring it into a bond of union with gravity itself."

But it is only within the last few years that we have begun to realise that electricity is closely connected with the vibrations which cause heat and light, and which seem to pervade all space —vibrations which may be termed the voice of the Creator calling to each atom and to each cell of protoplasm to fall into its ordained position, each, as it were, a musical note in the harmonious symphony which we call the universe.

Meteorology.

At the first meeting, in 1831, Prof. James D. Forbes was requested to draw up a report on the State of Meteorological Science, on the ground that this science is more in want than any other of that systematic direction which it is one great object of the Association to give.

Prof. Forbes made his first report in 1832, and a subsequent report in 1840. The systematic records now kept in various parts of the world of barometric pressure, of solar heat, of the temperature and physical conditions of the atmosphere at various altitudes, of the heat of the ground at various depths, of the rainfall, of the prevalence of winds, and the gradual elucidation not only of the laws which regulate the movements of cyclones and storms, but of the influences which are exercised by the sun and by electricity and magnetism, not only upon atmospheric conditions, but upon health and vitality, are gradually approximating meteorology to the position of an exact science.

England took the lead in rainfall observations. Mr. G. J. Symons organised the British Rainfall System in 1860 with 178 observers, a system which until 1876 received the help of the British Association. Now Mr. Symons himself conducts it, assisted by more than 3000 observers, and these volunteers not only make the observations, but defray the expense of their reduction and publication. In foreign countries this work is done by Government officers at the public cost.

At the present time a very large number of rain gauges are in daily use throughout the world. The British Islands have more than 3000, and India and the United States have nearly as many; France and Germany are not far behind; Australia probably has more—indeed, one colony alone, New South Wales, has more than 1100.

The storm warnings now issued under the excellent systematic organisation of the Meteorological Committee may be said to have had their origin in the terrible storm which broke over the Black Sea during the Crimean War, on November 27, 1855. Leverrier traced the progress of that storm, and seeing how its path could have been reported in advance by the electric telegraph, he proposed to establish observing stations which should report to the coasts the probability of the occurrence of a storm. Leverrier communicated with Airy, and the Government authorised Admiral FitzRoy to make tentative arrangements in this country. The idea was also adopted on the continent, and now there are few civilised countries north or south of the equator without a system of storm warning.[1]

BIOLOGICAL SCIENCE.

Botany.

The earliest Reports of the Association which bear on the biological sciences were those relating to botany.

In 1831 the controversy was yet unsettled between the advantages of the Linnean, or Artificial system, as contrasted with the Natural system of classification. Histology, morphology, and physiological botany, even if born, were in their early infancy.

Our records show that von Mohl noted cell division in 1835, the presence of chlorophyll corpuscles in 1837; and he first descriped protoplasm in 1846.

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[1] It has often been supposed that Leverrier was also the first to issue a daily weather map, but that was not the case, for in the Great Exhibition of 1851 the Electric Telegraph Company sold daily weather maps, copies of which are still in existence, and the data for them were, it is believed, obtained by Mr. James Glaisher, F.R.S., at that time Superintendent of the Meteorological Department at Greenwich.

Vast as have been the advances of physiological botany since that time, much of its fundamental principles remain to be worked out, and I trust that the establishment, for the first time, of a permanent Section for botany at the present meeting will lead the Association to take a more prominent part than it has hitherto done in the further development of this branch of biological science.

Animal Physiology.

In 1831 Cuvier, who during the previous generation had, by the collation of facts followed by careful inductive reasoning, established the plan on which each animal is constructed, was approaching the termination of his long and useful life. He died in 1832; but in 1831 Richard Owen was just commencing his anatomical investigations and his brilliant contributions to palæontology.

The impulse which their labours gave to biological science was reflected in numerous reports and communications, by Owen and others, throughout the early decades of the British Association, until Darwin propounded a theory of evolution which commanded the general assent of the scientific world. For this theory was not absolutely new. But just as Cuvier had shown that each bone in the fabric of an animal affords a clue to the shape and structure of the animal, so Darwin brought harmony into scattered facts, and led us to perceive that the moulding hand of the Creator may have evolved the complicated structures of the organic world from one or more primeval cells.

Richard Owen did not accept Darwin's theory of evolution, and a large section of the public contested it. I well remember the storm it produced—a storm of praise by my geological colleagues, who accepted the result of investigated facts; a storm of indignation such as that which would have burned Galileo at the stake from those who were not yet prepared to question the old authorities; but they diminish daily.

We are, however, as yet only on the threshold of the doctrine of evolution. Does not each investigation, even into the embryonic stage of the simpler forms of life, suggest fresh problems?

Anthropology.

The impulse given by Darwin has been fruitful in leading others to consider whether the same principle of evolution may not have governed the moral as well as the material progress of the human race. Mr. Kidd tells us that nature as interpreted by the struggle for life contains no sanction for the moral progress of the individual, and points out that if each of us were allowed by the conditions of life to follow his own inclination the average of each generation would distinctly deteriorate from that of the preceding one; but because the law of life is ceaseless and inevitable struggle and competition, ceaseless and inevitable selection and rejection, the result is necessarily ceaseless and inevitable progress. Evolution, as Sir William Flower said, is the message which biology has sent to help us on with some of the problems of human life, and Francis Galton urges that man, the foremost outcome of the awful mystery of evolution, should realise that he has the power of shaping the course of future humanity by using his intelligence to discover and expedite the changes which are necessary to adapt circumstances to man, and man to circumstances.

In considering the evolution of the human race, the science of preventive medicine may afford us some indication of the direction in which to seek for social improvement. One of the early steps towards establishing that science upon a secure basis was taken in 1835 by the British Association, who urged upon the Government the necessity of establishing registers of mortality showing the causes of death "on one uniform plan in all parts of the King's dominions, as the only means by which general laws touching the influence of causes of disease and death could be satisfactorily deduced." The general registration of births and deaths was commenced in 1838. But a mere record of death and its proximate cause is insufficient. Preventive medicine requires a knowledge of the details of the previous conditions of life and of occupation. Moreover, death is not our only or most dangerous enemy, and the main object of preventive medicine is to ward off disease. Disease of body lowers our useful energy. Disease of body or of mind may stamp its curse on succeeding generations.

The anthropometric laboratory affords to the student of anthropology a means of analysing the causes of weakness, not only in bodily, but also in mental life.

Mental actions are indicated by movements and their results. Such signs are capable of record, and modern physiology has shown that bodily movements correspond to action in nerve-centres, as surely as the motions of the telegraph-indicator express the movements of the operator's hands in the distant office.

Thus there is a relation between a defective status in brain power and defects in the proportioning of the body. Defects in physiognomical details, too finely graded to be measured with instruments, may be appreciated with accuracy by the senses of the observer; and the records show that these defects are, in a large degree, associated with a brain status lower than the average in mental power.

A report presented by one of your committees gives the results of observations made on 100,000 school-children examined individually in order to determine their mental and physical condition for the purpose of classification. This shows that about 16 per 1000 of the elementary school population appear to be so far defective in their bodily or brain condition as to need special training to enable them to undertake the duties of life, and to keep them from pauperism or crime.

Many of our feeble-minded children, and much disease and vice, are the outcome of inherited proclivities. Francis Galton has shown us that types of criminals which have been bred true to their kind are one of the saddest disfigurements of modern civilisation; and he says that few deserve better of their country than those who determine to lead celibate lives through a reasonable conviction that their issue would probably be less fitted than the generality to play their part as citizens.

These considerations point to the importance of preventing those suffering from transmissible disease, or the criminal, or the lunatic, from adding fresh sufferers to the teeming misery in our large towns. And in any case, knowing as we do the influence of environment on the development of individuals, they point to the necessity of removing those who are born with feeble minds, or under conditions of moral danger, from surrounding deteriorating influences.

These are problems which materially affect the progress of the human race, and we may feel sure that, as we gradually approach their solution, we shall more certainly realise that the theory of evolution, which the genius of Darwin impressed on this century, is but the first step on a biological ladder which may possibly eventually lead us to understand how in the drama of creation man has been evolved as the highest work of the Creator.

Bacteriology.

The sciences of medicine and surgery were largely represented in the earlier meetings of the Association, before the creation of the British Medical Association afforded a field for their more intimate discussion. The close connection between the different branches of science is causing a revival in our proceedings of discussions on some of the highest medical problems, especially those relating to the spread of infectious and epidemic disease.

It is interesting to contrast the opinion prevalent at the foundation of the Association with the present position of the question.

A report to the Association in 1834, by Prof. Henry, on contagion, says:—

"The notion that contagious emanations are at all connected with the diffusion of animalculæ through the atmosphere is at variance with all that is known of the diffusion of volatile contagion."

Whilst it had long been known that filthy conditions in air, earth and water fostered fever, cholera, and many other forms of disease, and that the disease ceased to spread on the removal of these conditions, yet the reason for their propagation or diminution remained under a veil.

Leeuwenhoek in 1680 described the yeast-cells, but Schwann in 1837 first showed clearly that fermentation was due to the activity of the yeast-cells; and, although vague ideas of fermentation had been current during the past century, he laid the foundation of our exact knowledge of the nature of the action of ferments, both organised and unorganised. It was not until 1860, after the prize of the Academy of Sciences had been awarded to Pasteur for his essay against the theory of spontaneous generation, that his investigations into the action of ferments[1] enabled him to show that the effects of the yeast-cell

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[1] In speaking of ferments one must bear in mind that there are two classes of ferments: one, living beings, such as yeast—"organised" ferments, as they are sometimes called—the other the products of living beings themselves, such as pepsin, &c.—"unorganised" ferments. Pasteur worked with the former, very little with the latter

are indissolubly bound up with the activities of the cell as a living organism, and that certain diseases, at least, are due to the action of ferments in the living being. In 1865 he showed that the disease of silkworms, which was then undermining the silk industry in France, could be successfully combated. His further researches into anthrax, fowl cholera, swine fever, rabies, and other diseases, proved the theory that those diseases are connected in some way with the introduction of a microbe into the body of an animal; that the virulence of the poison can be diminished by cultivating the microbes in an appropriate manner; and that when the virulence has been thus diminished their inoculation will afford a protection against the disease.

Meanwhile it had often been observed in hospital practice that a patient with a simple-fractured limb was easily cured, whilst a patient with a compound fracture often died from the wound. Lister was thence led, in 1865, to adopt his antiseptic treatment, by which the wound is protected from hostile microbes.

This investigation, followed by the discovery of the existence of a multitude of micro-organisms and the recognition of some of them—such as the bacillus of tubercle and the comma bacillus of cholera—as essential factors of disease; and by the elaboration of Koch and others of methods by which the several organisms might be isolated, cultivated, and their histories studied, have gradually built up the science of bacteriology. Amongst later developments are the discovery of various so-called antitoxins, such as those of diphtheria and tetanus, and the utilisation of these for the cure of disease. Lister's treatment formed a landmark in the science of surgery, and enabled our surgeons to perform operations never before dreamed of; whilst later discoveries are tending to place the practice of medicine on a firm scientific basis. And the science of bacteriology is leading us to recur to stringent rules for the isolation of infectious disease, and to the disinfection (by superheated steam) of materials which have been in contact with the sufferer.

These microbes, whether friendly or hostile, are all capable of multiplying at an enormous rate under favourable conditions. They are found in the air, in water, in the soil; but, fortunately, the presence of one species appears to be detrimental to other species, and sunshine, or even light from the sky, is prejudicial to most of them. Our bodies, when in health, appear to be furnished with special means of resisting attack, and, so far as regards their influence in causing disease, the success of the attack of a pathogenic organism upon an individual depends, as a rule, in part at least, upon the power of resistance of the individual.

But notwithstanding our knowledge of the danger arising from a state of low health in individuals, and of the universal prevalence of these micro-organisms, how careless we are in guarding the health conditions of every-day life! We have ascertained that pathogenic organisms pervade the air. Why, therefore, do we allow our meat, our fish, our vegetables, our easily contaminated milk, to be exposed to their inroads, often in the foulest localities? We have ascertained that they pervade the water we drink, yet we allow foul water from our dwellings, our pigsties, our farmyards, to pass into ditches without previous clarification, whence it flows into our streams and pollutes our rivers. We know the conditions of occupation which foster ill-health. Why, whilst we remove outside sources of impure air, do we permit the occupation of foul and unhealthy dwellings?

The study of bacteriology has shown us that although some of these organisms may be the accompaniments of disease, yet we owe it to the operation of others that the refuse caused by the cessation of animal and vegetable life is reconverted into food for fresh generations of plants and animals.

These considerations have formed a point of meeting where the biologist, the chemist, the physicist, and the statistician unite with the sanitary engineer in the application of the science of preventive medicine.

ENGINEERING.

Sewage Purification.

The early reports to the Association show that the laws of hydrostatics, hydrodynamics, and hydraulics necessary to the supply and removal of water through pipes and conduits had long been investigated by the mathematician. But the modern sanitary engineer has been driven by the needs of an increasing population to call in the chemist and the biologist to help him to provide pure water and pure air.

The purification and the utilisation of sewage occupied the attention of the British Association as early as 1864, and between 1869 and 1876 a committee of the Association made a series of valuable reports on the subject. The direct application of sewage to land, though effective as a means of purification, entailed difficulties in thickly settled districts, owing to the extent of land required.

The chemical treatment of sewage produced an effluent harmless only after having been passed over land, or if turned into a large and rapid stream, or into a tidal estuary; and it left behind a large amount of sludge to be dealt with.

Hence it was long contended that the simplest plan in favourable localities was to turn the sewage into the sea, and that the consequent loss to the land of the manurial value in the sewage would be recouped by the increase in fish-life.

It was not till the chemist called to his aid the biologist, and came to the help of the engineer, that a scientific system of sewage purification was evolved.

Dr. Frankland many years ago suggested the intermittent filtration of sewage; and Mr. Baldwin Latham was one of the first engineers to adopt it. But the valuable experiments made in recent years by the State Board of Health in Massachusetts have more clearly explained to us how by this system we may utilise micro-organisms to convert organic impurity in sewage into food fitted for higher forms of life.

To effect this we require, in the first place, a filter about five feet thick of sand and gravel, or, indeed, of any material which affords numerous surfaces or open pores. Secondly, that after a volume of sewage has passed through the filter, an interval of time be allowed, in which the air necessary to support the life of the micro-organisms is enabled to enter the pores of the filter. Thus this system is dependent upon oxygen and time. Under such conditions the organisms necessary for purification are sure to establish themselves in the filter before it has been long in use. Temperature is a secondary consideration.

Imperfect purification can invariably be traced either to a lack of oxygen in the pores of the filter, or to the sewage passing through so quickly that there is not sufficient time for the necessary processes to take place. And the power of any material to purify either sewage or water depends almost entirely upon its ability to hold a sufficient proportion of either sewage or water in contact with a proper amount of air.

Smoke Abatement.

Whilst the sanitary engineer has done much to improve the surface conditions of our towns, to furnish clean water, and to remove our sewage, he has as yet done little to purify town air. Fog is caused by the floating particles of matter in the air becoming weighted with aqueous vapour; some particles, such as salts of ammonia or chloride of sodium, have a greater affinity for moisture than others. You will suffer from fog so long as you keep refuse stored in your towns to furnish ammonia, or so long as you allow your street surfaces to supply dust, of which much consists of powdered horse manure, or so long as you send the products of combustion into the atmosphere. Therefore, when you have adopted mechanical traction for vehicles in your towns, you may largely reduce one cause of fog. And if you diminish your black smoke, you will diminish black fogs.

In manufactories you may prevent smoke either by care in firing, by using smokeless coal, or by washing the soot out of the products of consumption in its passage along the flue leading to the main chimney-shaft.

The black smoke from your kitchen may be avoided by the use of coke or of gas. But so long as we retain the hygienic arrangement of the open fire in our living-rooms I despair of finding a fireplace, however well constructed, which will not be used in such a manner as to cause smoke, unless, indeed, the chimneys were reversed and the fumes drawn into some central shaft, where they might be washed before being passed into the atmosphere.

Electricity as a warming and cooking agent would be convenient, cleanly, and economical when generated by water power, or possibly wind power, but it is at present too dear when it has to be generated by means of coal. I can conceive, however, that our descendants may learn so to utilise electricity that they in some future century may be enabled by its means to avoid the smoke in their towns.

Mechanical Engineering.

In other branches of civil and mechanical engineering, the reports in 1831 and 1832 on the state of this science show that the theoretical and practical knowledge of the strength of timber had obtained considerable development. But in 1830, before the introduction of railways, cast iron had been sparingly used in arched bridges for spans of from 160 to 200 feet, and wrought iron had only been applied to large-span iron bridges on the suspension principle, the most notable instance of which was the Menai Suspension Bridge, by Telford. Indeed, whilst the strength of timber had been patiently investigated by engineers, the best form for the use of iron girders and struts was only beginning to attract attention, and the earlier volumes of our *Transactions* contained numerous records of the researches of Eaton Hodgkinson, Barlow, Rennie, and others. It was not until twenty years later that Robert Stephenson and William Fairbairn erected the tubular bridge at Menai, followed by the more scientific bridge erected by Brunel at Saltash. These have now been entirely eclipsed by the skill with which the estuary of the Forth has been bridged with a span of 1700 feet by Sir John Fowler and Sir Benjamin Baker.

The development of the iron industry is due to the association of the chemist with the engineer. The introduction of the hot blast by Neilson, in 1829, in the manufacture of cast iron had effected a large saving of fuel. But the chemical conditions which affect the strength and other qualities of iron, and its combinations with carbon, silicon, phosphorus, and other substances, had at that time scarcely been investigated.

In 1856 Bessemer brought before the British Association at Cheltenham his brilliant discovery for making steel direct from the blast furnace, by which he dispensed with the laborious process of first removing the carbon from pig-iron by puddling, and then adding by cementation the required proportion of carbon to make steel. This discovery, followed by Siemens's regenerative furnace, by Whitworth's compressed steel, and by the use of alloys and by other improvements too numerous to mention here, have revolutionised the conditions under which metals are applied to engineering purposes.

Indeed, few questions are of greater interest, or possess more industrial importance, than those connected with metallic alloys. This is especially true of those alloys which contain the rarer metals; and the extraordinary effects of small quantities of chromium, nickel, tungsten and titanium on certain varieties of steel have exerted profound influence on the manufacture of projectiles and on the construction of our armoured ships.

Of late years, investigations on the properties and structure of alloys have been numerous, and among the more noteworthy researches may be mentioned those of Dewar and Fleming on the distinctive behaviour, as regards the thermo-electric powers and electrical resistance, of metals and alloys at the very low temperatures which may be obtained by the use of liquid air.

Prof. Roberts-Austen, on the other hand, has carefully studied the behaviour of alloys at very high temperatures, and by employing his delicate pyrometer has obtained photographic curves which afford additional evidence as to the existence of allotropic modifications of metals, and which have materially strengthened the view that alloys are closely analogous to saline solutions. In this connection it may be stated that the very accurate work of Heycock and Neville on the lowering of the solidifying points of molten metals, which is caused by the presence of other metals, affords a valuable contribution to our knowledge.

Prof. Roberts-Austen has, moreover, shown that the effect of any one constituent of an alloy upon the properties of the principal metal has a direct relation to the atomic volumes, and that it is consequently possible to foretell, in a great measure, the effect of any given combination.

A new branch of investigation, which deals with the microstructure of metals and alloys, is rapidly assuming much importance. It was instituted by Sorby in a communication which he made to the British Association in 1864, and its development is due to many patient workers, among whom M. Osmond occupies a prominent place.

Metallurgical science has brought aluminium into use by cheapening the process of its extraction; and if by means of the wasted forces in our rivers, or possibly of the wind, the extraction be still further cheapened by the aid of electricity, we may not only utilise the metal or its alloys in increasing the spans of our bridges, and in affording strength and lightness in the construction of our ships, but we may hope to obtain a material which may render practicable the dreams of Icarus and of Maxim, and for purposes of rapid transit enable us to navigate the air.

Long before 1831 the steam-engine had been largely used on rivers and lakes, and for short sea passages, although the first Atlantic steam-service was not established till 1838.

As early as 1820 the steam-engine had been applied by Gurney, Hancock, and others to road traction. The absurd impediments placed in their way by road trustees, which, indeed, are still enforced, checked any progress. But the question of mechanical traction on ordinary roads was practically shelved in 1830, at the time of the formation of the British Association, when the locomotive engine was combined with a tubular boiler and an iron road on the Liverpool and Manchester Railway.

Great, however, as was the advance made by the locomotive engine of Robert Stephenson, these earlier engines were only toys compared with the compound engines of to-day which are used for railways, for ships, or for the manufacture of electricity. Indeed, it may be said that the study of the laws of heat, which have led to the introduction of various forms of motive power, are gradually revolutionising all our habits of life.

The improvements in the production of iron, combined with the developed steam-engine, have completely altered the conditions of our commercial intercourse on land; whilst the changes caused by the effects of these improvements in shipbuilding, and on the ocean carrying trade, have been, if anything, still more marked.

At the foundation of the Association all ocean ships were built by hand, of wood, propelled by sails and manœuvred by manual labour; the material limited their length, which did not often exceed 100 feet, and the number of English ships of over 500 tons burden was comparatively small.

In the modern ships steam power takes the place of manual labour. It rolls the plates of which the ship is constructed, bends them to the required shape, cuts, drills, and rivets them in their place. It weighs the anchor; it propels the ship in spite of winds or currents; it steers, ventilates, and lights the ship when on the ocean. It takes the cargo on board and discharges it on arrival.

The use of iron favours the construction of ships of a large size, of forms which afford small resistance to the water, and with compartments which make the ships practically unsinkable in heavy seas, or by collision. Their size, the economy with which they are propelled, and the certainty of their arrival, cheapens the cost of transport.

The steam-engine, by compressing air, gives us control over the temperature of cool chambers. In these not only fresh meat, but the delicate produce of the Antipodes, is brought across the ocean to our doors without deterioration.

Whilst railways have done much to alter the social conditions of each individual nation, the application of iron and steam to our ships is revolutionising the international commercial conditions of the world; and it is gradually changing the course of our agriculture, as well as of our domestic life.

But great as have been the developments of science in promoting the commerce of the world, science is asserting its supremacy even to a greater extent in every department of war. And perhaps this application of science affords at a glance, better than almost any other, a convenient illustration of the assistance which the chemical, physical, and electrical sciences are affording to the engineer.

The reception of warlike stores is not now left to the uncertain judgment of "practical men," but is confided to officers who have received a special training in chemical analysis, and in the application of physical and electrical science to the tests by which the qualities of explosives, of guns, and of projectiles can be ascertained.

For instance, take explosives. Till quite recently black and brown powders alone were used, the former as old as civilisation, the latter but a small modern improvement adapted to the increased size of guns. But now the whole family of nitro-explosives are rapidly superseding the old powder. These are the direct outcome of chemical knowledge; they are not mere chance inventions, for every improvement is based on chemical theories, and not on random experiment.

The construction of guns is no longer a haphazard operation. In spite of the enormous forces to be controlled and the sudden violence of their action, the researches of the mathematician have enabled the just proportions to be determined with accuracy; the labours of the physicist have revealed the internal conditions of the materials employed, and the best means of their favourable employment. Take, for example, Longridge's coiled-wire system, in which each successive layer of which the gun is formed receives the exact proportion of tension which enables all the layers to act in unison. The chemist has rendered it clear that even the smallest quantities of certain ingredients are of supreme importance in affecting the tenacity and trustworthiness of the materials.

The treatment of steel to adapt it to the vast range of duties it has to perform is thus the outcome of patient research. And the use of the metals—manganese, chromium, nickel, molybdenum—as alloys with iron has resulted in the production of steels possessing varied and extraordinary properties. The steel required to resist the conjugate stresses developed, lightning fashion, in a gun necessitates qualities that would not be suitable in the projectile which that gun hurls with a velocity of some 2500 feet per second against the armoured side of a ship. The armour, again, has to combine extreme superficial hardness with great toughness, and during the last few years these qualities are sought to be attained by the application of the cementation process for adding carbon to one face of the plate, and hardening that face alone by rapid refrigeration.

The introduction of quick-firing guns from ·303 (*i.e.* about one-third) of an inch to 6-inch calibre has rendered necessary the production of metal cartridge-cases of complex forms drawn cold out of solid blocks or plate of the material; this again has taxed the ingenuity of the mechanic in the device of machinery, and of the metallurgist producing a metal possessed of the necessary ductility and toughness. The cases have to stand a pressure at the moment of firing of as much as twenty-five tons to the square inch—a pressure which exceeds the ordinary elastic limits of the steel of which the gun itself is composed.

There is nothing more wonderful in practical mechanics than the closing of the breech openings of guns, for not only must they be gas-tight at these tremendous pressures, but the mechanism must be such that one man by a single continuous movement shall be able to open or close the breech of the largest gun in some ten or fifteen seconds.

The perfect knowledge of the recoil of guns has enabled the reaction of the discharge to be utilised in compressing air or springs by which guns can be raised from concealed positions in order to deliver their fire, and then made to disappear again for loading; or the same force has been used to run up the guns automatically immediately after firing, or, as in the case of the Maxim gun, to deliver in the same way a continuous stream of bullets at the rate of ten in one second.

In the manufacture of shot and shell cast iron has been almost superseded by cast and wrought steel, though the hardened Palliser projectiles still hold their place. The forged-steel projectiles are produced by methods very similar to those used in the manufacture of metal cartridge-cases, though the process is carried on at a red heat and by machines much more powerful.

In every department concerned in the production of warlike stores electricity is playing a more and more important part. It has enabled the passage of a shot to be followed from its seat in the gun to its destination.

In the gun, by means of electrical contacts arranged in the bore, a time-curve of the passage of the shot can be determined. From this the mathematician constructs the velocity-curve, and from this, again, the pressures producing the velocity are estimated, and used to check the same indications obtained by other means. The velocity of the shot after it has left the gun is easily ascertained by the Boulangé apparatus.

Electricity and photography have been laid under contribution for obtaining records of the flight of projectiles and the effects of explosions at the moment of their occurrence. Many of you will recollect Mr. Vernon Boys' marvellous photographs showing the progress of the shot driving before it waves of air in its course.

Electricity and photography also record the properties of metals and their alloys as determined by curves of cooling.

The readiness with which electrical energy can be converted into heat or light has been taken advantage of for the firing of guns, which in their turn can, by the same agency, be laid on the object by means of range-finders placed at a distance and in advantageous and safe positions; while the electric light is utilised to illumine the sights at night, as well as to search out the objects of attack.

The compact nature of the glow-lamp, the brightness of the light, the circumstance that the light is not due to combustion, and therefore independent of air, facilitates the examination of the bore of guns, the insides of shells, and other similar uses—just as it is used by a doctor to examine the throat of a patient.

INFLUENCE OF INTERCOMMUNICATION AFFORDED BY THE BRITISH ASSOCIATION ON SCIENCE PROGRESS.

The advances in engineering which have produced the steam-engine, the railway, the telegraph, as well as our engines of war, may be said to be the result of commercial enterprise rendered possible only by the advances which have taken place in the several branches of science since 1831. Having regard to the intimate relations which the several sciences bear to each other, it is abundantly clear that much of this progress could not have taken place in the past, nor could further progress take place in the future, without intercommunication between the students of different branches of science.

The founders of the British Association based its claims to utility upon the power it afforded for this intercommunication. Mr. Vernon Harcourt (the uncle of your present General Secretary), in the address he delivered in 1832, said: "How feeble is man for any purpose when he stands alone—how strong when united with other men!

"It may be true that the greatest philosophical works have been achieved in privacy, but it is no less true that these works would never have been accomplished had the authors not mingled with men of corresponding pursuits, and from the commerce of ideas often gathered germs of apparently insulated discoveries, and without such material aid would seldom have carried their investigations to a valuable conclusion."

I claim for the British Association that it has fulfilled the objects of its founders, that it has had a large share in promoting intercommunication and combination.

Our meetings have been successful because they have maintained the true principles of scientific investigation. We have been able to secure the continued presence and concurrence of the master-spirits of science. They have been willing to sacrifice their leisure, and to promote the welfare of the Association, because the meetings have afforded them the means of advancing the sciences to which they are attached.

The Association has, moreover, justified the views of its founders in promoting intercourse between the pursuers of science, both at home and abroad, in a manner which is afforded by no other agency.

The weekly and sessional reunions of the Royal Society, and the annual soirées of other scientific societies, promote this intercourse to some extent, but the British Association presents to the young student during its week of meetings easy and continuous social opportunities for making the acquaintance of leaders in science, and thereby obtaining their directing influence.

It thus encourages, in the first place, opportunities of combination, but, what is equally important, it gives at the same time material assistance to the investigators whom it thus brings together.

The reports on the state of science at the present time, as they appear in the last volume of our *Transactions*, occupy the same important position, as records of science progress, as that occupied by those reports in our earlier years. We exhibit no symptom of decay.

SCIENCE IN GERMANY FOSTERED BY THE STATE AND MUNICIPALITIES.

Our neighbours and rivals rely largely upon the guidance of the State for the promotion of both science teaching and of research. In Germany the foundations of technical and industrial training are laid in the Realschulen, and supplemented by the Higher Technical Schools. In Berlin that splendid institution, the Royal Technical High School, casts into the shade the facilities for education in the various Polytechnics which we are now establishing in London. Moreover, it assists the practical workman by a branch department, which is available to the public for testing building materials, metals, paper, oil, and other matters. The standards of all weights and measures used in trade can be purchased from or tested by the Government Department for Weights and Measures.

For developing pure scientific research and for promoting new applications of science to industrial purposes the German Government, at the instance of von Helmholtz, and aided by the munificence of Werner von Siemens, created the Physikalische Reichsanstalt at Charlottenburg.

This establishment consists of two divisions. The first is charged with pure research, and is at the present time engaged in various thermal, optical, and electrical and other physical investigations. The second branch is employed in operations of delicate standardising to assist the wants of research students—

for instance, dilatation, electrical resistances, electric and other forms of light, pressure gauges, recording instruments, thermometers, pyrometers, lenses, tuning-forks, glass, oil-testing apparatus, viscosity of glycerine, &c.

Dr. Kohlrausch succeeded Helmholtz as president, and takes charge of the first division. Prof. Hagen, the director under him, has charge of the second division. A professor is in charge of each of the several sub-departments. Under these are various subordinate posts, held by younger men, selected for previous valuable work, and usually for a limited time.

The general supervision is under a Council consisting of a president, who is a Privy Councillor, and twenty-four members, including the president and director of the Reichsanstalt; of the other members, about ten are professors or heads of physical and astronomical observatories connected with the principal universities in Germany. Three are selected from leading firms in Germany representing mechanical, optical, and electric science, and the remainder are principal scientific officials connected with the Departments of War and Marine, the Royal Observatory at Potsdam, and the Royal Commission for Weights and Measures.

This Council meets in the winter, for such time as may be necessary, for examining the research work done in the first division during the previous year, and for laying down the scheme for research for the ensuing year; as well as for suggesting any requisite improvements in the second division. As a consequence of the position which science occupies in connection with the State in continental countries, the services of those who have distinguished themselves either in the advancement or in the application of science are recognised by the award of honours; and thus the feeling for science is encouraged throughout the nation.

ASSISTANCE TO SCIENTIFIC RESEARCH IN GREAT BRITAIN.

Great Britain maintained for a long time a leading position among the nations of the world by virtue of the excellence and accuracy of its workmanship, the result of individual energy; but the progress of mechanical science has made accuracy of workmanship the common property of all nations of the world. Our records show that hitherto, in its efforts to maintain its position by the application of science and the prosecution of research, England has made marvellous advances by means of voluntary effort, illustrated by the splendid munificence of such men as Gassiot, Joseph Whitworth, James Mason, and Ludwig Mond; and, whilst the increasing field of scientific research compels us occasionally to seek for Government assistance, it would be unfortunate if by any change voluntary effort were fettered by State control.

The following are the principal voluntary agencies which help forward scientific research in this country:—The Donation Fund of the Royal Society, derived from its surplus income. The British Association has contributed £60,000 to aid research since its formation. The Royal Institution, founded in the last century, by Count Rumford, for the promotion of research, has assisted the investigations of Davy, of Young, of Faraday, of Frankland, of Tyndall, of Dewar, and of Rayleigh. The City Companies assist scientific research and foster scientific education both by direct contributions and through the City and Guilds Institute. The Commissioners of the Exhibition of 1851 devote £6000 annually to science research scholarships, to enable students who have passed through a college curriculum and have given evidence of capacity for original research to continue the prosecution of science, with a view to its advance or to its application to the industries of the country. Several scientific societies, as, for instance, the Geographical Society and the Mechanical Engineers, have promoted direct research, each in their own branch of science, out of their surplus income; and every scientific society largely assists research by the publication, not only of its own proceedings, but often of the work going on abroad in the branch of science which it represents.

The growing abundance of matter year by year increases the burden thus thrown on their finances, and the Treasury has recently granted to the Royal Society £1000 a year, to be spent in aid of the publication of scientific papers not necessarily limited to those of that Society.

The Royal Society has long felt the importance to scientific research of a catalogue of all papers and publications relating to pure and applied science, arranged systematically both as to authors' names and as to subject treated, and the Society has been engaged for some time upon a catalogue of that nature. But the daily increasing magnitude of these publications, coupled with the necessity of issuing the catalogue with adequate promptitude, and at appropriate intervals, renders it a task which could only be performed under International co-operation. The officers of the Royal Society have therefore appealed to the Government to urge Foreign Governments to send delegates to a Conference to be held next July to discuss the desirability and the scope of such a catalogue, and the possibility of preparing it.

The universities and colleges distributed over the country, besides their function of teaching, are large promoters of research, and their voluntary exertions are aided in some cases by contributions from Parliament in alleviation of their expenses.

Certain executive departments of the Government carry on research for their own purposes, which in that respect may be classed as voluntary. The Admiralty maintains the Greenwich Observatory, the Hydrographical Department, and various experimental services; and the War Office maintains its numerous scientific departments. The Treasury maintains a valuable chemical laboratory for Inland Revenue, Customs, and agricultural purposes. The Science and Art Department maintains the Royal College of Science, for the education of teachers and students from elementary schools. It allows the scientific apparatus in the national museum to be used for research purposes by the professors. The Solar Physics Committee, which has carried on numerous researches in solar physics, was appointed by and is responsible to this Department. The Department also administers the Sir Joseph Whitworth engineering research scholarships. Other scientific departments of the Government are aids to research, as, for instance, the Ordnance and the Geological Surveys, the Royal Mint, the Natural History Museum, Kew Gardens, and other lesser establishments in Scotland and Ireland; to which may be added, to some extent, the Standards Department of the Board of Trade, as well as municipal museums, which are gradually spreading over the country.

For direct assistance to voluntary effort the Treasury contributes £4000 a year to the Royal Society for the promotion of research, which is administered under a board whose members represent all branches of science. The Treasury, moreover, contributes to marine biological observatories, and in recent years has defrayed the cost of various expeditions for biological and astronomical research, which in the case of the *Challenger* expedition involved very large sums of money.

In addition to these direct aids to science, Parliament, under the Local Taxation Act, handed over to the County Councils a sum, which amounted in the year 1893 to £615,000, to be expended on technical education. In many country districts, so far as the advancement of real scientific technical progress in the nation is concerned, much of this money has been wasted for want of knowledge. And whilst it cannot be said that the Government or Parliament have been indifferent to the promotion of scientific education and research, it is a source of regret that the Government did not devote some small portion of this magnificent gift to affording an object-lesson to County Councils in the application of science to technical instruction, which would have suggested the principles which would most usefully guide them in the expenditure of this public money.

Government assistance to science has been based mainly on the principle of helping voluntary effort. The Kew Observatory was initiated as a scientific observatory by the British Association. It is now supported by the Gassiot Trust Fund, and managed by the Kew Observatory Committee of the Royal Society. Observations on magnetism, on meteorology, and the record of sun-spots, as well as experiments upon new instruments for assisting meteorological, thermometrical, and photographic purposes, are being carried on there. The Committee has also arranged for the verification of scientific measuring instruments, the rating of chronometers, the testing of lenses and of other scientific apparatus. This institution carries on to a limited extent some small portion of the class of work done in Germany by that magnificent institution, the Reichsanstalt of Charlottenburg, but its development is fettered by want of funds. British students of science are compelled to resort to Berlin and Paris when they require to compare their more delicate instruments and apparatus with recognised standards. There could scarcely be a more advantageous addition to the assistance which Government now gives to science than for it to allot a substantial annual sum to the extension of the Kew Observatory, in order to develop it on the model of the Reichsanstalt. It might advantageously retain its connection with the Royal Society, under a Committee of Management representative of the various branches of science concerned, and of all parts of Great Britain.

CONCLUSION.

The various agencies for scientific education have produced numerous students admirably qualified to pursue research; and at the same time almost every field of industry presents openings for improvement through the development of scientific methods. For instance, agricultural operations alone offer openings for research to the biologist, the chemist, the physicist, the geologist, the engineer, which have hitherto been largely overlooked. If students do not easily find employment, it is chiefly attributable to a want of appreciation for science in the nation at large.

This want of appreciation appears to arise from the fact that those who nearly half a century ago directed the movement of national education were trained in early life in the universities, in which the value of scientific methods was not at that time fully recognised. Hence our elementary, and even our secondary and great public schools, neglected for a long time to encourage the spirit of investigation which develops originality. This defect is diminishing daily.

There is, however, a more intangible cause which may have had influence on the want of appreciation of science by the nation. The Government, which largely profits by science, aids it with money, but it has done very little to develop the national appreciation for science by recognising that its leaders are worthy of honours conferred by the State. Science is not fashionable, and science students—upon whose efforts our progress as a nation so largely depends—have not received the same measure of recognition which the State awards to services rendered by its own officials, by politicians, and by the Army and by the Navy, whose success in future wars will largely depend on the effective applications of science.

The Reports of the British Association afford a complete chronicle of the gradual growth of scientific knowledge since 1831. They show that the Association has fulfilled the objects of its founders in promoting and disseminating a knowledge of science throughout the nation.

The growing connection between the sciences places our annual meeting in the position of an arena where representatives of the different sciences have the opportunity of criticising new discoveries and testing the value of fresh proposals, and the Presidential and Sectional Addresses operate as an annual stocktaking of progress in the several branches of science represented in the Sections. Every year the field of usefulness of the Association is widening. For, whether with the geologist we seek to write the history of the crust of the earth, or with the biologist to trace out the evolution of its inhabitants, or whether with the astronomer, the chemist, and the physicist we endeavour to unravel the constitution of the sun and the planets or the genesis of the nebulæ and stars which make up the universe, on every side we find ourselves surrounded by mysteries which await solution. We are only at the beginning of work.

I have, therefore, full confidence that the future records of the British Association will chronicle a still greater progress than that already achieved, and that the British nation will maintain its leading position amongst the nations of the world, if it will energetically continue its voluntary efforts to promote research, supplemented by that additional help from the Government which ought never to be withheld when a clear case of scientific utility has been established.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. W. M. HICKS, M.A., D.Sc., F.R.S., PRESIDENT OF THE SECTION.

IN making a choice of subject for my address the difficulty is not one of finding material but of making selection. The field covered by this Section is a wide one. Investigation is active in every part of it, and is being rewarded with a continuous stream of new discoveries and with the growth of that coordination and correlation of facts which is the surest sign of real advancement in science. The ultimate aim of pure science is to be able to explain the most complicated phenomena of nature as flowing by the fewest possible laws from the simplest fundamental data. A statement of a law is either a confession of ignorance or a mnemonic convenience. It is the latter, if it is deducible by logical reasoning from other laws. It is the former when it is only discovered as a fact to be a law. While, on the one hand, the end of scientific investigation is the discovery of laws, on the other, science will have reached its highest goal when it shall have reduced ultimate laws to one or two, the necessity of which lies outside the sphere of our cognition. These ultimate laws—in the domain of physical science at least—will be the dynamical laws of the relations of matter to number, space, and time. The ultimate data will be number, matter, space, and time themselves. When these relations shall be known, all physical phenomena will be a branch of pure mathematics. We shall have done away with the necessity of the conception of potential energy, even if it may still be convenient to retain it; and—if it should be found that all phenomena are manifestations of motion of one single continuous medium—the idea of force will be banished also, and the study of dynamics replaced by the study of the equation of continuity.

Before, however, this can be attained, we must have the working drawings of the details of the mechanism we have to deal with. These details lie outside the scope of our bodily senses; we cannot see, or feel, or hear them, and this, not because they are unseeable, but because our senses are too coarse-grained to transmit impressions of them to our mind. The ordinary methods of investigation here fail us; we must proceed by a special method, and make a bridge of communication between the mechanism and our senses by means of hypotheses. By our imagination, experience, intuition we form theories; we deduce the consequences of these theories on phenomena which come within the range of our senses, and reject or modify and try again. It is a slow and laborious process. The wreckage of rejected theories is appalling; but a knowledge of what actually goes on behind what we can see or feel is surely if slowly being attained. It is the rejected theories which have been the necessary steps towards formulating others nearer the truth. It would be an extremely interesting study to consider the history of these discarded theories; to show the part they have taken in the evolution of truer conceptions, and to trace the persistence and modification of typical ideas from one stratum of theories to a later. I propose, however, to ask your attention for a short time to one of these special theories—or rather to two related theories—on the constitution of matter and of the ether. They are known as the vortex atom theory of matter, and the vortex sponge theory of the ether. The former has been before the scientific world for a quarter of a century, since its first suggestion by Lord Kelvin in 1867, the second for about half that time. In what I have to say I wish to take the position not of an advocate for or against, but simply as a prospector attempting to estimate what return is likely to be obtained by laying down plant to develop an unknown district. This is, in fact, the state of these two theories at present. Extremely little progress has been made in their mathematical development, and until this has been done more completely we cannot test them as to their powers of adequately explaining physical phenomena.

The theory of the rigid atom has been a very fruitful one, especially in explaining the properties of matter in the gaseous state; but it gives no explanation of the apparent forces which hold atoms together, and in many other respects it requires supplementing. The elastic solid ether explained much, but there are difficulties connected with it—especially in connection with reflection and refraction—which decide against it. The mathematical rotational ether of MacCullagh is admirably adapted to meet these difficulties, but he could give no physical conception of its mechanism. Maxwell and Faraday proposed a special ether for electrical and magnetic actions. Maxwell's identification of the latter with the luminiferous ether, his deduction of the velocity of propagation of light and of indices of refraction in terms of known electrical and magnetic constants, will form one of the landmarks in the history of science. This ether requires the same mathematical treatment as that of MacCullagh. Lord Kelvin's gyrostatic model of an ether is also of the MacCullagh type. Lastly, we have Lord Kelvin's labile ether, which again avoids the objections to the elastic solid ether. In MacCullagh's type of ether the energy of the medium when disturbed depends only on the twists produced in it. This ether has recently been mathematically discussed by Dr. Larmor, who has shown that it is adequate to explain all the various phenomena of light, electricity, and magnetism. To this I hope to return later. Meanwhile, it may be borne in mind that the vortex sponge ether belongs to MacCullagh's type.

Already before a formal theory of a fluid ether had been attempted, Lord Kelvin ("Vortex Atoms," *Proc. Roy. Soc., Edin.*, vi. 94; *Phil. Mag.* (4), 34) had proposed his theory of vortex atoms. The permanence of a vortex filament with its infinite flexibility, its fundamental simplicity with its potential capacity for complexity, struck the scientific imagination as the thing which was wanted. Unfortunately the mathematical difficulties connected with the discussion of these motions, especially the reactions of one on another, have retarded the full development of the theory. Two objections in chief have been raised against it, viz. the difficulty of accounting for the densities of various kinds of matter, and the fact that in a vortex ring the velocity of translation decreases as the energy increases. There are two ways of dealing with a difficulty occurring in a general theory—one is to give up the theory, the other is to try and see if it can be modified to get over the difficulty. Such difficulties are to be welcomed as means of help in arriving at greater exactness in details. It is a mistake to submit too readily to crucial experiments. The very valid crucial objection of Stokes to MacCullagh's ether is a case in point. It drew away attention from a theory which, in the light of later developments, gives great hope of leading us to correct ideas. As Larmor has pointed out, this objection vanishes when we have intrinsic rotations in the ether itself. A special danger to guard against is the importation into one theory of ideas which have grown out of one essentially different. This remark has reference to the apparent difficulty of decrease of velocity with increased energy.

Maxwell was, I believe, the first to point out the difficulty of explaining the masses of the elements on the vortex atom hypothesis. To me it has always appeared one of the greatest stumbling-blocks to the acceptance of the theory. We have always been accustomed to regard the ether as of extreme tenuity, as of a density extremely though not infinitely smaller than that of gross matter, and we carry in our minds that Lord Kelvin has given an inferior limit of about 10^{-19}. There are two directions in which to seek a solution. The first is to cut the knot by supposing that the atoms of gross matter are composed of filaments whose rotating cores are of much greater density than the ether itself. The second is to remember that Lord Kelvin's number was obtained on the supposition of elastic solid ether, and does not necessarily apply to the vortex sponge. Unfortunately, however, for the first explanation, the mathematical discussion[1] shows that a ring cannot be stable unless the density of the fluid outside the core is equal to, or greater than, that inside. This instability also cannot be cured by supposing an additional circulation added outside the core. Unless, therefore, some modification of the theory can be made to secure stability this idea of dense fluid cores must be given up.

We seem, then, forced back to the conclusion that the density of the ether must be comparable with that of ordinary matter. The effective mass of any atom is not composed of that of its core alone, but also of that portion of the surrounding ether which is carried along with it as it moves through the medium. Thus a rigid sphere moving in a liquid behaves as if its mass were increased by half that of the displaced liquid. In the case of a vortex filament the ratio of effective to actual mass may be much larger. In this explanation the density of the matter composing an atom is the same for all, whilst their masses depend on their volumes and configurations combined. Now the configuration alters with the energy, and this would make the mass depend to some extent at least on the temperature. However repugnant this may be to current ideas, we are not entitled to deny its possibility, although such an effect must be small or it would have been detected. Such a variation, if it exists, is not to be looked for by means of the ordinary gravitation balance, but by the inertia or ballistic balance. The mass of the core itself remains, of course, constant, but the effective mass—that which we can measure by the mechanical effects which the moving vortex produces—is a much more complicated matter, and requires much fuller consideration than has been given to it.

The conditions of stability allow us to assume vacuous cores or cores of less density than the rest of the medium. If we do this, then the density of the ether itself may be greater than that of gross matter. Until, however, we meet with phenomena whose explanation requires this assumption, it would seem preferable to take the density everywhere the same. In this case the density of the ether must be rather less than the apparent density of the lightest of any of the elements, taking the apparent density to mean the effective mass of a vortex atom per its volume. This will probably be commensurable with the density of the matter in its most compressed state, and will lie between ·5 and 1—comparable, that is to say, with the density of water. Larmor,[1] from a special form of hypothesis for a magnetic field in the rotationally elastic ether, is led to assign a density of the same order of magnitude. If the density be given it is easy to calculate the intrinsic energy per c.c. in the medium. The velocity of propagation of light in a vortex sponge ether, as deduced by Lord Kelvin,[2] is ·47 times the mean square velocity of the intrinsic motion of the medium. This gives for the mean square velocity $6{\cdot}3 \times 10^{10}$ cm. per second. If we follow Lord Kelvin and use for comparison the energy of radiation per c.c. near the sun, or say 1·8 erg per c.c., the resulting density will be 10^{-21}. The energy per c.c. in a magnetic field of 15,000 c.g.s. units is about 1 joule. If we take this for comparison we get a density of 10^{-14}. But the intrinsic energy of the fluid must be extremely great compared with the energy it has to transmit. If it were a million times greater the density would still only amount to 10^{-8}—comparable with the density of the residual gas in our highest vacua. To account for the density of gross matter on the supposition that it is built up out of the same material as the ether leads to a density between ·5 and 1. This gives the enormous energy of 10^{14} joules per c.c. In other words, the energy contained in one cubic centimetre of the ether is sufficient to raise a kilometre cube of lead 1 metre high against its weight. Thus the difficulty in explaining the mass of ordinary matter seems to reduce itself to a difficulty in believing that the ether possesses such an enormous store of energy. It may be that there are special reasons against such a large density. Larmor refers to the large forcives which would be called into play by hydrodynamical motions. Perhaps an answer to this may be found in the remark that where all the matter is of the same density the motions are kinematically deducible from the configuration at the instant, and are independent of the density. It is only where other causes act, such, *e.g.*, as indirectly depend on the mean pressure of the fluid or where vacuous spaces occur, that the actual value of the density may modify the measurable forcives.

Ever since Prof. J. J. Thomson proved that a vortex atom theory of matter is competent to serve as a basis of a kinetic theory of gases, it has been urged by various persons as a fatal objection that the translation velocity of the atoms falls off as the temperature rises. I must confess this objection has never appealed to me. Why should not the velocity fall off? The velocity of gaseous molecules has never been directly observed, nor has it been experimentally proved that it increases with rise of temperature. We have no right to import ideas based on the kinetic theory of hard discrete atoms into the totally distinct theory of mobile atoms in continuity with the medium surrounding them. Doubtless the molecules of a gas effuse through a small orifice more quickly as the temperature rises, but it is natural to suppose that a vortex ring would do the same as its energy increases. To make the objection valid, it is necessary to show that a vortex ring passing through a small tube, comparable with its own diameter, would pass through more slowly the greater its energy. It is not, however, necessarily the case that in every vortex aggregate the velocity decreases as the energy increases. The mathematical treatment of thin vortex filaments is comparatively easy, and little attention has been paid to other cases. Let us attempt to trace the life history as to translation velocity and energy of a vortex ring. We start with the energy large; the ring now has a very large aperture, and has a very thin filament. As the energy decreases the aperture becomes smaller, the filament thicker, and the velocity of translation greater. We can trace quantitatively the whole of this part of its history until the thickness of the ring has increased to about four times the diameter of the aperture, or perhaps a little further. Then the mathematical treatment employed fails us or becomes very laborious to apply. Till eighteen months ago, this was the only portion of its history we could trace. Then Prof. M. J. M. Hill ("On a Spherical Vortex," *Phil. Trans.*, 1894) published his beautiful discovery of the existence of a spherical vortex. This consists of a spherical

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[1] An error in the expression on p. 768 of "Researches in the Theory of Vortex Rings," *Phil. Trans.*, pt. ii. 1885, vitiates the conclusion there drawn. If this be corrected the result mentioned above follows. See also Basset, "Treatise on Hydrodynamics," § 338, and *Amer. Jour. Math.*

[1] "A Dynamical Theory of the Electric and Luminiferous Medium," *Phil. Trans.*, 1894, A. p. 779.

[2] "On the Propagation of Laminar Motion through a Turbulently Moving Inviscid Liquid," *Phil. Mag.*, October 1887.

mass of fluid in vortical motion and moving bodily through the surrounding fluid, precisely as if it were a rigid sphere. This enables us to catch a momentary glimpse, as it were, of our vortex ring some little time after it has passed out of our ken. The aperture has gone on contracting, the ring thickening, and altering the shape of its cross section in a manner whose exact details have not yet been calculated. At last we just catch sight of it again as the aperture closes up. We find the ring has changed into a spherical ball, with still further diminished energy and increased velocity. We then lose sight of it again, but it now lengthens out, and towards the end of its course approximates to the form of a rod moving parallel to its length through the fluid with energy and velocity which again can be approximately determined. In this part of its life the velocity of translation decreases with decrease of energy. I believe it will be found, when the theory is completely worked out, that the spherical atom is the stage where this reversal of property takes place.

Even in the ring state, however, the change of velocity with energy is very small; much smaller, I think, than is generally recognised. When the energy is increased to twenty times that of the spherical vortex, the velocity is only diminished to two-thirds its previous value. If at ordinary temperatures, say 20° C., the vortex was in the spherical shape, then at 3000° C. its velocity of translation would only have been reduced to four-fifths its value at the lower temperature, whilst the aperture of the ring would have a radius about 1·4 times that of the sphere. At 2000° C. the velocity would not differ by much more than one-twentieth from its original value. In fact, near the spherical state the alteration in velocity of translation is very slow. It is therefore possible, that if the atoms of matter be vortex aggregates, the state in which we can experimentally test our theory is just that in which the mathematical discussion fails us. Other modifications tend to diminish this change of velocity. I will refer here to three only. The first is that of hollow vortices. We must not, however, postulate vacuous atoms without any rotational core at all; for in this case we should probably lose the essential property of permanence. The question has not been fully investigated, but there can be little doubt that by diminishing the energy of a completely hollow vortex we can cause it to disappear. We can certainly create one in a perfect fluid. Secondly, J. J. Thomson has shown that if a molecule be composed of linked filaments, the energy increases as the components move further apart. In such a case an extra supply of energy goes to expanding the molecule, and less, if any, to increasing the aperture. Lastly, a modification of the atomic motion to which I shall refer later, and which seems called for to explain the magnetic rotation of the plane of polarisation of light, will also tend to lessen the change of size, and therefore change of velocity with change of energy, even if it does not reverse the property.

If we pass on to consider how a vortex atom theory lends itself to the explanation of physical and chemical properties of matter independently of what may be called ether relations, we find that we owe almost all our knowledge on this point to the work of Prof. J. J. Thomson ("A Treatise on the Motion of Vortex Rings," Macmillan, 1883), which obtained the Adams' Prize in 1882. This, however, is confined to the treatment of *thin* vortex rings, still leaving a wide field for future investigations in connection with thick rings and with vortex aggregates which produce no cyclosis in the surrounding medium. His work is an extremely suggestive one. He shows that such a theory is capable not only of explaining the gaseous laws of a so-called perfect gas, but possibly also the slight deviations therefrom. Quite as striking is his explanation of chemical combination—an explanation which flows quite naturally from the theory. A vortex filament can be linked on itself: two or more can be linked together, like helices drawn on an anchor ring; or, lastly, several can be arranged together like parallel rings successively threading one another. In the latter case, for such an arrangement to be permanent, the strengths of each ring must be the same, and further, not more than six can thus be combined together. The linked vortices will be in permanent combination on account of their linkedness; the other arrangement may be permanent if subject to no external actions. If, however, they are disturbed by the presence of other vortices they may break up. When atoms are thus combined to form a compound, a certain number of molecules will always be dissociated; the compound will be permanent when the ratio of the average paired time to the unpaired time of any atom is large. Thomson considers every filament to be of the same strength. Then an atom consisting of two links will behave like a ring of twice the strength, one of three links, of three times the strength, and so on. On this theory chemical compounds are to be regarded as systems of rings, not linked into one another but close together, and all engaged in the operation of threading each other. The conditions for permanence are: (1) the strength of each ring must be the same, (2) the number must be less than 6. Now apply this. H and Cl have equal linkings, therefore equal strength. Consequently we can have molecules of HCl, or any combinations up to 6 atoms per molecule, although the simpler one is the most likely. O has twice the linking, therefore the strength double. Hence one of H and one of O cannot revolve in permanent connection. We require first to arrange two of H together to form one system. This system has the same strength as O, they can therefore revolve in permanent connection, and we get the water molecule. *Or* we may take two of the O atoms and one of the double H molecule, and they can form a triple system of three rings threading one another in permanent connection, and we get the molecule H_2O_2. This short example will be sufficient to indicate how the theory gives a complete account of valency.

The energy of rings thus combined is less than when free; consequently they are stable, and the act of combination sets free energy. Further, Thomson points out that for two rings to combine their sizes must be about the same when they come into proximity; consequently combination can only occur between two limits of temperature corresponding to the energies within which the radii of both kinds of rings are near an equality.

We can easily extend Thomson's reasoning to explain the combination of two elements by the presence of a third neutral substance. Call the two elements which are to combine A and B, and the neutral substance C. The radii of A and B are to be supposed too unequal to allow them to come close enough together to combine. If now at the given temperature the C atom has a radius intermediate to those of A and B, it is more nearly equal to each than they are to one another; C picks up one of A, and after a short time drops it; A will leave C with its radius brought up (say) to closer equality with it. The same thing happens with the B atoms, and they leave C with their radii brought down to closer equality with it. The result is that A and B are brought into closer equality with one another, and if this is of sufficient amount, they can combine and do so, while C remains as before and apparently inert.

Thomson's theory of chemical combination applies only to thin rings. Something analogous may hold also for thick rings, but it is clearly inapplicable to vortex aggregates similar to that of Hill's. We are not confined, however, to this particular kind of association of vortex atoms in a molecule. For instance, I have recently found (not yet published) that one of Hill's vortices can swallow up another and retain it inside in relative equilibrium. The matter requires fuller discussion, but it seems to open up another mode of chemical combination.

A most important matter which has not yet been discussed at all is the relation between the mean energy of the vortex cores, and the energy of the medium itself when the atoms are close enough to affect each other's motions (as in a gas). The fundamental ideas are quite different from those underlying the well-known kinetic theory of gases of hard atoms. Nevertheless, many of the results must be very similar, based as both are on dynamical ideas. Whether it will avoid certain difficulties of the latter, especially those connected with the ratio of the specific heats, remains to be seen. The first desideratum is the determination of the equilibrium of energy between vortices and medium, and before this is done it is useless to speculate further in this region.

A vortex atom theory of matter carries with it the necessity of a fluid ether. If such a fluid is to transmit transversal radiations, some kind of quasi-elasticity must be produced in it. This can be done by supposing it to possess energetic rotational motions whose mean velocity is zero, within a volume whose linear dimension is small compared with the wave-length of light, but whose velocity of mean square is considerable. That an ether thus constituted is capable of transmitting transverse vibrations I showed before this Section at the Aberdeen meeting of the Association ("On the Constitution of the Luminiferous Ether on the Vortex Atom Theory," *Brit. Assoc. Reports*, 1885, p. 930), by considering a medium composed of closely packed discrete small vortex rings. Lord Kelvin ("On the Vortex Theory of the Luminiferous Ether," *Brit. Assoc. Reports*, 1887, p. 486, also

Phil. Mag., October 1887, p. 342) at the Manchester meeting discussed the question much more thoroughly and satisfactorily, and deduced that the velocity of propagation was $\sqrt{2/3}$ times the velocity of mean square of the turbulent motion. We can make little further progress until we know something of the arrangement of the small motions which confer the quasi-rigidity. This may be completely irregular and unsteady, or arranged in some definite order of steady motions. I am inclined to the view that the latter is nearer the truth. In this case we should expect a regular structure of small cells in which the motions are all similar. By the word cell I do not mean a small vessel bounded by walls, but a portion of the fluid in which the motion is a complete system in itself. Such a theory might be called a cell theory of the ether. The simplest type, perhaps, is to suppose the medium spaced into rectangular boxes, in each of which the motion may be specified as follows: Holding the box with one set of faces horizontal the fluid streams up in the centre of the box, then turns round, flows down the sides and up the centre again. In fact, it behaves like a Hill's vortex squeezed from a spherical into a box form. Each box has thus rotational circulation complete in itself. The six adjoining compartments have their motion the same in kind, but in the reverse direction, and so on. In this way we get continuous and energetic small motions throughout the medium, and the state is a stable one. If there is a shear, so that each cell becomes slightly rhomboidal, the rotational motions inside tend to prevent it, and thus propagate the disturbance, but the cells produce no effect on the general irrotational motion of the fluid, at least when the irrotational velocities are small compared with those of the propagation of light. In this case the rate at which the cells adjust themselves to an equilibrium position is far quicker than the rate at which this equilibrium distribution is disturbed by the gross motions. The linear dimensions of the cells must be small compared with the wave-lengths of light. They must probably be small also compared with the atoms of gross matter, which are themselves small compared with the same standard.

We may regard each cell as a dynamical system by itself, into which we pour or take away energy. This added energy will depend only on the shape into which the box is deformed. We may then, for our convenience in considering the gross motions of the medium as a whole, *i.e.* our secondary medium, regard these as interlocked systems, neglect the direct consideration of the motions inside them, but regard the energy which they absorb as a potential function for the general motion. This potential function will contain terms of two kinds, one involving the shear of the cells, and this shear will be the *same* as the rotational deformation in the secondary medium. The second will depend on alterations in the ratios of the edges of the cells (including other changes of form involving no rotations). The former will give rise to waves of transversal displacements. The second cannot be transmitted as waves, but may produce local effects. If a continuous solid be placed in such a medium, the cells will rearrange themselves so as to keep the continuity of their motions. The cells will become distorted (but without resultant shear), and a static stress will be set up. We have then to deal with the primary stuff itself, whose rotation gives a structure to the ether, and the structural ether itself. The former we may call the primary medium. The ether which can transmit transversal disturbances, and which is built up out of the first, we may call the secondary medium. Whether an atom of matter is to be considered as a vortical mass of the primary or of the secondary medium is a matter to be left open in the present state of the theory.

At the Bath meeting of this Association, I sketched out a theory of the electrical action of a fluid ether in which electrical lines of force were vortex filaments combined with an equivalent number of hollow vortices of the same vortical strength. ("A Vortex Analogue of Static Electricity," *Brit. Assoc. Rep.*, 1888, p. 577.) An electric charge on a body depended on the number of ends of filaments abutting on it, the sign being determined by the direction of rotation of the filament looked at from the body. This theory gave a complete account of electrostatic actions, both quantitatively and qualitatively, and a more speculative one as to currents and magnetism. I could only succeed in proving at that time that if the filaments were distributed according to the same laws as electric lines of force, the distribution would be one of equilibrium. Larmor ("A Dynamical Theory of the Electric and Luminiferous Medium," *Phil. Trans.*, 1894, p. 748) has recently proved that this is also the necessary distribution for any type of a rotationally elastic ether, and consequently also for this particular case. Currents along a wire were supposed to consist of the ends of filaments running along it, with disappearance of the hollow companions, the filaments producing at the same time a circulation round the wire. A magnetic field was thus to be produced by a flow of the ether, but probably with the necessary accompaniment of rotational elements in it.

This latter, however, was clearly wrong, because each kind of filament would produce a circulation in opposite directions. The correct deduction would have been to lay stress on the fact that the field is due to the motion through the stationary ether of the vortex filaments, the field being perpendicular to the filament and to its direction of motion. This motion would doubtless produce stresses in the cell-ether due to deformations of the cells, and be the proximate cause of the mechanical forces in the field. In any case, it is not difficult to show that a magnetic field cannot be due to an irrotational flow of the ether alone.[1] Such electrostatic and magnetic fields produce states of motion in the medium, but no bodily flow in it; consequently we ought not to expect an effect to be produced on the velocity of transmission of light through it.

The fundamental postulate underlying this explanation of electric action is that when two different kinds of matter are brought into contact a distribution of vortex filaments in the neighbourhood takes place, so that a larger number stretch from one to the other than in the opposite direction—the distinction between positive and negative ends being that already indicated. To see how such a distribution may be caused, let us consider each vortex atom to be composed of a vortical mass of our secondary medium or cell-structure ether. The atom is much larger than a cell, and contains practically an infinite number of them. It is a dynamical system of these cells with equilibrium of energy throughout its volume. The second atom is a dynamical system with a different equilibrium of energy. Where they come into contact there will be a certain surface rearrangement, which will show itself as a surface distribution of energy in a similar manner to that which exists between a molar collection of one kind of molecules in contact with one of another, and which shows itself in the phenomenon which we call surface tension. In the present case the effect may take place at the interface of two atomic systems in actual contact, or be a difference effect between the two interfaces of the ether and each atom when the latter are sufficiently close. The surface effect we are now considering shows itself as contact electricity.

Such a distribution of small vortex filaments, stretching from one atom to another, will tend to hold them together. We therefore get an additional cause for aggregation of atoms. This does not exclude the others already referred to. They may all act concurrently, some producing one effect, some another—one combining, perhaps, unknown primitive atoms into elements, one elements into chemical compounds, and another producing the cohesion of matter into masses.

On this theory the difference between a conductor and a dielectric is that in a dielectric the ends of the filaments cannot pass from atom to atom, possibly because the latter never come into actual contact. In a conductor, however, we are to suppose that the atomic elements can do so. When a current is flowing, a filament and its equivalent hollow stretch between two neighbouring atoms, they are pulled into contact, or their motions bring them into contact, the hollow disappears, and the rotational filament joins its two ends and sails away as a small neutral vortex ring into the surrounding medium, or returns to its function as an ether cell. The atoms being free are now pulled back to perform a similar operation for other filaments. The result is that the atoms are set into violent vibrations, causing the heating of the conductor. When, however, the metal is at absolute zero of temperature, there is no motion, the atoms are already in contact, and there is no resistance, as the observation of Dewar and Fleming tends to show. Further, as the resistance depends on the communication of motion from molecule to molecule, we should expect the electrical conduc-

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[1] To prove this, consider a straight conductor moving parallel to itself and perpendicular to a uniform magnetic field. There exists a permanent potential difference between its ends. If, however, the field consists of a flow of ether, the effect is the same as if the conductor is at rest, and the direction of the magnetic field shifted through an angle. But this is the case of a conductor at rest in a field, and there is therefore no potential difference between the ends. Hence a magnetic field must consist of some structure across which the conductor cuts. A field may possibly demand a flow of the ether, but, if so, it must carry in it some structure definitely oriented at each point to the direction of flow.

tivity of a substance to march with its thermal conductivity. Again, on this theory the resistance clearly increases with increase of distance between atoms—*i.e.* with increase of temperature. On the contrary, in electrolytic conduction the same junction of filament ends is brought about, not by oscillations of molecule to molecule, but by disruption of the molecule and passage of atom to atom. In this case conduction is easier the more easily a molecule is split up, and thus resistance decreases with increase of temperature. To explain the laws of electrolysis it is only necessary to assume that the strengths of all filaments are the same. A similar hypothesis, as we have seen, lies at the basis of J. J. Thomson's explanation of chemical combination, although it is not necessarily the case that we are dealing with the same kind of filaments. It is evident that the theory easily lends itself to his views as to the mechanism of the electric discharge through gases. The *modus operandi* of the production of the mechanical forcive on a conductor carrying a current in a magnetic field and of electrodynamic induction is not clear. Probably the full explanation is to be found in the stresses produced in the ether owing to the deformation of the cells by the passage of the filaments through them. The fluid moves according to the equation of continuity without slip, and subject to the surface conditions at the conductors. This motion, however, distorts the cells, and stresses are called into play. Any theory which can explain the mechanical forcives and also Ohm's law, must, on the principles of the conservation of energy, also explain the induction of currents.

The magnetic rotation of the plane of polarisation of light does not depend on the structure of the ether, or on the magnetic field itself, but is a result of the atomic configuration of the matter in the field modified by the magnetism. It is generally recognised as caused by something in the field rotating round the direction of the magnetic lines of force. Now the vortex atom, as usually pictured, is incapable of exhibiting this property. It is, however, an interesting fact, and one which I hope to demonstrate to this Section during the meeting, that a vortex ring can have two simultaneous and independent cyclic motions —one the ordinary one, and another which is capable of producing just the action on light which shows itself as a rotation of the plane of polarisation. The motion is rather a complicated one to describe without a diagram, but an idea of its nature may be obtained by considering the case of a straight cylindrical vortex. The ordinary straight vortex consists, as every one knows, of a cylinder of fluid revolving like a solid, and surrounded by a fluid in irrotational motion. In the core the velocity increases from zero at the axis to a maximum at its surface. Thence it continuously decreases in the outer fluid as the distance increases. Everywhere the motion is in a plane perpendicular to the axis. Let us now consider a quite different kind of vortical motion. Suppose the fluid is flowing along the core like a viscous fluid through a pipe; the velocity is zero at the surface and a maximum at the axis. Everywhere it is parallel to the axis, the vortex lines are circles in planes perpendicular to the axis, and concentric with it. Since the velocity at the surface of the core is zero, the surrounding fluid is also at rest. Now superpose this motion on the previous one, and it will be found to be steady. If a short length of this vortex be supposed cut off, bent into the shape of a circle and the ends joined, we shall have very a rough idea of the compound vortex ring of which I speak. I say a very rough idea, because the actual state of motion in a ring vortex or a Hill's vortex is not quite so simple as the analogy might lead one to think.

Now a compound vortex atom of this kind is just what we want to produce rotation of the plane of polarisation of light. The light passing through such a vortex has the direction of vibration twisted in the wave front. In ordinary matter no such rotation is produced, because the various atoms are indifferently directed, and they neutralise each other's effects. Let, however, a magnetic field be produced, and they will range themselves so that, on the average, the primary[1] circulations through the apertures will point in the direction of the field. Consequently the average direction of the secondary spin will be in planes perpendicular to this, and will rotate the plane of polarisation of any light whose wave front passes them. The rotation is produced only on the light which is transmitted *through* the vortex. The rotation observed is a resultant effect. In fact it is clear that in the case of refraction the optical media belong to the type in which every portion transmits the light, and not to the type in which refraction is produced by opaque bodies embedded in the ether. The atoms are only opaque if they contain vacuous cores. The question of the grip of the particles on the ether does not enter, but difference of quality—showing itself in refraction and dispersion—is due to difference in average rotational quasi-elasticity produced by the atomic circulations, and possibly absorption is due to precessional and nutational motion set up by the secondary spins. These, however, are perhaps rather vague speculations.

Instead of attempting to invent ethers, to deduce their properties from their specifications, and then seeing whether they fit in with experience, we may begin half-way. We may assume different forms for the function giving the energy of the medium when disturbed, apply general dynamical methods, and distinguish between those which are capable of explaining the phenomena we are investigating and those which are not. Invention is then called upon to devise a medium for which the desired energy-function is appropriate. This was the method applied by MacCullagh to the luminiferous ether. He obtained an algebraical form of the energy function which completely satisfied the conditions for a luminiferous ether; its essential property being that the energy depended only on the rotational displacements of its small parts. He was unable, however, to picture a stable material medium which would possess this property. We recognise now that such a medium is possible if the rotational rigidity is produced by intrinsic motions in the small parts of the medium of a gyrostatic nature. In a most masterly manner Larmor ("A Dynamical Theory of the Electric and Luminiferous Medium," *Phil. Trans.*, 1894) has recently investigated by general dynamical methods the possibility of explaining electric and magnetic phenomena by means of the same energy function. Electric lines of force are rotational filaments in the ether,[1] similar in fact to those I suggested at Bath, while a magnetic field consists of a flow of the ether. The same difficulty in accounting for electro-dynamic induction arises, but the general form of the equations for the electrodynamic and magnetic fields are the same as those generally received.

Towards the end of this paper he is led to postulate a theory of electrons whose convection through the ether constitutes an electric current. Two rotating round each other are supposed to produce the same effect as a vortex ring. The mass of ordinary matter is attributed to the electric inertia of these electrons. The electron itself is a centre or nucleus of rotational strain. If I express a doubt as to the possibility of the existence of these nuclei as specified, I do so with great diffidence.[2] Whether they can or cannot exist, however, the general results of the investigation are not affected.

Since this paper was published Larmor has read a second one on the same subject before the Royal Society, developing further his theory of the electron. The publication of this will be awaited with interest. It is impossible in an address such as this to go *seriatim* into the numerous points which he takes up and illuminates, because the mathematical treatment of the general question does not lend itself easily to oral exposition even to an audience composed of professed mathematicians. There is no doubt but that this paper has put the theory of a rotationally elastic ether—and with it that of a fluid vortex ether—on a sounder basis, and will lead to its discussion and elucidation by a wider circle of investigators.

One further class of physical phenomena yet remains, viz. those of gravitation. The ether must be capable of transmitting gravitational forces as well as electric and optical effects. Does the rotational ether give any promise of doing this? No satisfactory explanation of gravitation on any theory has yet been offered. Perhaps the least unsatisfactory is that depending on the vortex atom theory of matter ("On the Problem of Two Pulsating Spheres in a Fluid," *Proc. Camb. Phil. Soc.*, iii. p. 283), which attributes it to pulsations of hollow vortex atoms. But this necessitates that they should all pulsate with the same period and in the same phase. It is very difficult to conceive how this can happen, unless, as Larmor suggests, all matter is built

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[1] "Primary" refers to the motion as usually understood; "secondary" to the superposed, as explained above.

[1] The necessity that the filaments shall be in pairs does not seem to be recognised. This is, however, essential. Moreover, if the complementary circulations of the filaments between (say) a plate condenser be placed otherwhere than in the same region, the filaments between the plates must rotate as a whole; that is, an electric field would always be combined with a magnetic one.

[2] It would appear that the same results would flow if two particles oppositely electrified—*i.e.* joined by two complementary filaments, as already described—were to rotate round each other.

up of constant elements like his electrons, whose periods are necessarily all alike. It is possible that the vortex cell theory of the ether, of which I have already spoken, may suffice to explain gravitation also. The cells, besides their rotational rigidity, have, in addition, as we saw, a peculiar elasticity of form. To get an idea of how this theory may account for weight, let us suppose the simplest case where all the cells are exactly alike, and the medium is in equilibrium. Now suppose one of the cells begins to grow. It forces the medium away on all sides; the cells will be distorted in some definite way, and a strain set up. Further, this strain will be transmitted from the centre, so that the total amount across any concentric sphere will be the same. Stresses will therefore be set up in the whole medium. If a second cell begins to grow at another place it will produce also a state of strain, the total strain depending on the presence of both. The stresses called into play in the medium will produce a stress between the bodies, but it is questionable whether it would be inversely as the square of the distance. Whether it would be an attraction or repulsion can only be determined by mathematical investigation. The problem is quite determinate, though probably a very difficult one, and would be of mathematical interest quite apart from its physical importance. Since apparently the phenomena of gravitation have no direct interaction with those of light and electricity, whilst the mind rejects the possibility of two different media occupying the same space, we seem driven to look for it in an independent structure of the same medium. Such a structure is already to our hands, with its effects waiting to be determined. It may well be that it may prove to be the cause we are seeking.

The rapid survey I have attempted to make is no doubt a medley of suppositions and inferences combined with some sound deductions. This is the necessary consequence of a prospecting survey in a region whose surface has been merely scratched by pioneers. My object has been to show that this theory of an ether, based on a primitive perfect fluid, is one which shows very promising signs of being able to explain the various physical phenomena of our material universe. Probably, nay certainly, the explanations suggested are not all the true ones. Some will have to be given up, others modified with further knowledge. We cannot proceed to particularise in our secondary hypotheses until we know more about the properties of such media as we have been considering. Every special problem solved in vortex motion puts us in a position to form clearer ideas of what can and what cannot happen. The whole question of vortex aggregates and their interactions is practically untouched, and a rich field is open for mathematical investigation in this portion only of the subject. In all cases, whether a fluid ether is an actual fact or not, the results obtained will be of special interest as types of fluid motion. It is at present a subject in which the mathematicians must lead the attack. I shall have attained my object in choosing this subject for my address, if by it I can induce some of our younger mathematicians to take it up and work out its details.

SECTION B.

CHEMISTRY.

Opening Address by Prof. Raphael Meldola, F.R.S., F.I.C., For. Sec. C.S., President of the Section.

The State of Chemical Science in 1851.

In order to estimate the progress of chemical science since the year 1851, when the British Association last met in this town, it will be of interest for us to endeavour to place ourselves in the position of those who took part in the proceedings of Section B on that occasion. Perhaps the best way of performing this retrograde feat will be to confront the fundamental doctrines of modern chemistry with the state of chemical theory at that period, because at any point in the history of a science the theoretical conceptions in vogue—whether these conceptions have survived to the present time or not—may be taken as the abstract summation of the facts, *i.e.* of the real and tangible knowledge existing at the period chosen as the standard of reference.

Without going too far back in time I may remind you that in 1811 the atomic theory of the chemists was grafted on to the kindred science of physics through the enunciation of the law associated with the name of Avogadro di Quaregna. The rationalising of this law had been accomplished in 1845, but the kinetic theory of gases, which had been foreshadowed by D. Bernoulli in 1738, and in later times by Herapath, Joule, and Krönig, lay buried in the archives of the Royal Society until recently unearthed by Lord Rayleigh and given to the world in 1892 under the authorship of Waterston, the legitimate discoverer. The later developments of this theory did not take place till after the last Ipswich meeting, viz. in 1857–62, by Clausius, and by Clerk Maxwell in 1860–67. Thus the kinetic theory of gases of the physicists had not in 1851 acquired the full significance for chemists which it now possesses: the hypothesis of Avogadro was available, analogous conceptions had been advanced by Davy in 1812, and by Ampère in 1814; but no substantial chemical reasons for its adoption were adduced until the year 1846, when Laurent published his work on the law of even numbers of atoms and the nature of the elements in the free state (*Ann. Chim. Phys.* [3], **18**, 266).

The so-called "New Chemistry" with which students of the present time are familiar was, in fact, being evolved about the period when the British Association last assembled at Ipswich; but it was not till some years later, and then chiefly through the writings of Laurent and Gerhardt, that the modern views became accepted. It is of interest to note in passing that the nomenclature of organic compounds formed the subject of a report by Dr. Daubeny at that meeting in which he says:—"It has struck me as a matter of surprise that none of the British treatises on chemistry with which I am acquainted should contain any rules to guide us, either in affixing names to substances newly discovered or in divining the nature and relations of bodies from the appellations attached to them. Nor do I find this deficiency supplied in a manner which to me appears satisfactory when I turn to the writings of continental chemists." In a subsequent portion of the report Dr. Daubeny adds:—"No name ought, for the sake of convenience, to exceed in length six or seven syllables." I am afraid the requirements of modern organic chemistry have not enabled us to comply with this condition.

Among other physical discoveries which have exerted an important influence on chemical theory the law of Dulong and Petit, indicating the relationship between specific heat and atomic weight, had been announced in 1819, had been subsequently extended to compounds by Neumann, and still later had been placed upon a sure basis by the classical researches of Regnault in 1839. But here, again, it was not till after 1851 that Cannizzaro (1858) gave this law the importance which it now possesses in connection with the determination of atomic weights. Thermo-chemistry as a distinct branch of our science may also be considered to have arisen since 1851, although the foundations were laid before this period by the work of Favre and Silbermann, Andrews, Graham, and especially Hess, whose important generalisation was announced in 1840, and whose claim to just recognition in the history of physical chemistry has been ably advocated in recent times by Ostwald. But the elaboration of thermo-chemical facts and views in the light of the dynamical theory of heat was first commenced in 1853 by Julius Thomsen, and has since been carried on concurrently with the work of Berthelot in the same field which the latter investigator entered in 1865. Electro-chemistry in 1851 was in an equally rudimentary condition Davy had published his electrochemical theory in 1807, and in 1812 Berzelius had put forward those views on electric affinity which became the basis of his dualistic system of formulation. In 1833 Faraday announced his famous law of electro-chemical equivalence, which gave a fatal blow to the conception of Berzelius, and which later (1839–40) was made use of by Daniell in order to show the untenability of the dualistic system. By 1851 the views of Berzelius had been abandoned, and, so far as chemical theory is concerned, the whole subject may be considered to have been in abeyance at that time. It is of interest to note, however, that in that year Williamson advanced on quite distinct grounds his now well-known theory of atomic interchange between molecules, which theory in a more extended form was developed independently from the physical side and applied to electrolytes by Clausius in 1857. The modern theory of electrolysis associated with the names of Arrhenius, van 't Hoff, and Ostwald is of comparatively recent growth. It appears that Hittorf in 1878 was the first to point out the relationship between electrolytic conductivity and chemical activity, this same author as far back as 1856 having combated the prevailing view that the electric current during electrolysis does the work of overcoming the affinities of the ions. Arrhenius formulated his theory of electrolytic dissociation in

1887, Planck having almost simultaneously arrived at similar views on other grounds.

Closely connected with electrolysis is the question of the constitution of solutions, and here again a convergence of work from several distinct fields has led to the creation of a new branch of physical chemistry which may be considered a modern growth. The relationship between the strength of a solution and its freezing point had been discovered by Blagden towards the end of the last century, but in 1851 chemists had no notion that this observation would have any influence on the future development of their science. Another decade elapsed before the law was rediscovered by Rüdorff (1861), and ten years later was further elaborated by de Coppet. Raoult published his first work on the freezing point of solutions in 1882, and two years later the relationship between osmotic pressure and the lowering of freezing point was established by H. de Vries, who first approached the subject as a physiologist, through observations on the cell contents of living plants. As the work done in connection with osmotic pressure has had such an important influence on the "dissociation" theory of solutions, it will be of interest to note that at the last Ipswich meeting Thomas Graham made a communication on liquid diffusion, in which he "gave a view of some of the unpublished results, to ascertain whether solutions of saline bodies had a power of diffusion among liquids, especially water." In 1877 Pfeffer, who, like de Vries, entered the field from the botanical-physiological side, succeeded in effecting the measurement of osmotic pressure. Ten years later van 't Hoff formulated the modern dissociation theory of solution by applying to dissolved substances the laws of Boyle, Gay-Lussac, and Avogadro, the law of osmotic pressure, and Raoult's law connecting the depression of freezing point with molecular weight, thus laying the foundation of a doctrine which, whether destined to survive in its present form or not, has certainly exerted a great influence on contemporary chemical thought.

Consider, further, the state of knowledge in 1851 concerning such leading principles as dissociation or thermolysis, mass action, and chemical equilibrium. Abnormal vapour densities had been observed by Avogadro in 1811, and by Ampère in 1814. Grove had dissociated water vapour by heat in 1847, but the first great advance was made ten years later by Sainte-Claire Deville, from whose work has emanated our existing knowledge of this subject. I may add that the application of this principle to explain the cases of abnormal vapour density was made in 1858 by Kopp, Kekulé, and Cannizzaro almost simultaneously; but, strangely enough, this explanation was not accepted by Deville himself. The subsequent stages are subjects of modern history. The current views on mass action were foreshadowed, as is well known, by Berthollet in his "Statique Chimique," published in 1803, but no great advance had been made when the British Association last met here. The subject first began to assume a quantitative aspect through the researches of Bunsen and Debus in 1853, and was much advanced by Gladstone in 1865 and by Harcourt and Esson a year later. Guldberg and Waage published their classical work on this subject in 1867.

Equally striking will appear the advances made since 1851 if we consider that the whole subject of spectrum analysis, which brings our science into relationship with astronomy, has been called into existence since that date. The celebrated work of Bunsen and Kirchhoff was not published till 1859. Neither can I refrain from reminding you that the coal-tar colour industry, with which I have been to a small extent connected, was started into activity by Perkin's discovery of mauve in 1856; the reaction of this industry on the development of organic chemistry is now too well known to require further mention. In that direction also which brings chemistry into relationship with biology the progress has been so great that it is not going beyond the fact to state that a new science has been created. Pasteur began his studies on fermentation in 1857, and out of that work has arisen the science of bacteriology, with its multifarious and far-reaching consequences. As this chapter of chemical history forms the subject of one of the evening discourses at the present meeting, it is unnecessary to dwell further upon it now. One other generalisation may be chronicled among the great developments achieved since 1851. I refer to the periodic law connecting the atomic weights of the chemical elements with their physical and chemical properties. Attempts to establish numerical relationships in the case of isolated groups of elements had been made by Döbereiner in 1817, by Gmelin in 1826, and again by Döbereiner in 1829. The triad system of grouping was further developed by Dumas in 1851. I am informed by Dr. Gladstone that at the last Ipswich meeting Dumas' speculations in this direction excited much interest. All the later steps of importance have, however, been made since that time, viz. by de Chancourtois in 1862, the "law of octaves" by Newlands in 1864, the periodic law by Mendeléeff, and almost contemporaneously by Lothar Meyer in 1869.

I have been tempted into giving this necessarily fragmentary and possibly tedious historical sketch because it is approaching half a century since the British Association visited this town, and the opportunity seemed favourable for going through that process which in commercial affairs is called "taking stock." The result speaks for itself. Our students of the present time who are nourished intellectually by these doctrines should be made to realise how rapid has been their development. The pioneers of our science, on whose shoulders we stand—and many of whom are happily still among us—will derive satisfaction from the retrospect, and will admit that their labours have borne goodly fruit. It is not, however, simply for the purpose of recording this enormous progress that I have ventured to assume the office of stock-taker. The year 1851 may be regarded as occurring towards the close of one epoch and the dawn of a new era in chemical history. Consider broadly the state of organic chemistry at that time. There is no occasion for going into detail, even if time admitted, because our literature has recently been enriched by the concise and excellent historical works of Schorlemmer and of Ernst von Meyer. It will suffice to mention that the work and writings of Liebig, Berzelius, Wöhler, Dumas, Gay-Lussac, Bunsen, and others had given us the leading ideas of isomerism, substitution, compound radicles, and types. Wurtz and Hofmann had just discovered the organic ammonias; Williamson that same year made known his celebrated work on the ethers; and Gerhardt discovered the acid anhydrides a year later. The newer theory of type was undergoing development by Gerhardt and his followers; the mature results were published in the fourth volume of the "Traité de Chimie" in 1856. In this country the theory was much advanced by the writings of Odling and Williamson.

SUBSEQUENT DEVELOPMENT OF CHEMISTRY ALONG TWO LINES.

The new era which was dawning upon us in 1851 was that of structural or constitutional chemistry, based on the doctrine of the valency of the atoms. It is well known that this conception was broached by Frankland in 1852, as the result of his investigations on the organo-metallic compounds. But it was not till 1858 that Kekulé, who had previously done much to develop the theory of types, and Couper, almost simultaneously, recognised the quadrivalent character of carbon. To attempt to give anything approaching an adequate notion of the subsequent influence of this idea on the progress of organic chemistry would be tantamount to reviewing the present condition of that subject. I imagine that no conception more prolific of results has ever been introduced into any department of science. If we glance back along the stream it will be seen that shortly after the last meeting here the course of discovery began to concentrate itself into two channels. In one we now find the results of the confluent labours of those who have regarded our science from its physical side. In the other channel is flowing the tide of discovery arising from the valency doctrine and its extension to the structure of chemical molecules. The two channels are at present fairly parallel and not far apart; an occasional explorer endeavours now and again to make a cross-cut so as to put the streams into communication. The currents in both are running very rapidly, and the worker who has embarked on one or the other finds himself hurried along at such a pace that there is hardly breathing time to step ashore and see what his neighbours are doing. It speaks well for the fertility of the conception of valency that the current in this channel is flowing with unabated vigour, although its catchment area—to pursue the metaphor—is by no means so extensive as that of the neighbouring stream.

The modern tendency to specialisation, which is a necessity arising from the large number of workers and the rapid multiplication of results, is apparently in the two directions indicated. We have one class of workers dealing with the physics of matter in relation to general chemical properties, and another class of investigators concerning themselves with the special properties of individual compounds and classes of compounds—with atomic idiosyncrasies. The workers of one class are differentiating while their colleagues are integrating. It would be nothing less than unscientific to institute a comparison between the relative merits of the two methods; both are necessary for the development of our science. All methods of attacking the unknown are equally welcomed. In some cases physical methods are available, in other cases purely chemical methods have alone been found of use. There is no antagonism, but co-operation. If the results of the two methods are sometimes at variance it is simply because we have not known how to interpret them. The physical chemist has adopted the results of the application of chemical methods of determining "constitution," and is endeavouring to furnish us with new weapons for attacking this same problem. The chemist who is seeking to unravel the architecture of molecules is dependent at the outset upon physical methods of determining the relative weights of his molecules. The worker who is bringing about new atomic groupings is furnishing material for the further development of generalisations from which new methods applicable to the problem of chemical structure may again be evolved. The physical chemist sometimes from the broadness of his view is apt to overlook or to minimise the importance of chemical individuality. On the other hand the chemist who is studying the numberless potentialities of combination resident in the atoms, and who has grasped to the full extent their marvellous individualities, is equally liable to forget that there are connecting relationships as well as specific differences in the properties of elements and compounds. These are but the mental traits—the unconscious bias engendered by the necessary specialisation of work to which I have referred, and which is observable in every department of scientific labour.

THE PRESENT STATE OF STRUCTURAL CHEMISTRY.

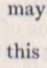
The success attending the application of the doctrine of valency to the compounds of carbon has helped its extension to all compounds formed by other elements, and the student of the present day is taught to use structural formulæ as the A B C of his science. It is, I think, generally recognised among chemists that this doctrine in its present state is empirical, but it does not appear to me that this point is sufficiently insisted upon in chemical teaching. I do not mean to assert that for the last thirty years chemists have been pursuing a phantom; neither do I think that we should be justified in applying to this doctrine the words applied to its forerunner, the "types" of Gerhardt, by Lothar Meyer, who says that these "have rendered great service in the development of the science, but they can only be regarded as a part of the scaffolding which was removed when the erection of the system of organic chemistry had made sufficient progress to be able to dispense with it" ("Modern Theories of Chemistry," p. 194.) It appears to me, on the contrary, that there is a physical reality underlying the conception of valency, if for no other reason because of the conformability of this property of the atoms to the periodic law. But the doctrine as it stands is empirical in so far that it is only representative and not explanatory. Frankland and Kekulé have given us a great truth, but its very success is now making it more and more obvious that it is a truth which is pressing for further development from the physical side. If we are asked why CO exists, and why CH_2 and CCl_2 do not, together with innumerable similar questions which the inquisitive mind will raise, we get no light from this doctrine. If any over-sanguine disciple goes so far as to assert that all the possible compounds of the elements indicated by their valency are capable of existence, and will sooner or later be prepared, he will, I imagine, find himself rapidly travelling away from the region of fact.

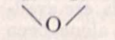
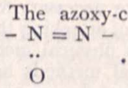
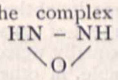
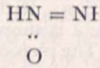
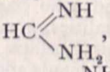
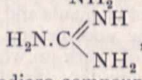
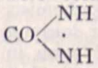
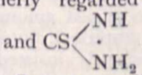
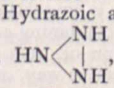
There is something to be reckoned with besides valency. The one great desideratum of modern chemistry is unquestionably a physical or mechanical interpretation of the combining capacities of the atoms. Attempts at the construction of such theories have been made, and thus far only in a tentative way, and these views cannot be said to have yet come within the domain of practical chemical politics. I have in mind, among other suggestions, the dynamical theory of van 't Hoff published in 1881 ("Ansichten über die organische Chemie"), the theory of electric charges on the atoms broached by Johnstone Stoney in 1874, and so ably advocated by the late Prof. v. Helmholtz in his Faraday lecture in 1881, and the electric polar theory of Victor Meyer and Riecke, published in 1888 ("Einige Bemerkungen über den Kohlenstoffatom und die Valenz," *Ber.*, 21, pp. 946, 1620).

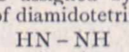
Pending the rationalisation of the doctrine of valency its promulgation must continue in its present form. Its services in the construction of rational formulæ, especially within the limits of isomerism, have been incalculable. It is the ladder by which we have climbed to the present brilliant achievements in chemica synthesis, and we are not in a position to perform the ungracious task of kicking it away. In recalling attention to its weaknesses I am only putting myself in the position of th physician who diagnoses his patient's case with the ulterior object of getting him strengthened. There can be no doubt that renewed vitality has been given to the doctrine by the conceptions of tautomerism and desmotropy, formulated by Conrad Laar in 1885, and by Paul Jacobson in 1887. The importance of these ideas is becoming more evident with the advancement of chemical discovery. Any attempt to break down the rigidly statical conception of our structural formulæ appears to me to be a step in the right direction. Then, again, I will remind you of the prolific development of the doctrine in the hands of Le Be and van 't Hoff by the introduction of the stereochemical hypothesis in 1874—unquestionably the greatest advance in structural chemistry since the recognition of the quadrivalent character of the carbon atom. If evidence be required that there is a physical reality underlying the conception of valency, we need only point to the close accordance of this notion of the asymmetric carbon atom with the facts of so-called "physical isomerism" and the splendid results that have followed from its introduction into our science, especially in the field of carbohydrates through the investigations of Emil Fischer and his pupils. In other directions the stereochemical hypothesis has proved to be a most suggestive guide. It was applied by Prof. v. Baeyer in 1885 (*Ber.*, 18, 2277) to explain the conditions of stability or instability of certain atomic groupings, such as the explosiveness of polyacetylene compounds and the stability of penta- and hexa-cyclic systems. Again, in 1888 this eminent chemist showed its fertility in a series of brilliant researches upon benzene derivatives (*Ann.*, **137**, 158, and subsequent papers). Nor can I omit to mention the great impetus given in this field by the classical work of Wislicenus, who in 1887 applied the hypothesis to unsaturated compounds and to cyclic systems with remarkable success ("Ueber die räumliche Anordnung der Atome in organischen Molekulen," &c.). Quite recently Victor Meyer and J. Sudborough have shown that the ability of certain derivatives of benzoic and naphthoic acids to form ethers is governed by stereochemical considerations (*Ber.*, **27**, 510, 1580, 3146, and **28**, 182, 1254). But I must avoid the temptation to enlarge upon this theme because the whole subject has been recently brought together by C. A. Bischoff in his "Handbuch der Stereochemie" (Frankfurt, 1893–94), a work to which all who are interested in the subject will naturally turn for reference.

While the present advanced state of structural chemistry may thus be looked upon as the outcome of the conceptions of Frankland and Kekulé, it may be well to bear in mind that the idea of structure is not *necessarily* bound up with the hypothesis of valency in its present form. Indeed, some advance had been made in representing "constitution," especially by Kolbe, before the formal introduction of this hypothesis. The two ideas have grown up together, but the experimental evidence that in any molecule the atoms are grouped together in a particular way is really independent of any theory of valency. It is only after this evidence has been acquired, either by analysis or synthesis, that we proceed to apply the hypothesis in building up the structural formula. It is of course legitimate to assume the truth of the hypothesis, and to endeavour by its use to convert an empirical into a rational formula; but this method generally gives us a choice of formulæ from which the true one can only be selected by further experimental investigation. Even within the narrower limits of isomerism it is by no means certain that all the modifications of a compound indicated by hypothesis are actually capable of existence. There is, for example, evidence that some of the "position isomerides" among the derivatives of mono- and poly-cyclic compounds are too unstable to exist; a fact which in itself is sufficient to indicate the necessity for a revision and extension of our notions of valency. Thus, by way of illustration, there is nothing in the hypothesis to indicate why orthoquinones of the benzene series should not be capable of existence; yet it is a fact that in spite of all efforts such compounds have never been obtained. The conditions essential for the existence of these compounds appear to be that the hydrogen of the benzene ring should be replaced by acid substituents such as oxygen, hydroxyl, chlorine, or bromine. Under these circumstances, as Zincke has shown (*Ber.*, **20**, 1776), tetrachlor and tetrabrom-orthobenzoquinone are stable compounds. So also the interesting researches of Nietzki have proved that in such a compound as rhodizonic acid (*Ibid.*, **19**, 308, and **23**, 3136) orthoquinone oxygen atoms are present. But there is nothing in the doctrine of valency which leads us to suspect that these orthoquinone derivatives can exist while their parent compound resists all attempts at isolation. I am aware that it is dangerous to argue from negative evidence, and it would be rash to assert that these orthoquinones will never be obtained. But even in the present state of knowledge it may be distinctly affirmed that the methods which readily furnish an orthoquinone of naphthalene completely fail in the case of benzene, and it is just on such points as this that the inadequacy of the hypothesis becomes apparent. In other words, the doctrine fails in the fundamental requirement of a scientific theory; in its present form it gives us no power of prevision—it hints at possibilities of atomic groupings, but it does not tell us *à priori* which of these groupings are likely to be stable and which unstable. I am not without hope that the next great advance in the required direction may yet come from the stereochemical extension of the hypothesis, although the attempts which have hitherto been made to supply its deficiencies cannot but be regarded as more or less tentative.

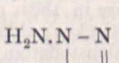
THE NEW THEORY OF ABSTRACT TYPES.

I will venture, in the next place, to direct attention to a modern development of structural chemistry which will help to illustrate still further some of the points raised. For many years we have been in the habit of abstracting from our structural formulæ certain ideal complexes of atoms which we consider to represent the nucleus or type from which the compound of known constitution is derived. In other words, the hypothesis of valency which was developed originally from Gerhardt's types is now leading us back to another theory of types based upon a more intimate knowledge of atomic grouping within the molecule. In some cases these types have been shown to be capable of existence; in others they are still ideal. Used in this way the doctrine of valency is most suggestive, but at the same time its lack of prevision is constantly forcing itself upon the attention of chemical investigators. The parent compound has sometimes been known before its derivatives, as in the case of ammonia, which was known long before the organic amines and amides. In other instances the derivatives were obtained before the type was isolated, as in the case of the hydrazines, which were characterised by Emil Fischer in 1875, and the hydrazo-compounds, which have been known since 1863, while hydrazine itself was first obtained by Curtius in 1887. Phenylazimide was discovered by Griess in 1864, and many representatives of this group have been since prepared; but the parent compound, hydrazoic acid, was only isolated by Curtius in 1890. Derivatives of triazole and tetrazole were obtained by Bladin in 1885; the types were isolated by this chemist and by Andreocci in 1892, Pyrazole derivatives were prepared by Knorr in 1883; pyrazole itself was not isolated till 1889, by Buchner. Alkyl nitramides were discovered by Franchimont and Klobbie many years before the typical compound, nitramide, $NO_2.NH_2$, which was isolated last year by Thiele and Lachman (*Ber.*, 27, 1909). Examples might be multiplied to a formidable extent, but enough have been given to illustrate the principle of the erection of types, which were at first imaginary, but which have since become real. The utility of the hypothesis is undeniable in these cases, and we are justified in pushing it to its extreme limits. But no chemist, even if endowed with prophetic instinct, could have certainly foretold six years ago that the type of Griess' "triazobenzene" would be capable of free existence, and still less that when obtained it would prove to be a strong acid. The fact, established by Curtius, that the group  $\mathrm{N}\!\diagup\!\!\diagdown\mathrm{N}$-functions in chemical molecules like the atom of chlorine is certainly among the most striking of recent discoveries. Only last year the list of nitrogen compounds was enriched by the addition of $CO(N_3)_2$, the nitrogen analogue of phosgene (Curtius, *Ber.*, 27, 2684).

These illustrations, drawn from the compounds of nitrogen, will serve to bring out the wonderful development which our knowledge of the chemistry of this element has undergone within the last few years. I might be tempted here into a digression on the general bearing of the very striking fact that an element comparatively inactive in the free state should be so remarkably active in combination, but I must keep to the main topic, as by means of these compounds it is possible to illustrate still further both the strength and the weakness of our modern conceptions of chemical structure. Consider some of the undiscovered compounds which are foreshadowed by the process of ideal abstraction of types. The azoxy-compounds contain the complex  $\underset{\diagdown\mathrm{O}\diagup}{-\mathrm{N}-\mathrm{N}-}$ or  $\underset{\mathrm{O}}{\overset{-\mathrm{N}=\mathrm{N}-}{\cdot\cdot}}$. The types would be  $\underset{\diagdown\mathrm{O}\diagup}{\mathrm{HN}-\mathrm{NH}}$ or  $\underset{\mathrm{O}}{\overset{\mathrm{HN}=\mathrm{NH}}{\cdot\cdot}}$. The first of these formulæ represents the unknown dihydro-nitrous oxide. The azo-compounds are derivatives of the hypothetical diimide HN : NH. An attempt to prepare this compound from azodicarbonic acid (Thiele, *Ann.*, 271, 130) resulted in the formation of hydrazine. The diethyl-derivative may have been obtained by Harries (*Ber.*, 27, 2276), but this is doubtful. It is at present inexplicable why compounds in which the group · N : N · is in combination with aromatic radicles should be so remarkably stable, while the parent compound appears to be incapable of existence. The addition of two atoms of hydrogen converts this type again into a stable compound. There is nothing in the structural formulæ to indicate these facts. The amidines are stable compounds, and the so-called "anhydro-bases," or imidazoles, are remarkably stable; the parent compound  $\mathrm{HC}\begin{cases}\!=\mathrm{NH}\\ -\mathrm{NH_2}\end{cases}$, has not been obtained, while its amido-derivative,  $\mathrm{H_2N.C}\begin{cases}\!=\mathrm{NH}\\ -\mathrm{NH_2}\end{cases}$, is the well-known substance guanidine. The isodiazo-compounds recently discovered by Schraube and Schmidt and by Bamberger (*Ibid.*, 27, 514, 679, &c.) are possibly derivatives of the hypothetical substance $O:N.NH_2$, which might be named nitrosamide. Why this compound should not exist as well as nitramide is another question raised by the principle of abstract types. The carbizines were formerly regarded as derivatives of the compounds  $\mathrm{CO}\begin{cases}\mathrm{NH}\\ \cdot\\ \mathrm{NH}\end{cases}$ and  $\mathrm{CS}\begin{cases}\mathrm{NH}\\ \cdot\\ \mathrm{NH_2}\end{cases}$ (Fischer, *Ann.*, 212, 326; Freund and Goldsmith, *Ber.*, 21, 2456). Although this structure has now been disproved the possible existence of the types has been suggested. Carbizine and thiocarbizine differ from urea and thiocarbamide only by two atoms of hydrogen. These types have not been isolated; if they are incapable of existence the current views of molecular structure give no suggestion of a reason. The diazoamides are derivatives of the hypothetical $H_2N.NH.NH_2$ or $HN:N.NH_2$, compounds which Curtius speaks of as the propane and propylene of the nitrogen series. The latter complex was at one time thought to exist in diazohippuramide (*Ber.*, 24, 3342. This has since been shown to be hippurazide, *i.e.* a derivative of N_3H, *Ber.*, 27, 779), and a biacidyl derivative of the former type has also been obtained (*Ibid.*, 3344). Both these types await isolation if they are capable of existence. I may add that several attempts to convert diazoamides into dihydro-derivatives by mild alkaline reduction have led me to doubt whether this nitrogen chain can exist in combination with hydrocarbon radicles. The bisdiazoamides of H. v. Pechmann and Frobenius (*Ber.*, 27, 898) are derivatives of the 5-atom chain $H_2N.NH.NH.NH.NH_2$ or $HN:N.NH.N:NH$, a type which hardly seems likely to be of sufficient stability to exist. The tetrazones of Emil Fischer have for their type the 4-atom chain $H_2N.N:N.NH_2$ or $H_2N.NH.NH.NH_2$, of which the free existence is equally problematical, although a derivative containing the chain $-N:N.NH.NH-$ has been obtained by Curtius (*Ibid.*, 26, 1263). Hydrazoic acid may be regarded as a derivative of triimide,  $\mathrm{HN}\begin{cases}\mathrm{NH}\\ \;|\\ \mathrm{NH}\end{cases}$, but this type appears to be also incapable of isolation (Curtius, *Ber.*, 26, 407). The hydrazidines or formazyls of Pinner (*Ber.*, 17, 182) and of H. v. Pechmann (*Ibid.*, 25, 3175), have for their parent compound the hypothetical substance $H_2N.N:CH.N:NH$. In 1888 Limpricht described certain azo-compounds (*Ibid.*, 21, 3422) which, if possessing the structure assigned by that author, must be regarded as derivatives of diamidotetrimide:



$$\begin{array}{c}\mathrm{HN}-\mathrm{NH}\\ |\quad\;\;|\\ \mathrm{HN}-\mathrm{NH}\end{array}$$

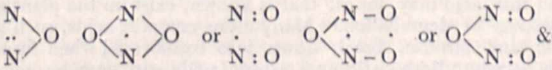


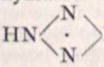
$$\begin{array}{r}\mathrm{H_2N.N}-\mathrm{N}\\ |\quad\;\;\|\\ \mathrm{H_2N.N}-\mathrm{N}\end{array}$$

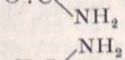
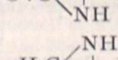
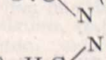
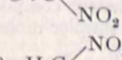
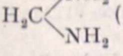
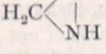
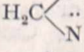
Both these types are at present imaginary; whether it is possible for cyclic nitrogen systems to exist we have no means of knowing—all that can be said is that they have never yet been obtained. It is possible, as I pointed out in 1890 at the Leeds meeting of the British Association, that mixed diazoamides may be derivatives of such a 4-atom ring.

Any chemist who has followed the later developments of the chemistry of nitrogen could supply numerous other instances of undiscovered types. A chapter on the unknown compounds of this element would furnish quite an exciting addition to many of those books which are turned out at the present time in such profusion to meet the requirements of this or that examining body. I have selected my examples from these compounds simply because I can claim some of them as personal acquaintances. It would be easy to make use of carbon compounds for the same purpose, but it is unnecessary to multiply details. It has frequently happened in the history of science that a well-considered statement of the shortcomings of a theory has led to its much-desired extension. This is my hope in venturing to point out one of the chief deficiencies in the structural chemistry of the present time. I am afraid that I have handled the case badly, but I am bound to confess that I am influenced by the same feelings as those which prevent us from judging an old and well-tried friend too severely.

The theory of types to which we have reverted as the outcome of the study of molecular structure is capable of almost indefinite extension if, as there is good reason for doing, we replace atoms or groups by their valency analogues in the way of other atoms or groups of atoms. The facts that in cyclic systems N can replace CH (benzene and pyridine), that O, S, and NH are analogues in furfurane, thiophene, and pyrrole, are among the most familiar examples. The remarkable iodo- and iodoso-compounds recently discovered by Victor Meyer and his colleagues are the first known instances in which the trivalent atom of iodine has been shown to be the valency analogue of nitrogen in organic combination. Pushing this principle to the extreme we get further suggestions for new groupings, but, as before, no certainty of prevision. Thus, if nitrogen formed the oxide N_2O_2 the series might be written:



Of course these formulæ are more or less conjectural, being based on valency only. But since nitrous oxide is the analogue of hydrazoic acid, they hint at the possibility of such compounds as , &c. If a student produced a set of formulæ corresponding to the above, in which NH had been substituted for O, and asked whether they did not indicate the existence of a whole series of unknown hydrogen compounds of nitrogen, we should probably tell him that his notions of chemical structure had run wild. At the same time I am bound to admit that it would be very difficult, if not impossible, to furnish him with satisfactory reasons for believing that such groupings are improbable. Compare again the series:


(1)

(2)

(3)

(4)

(5)

(6)

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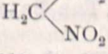
(8)

The first is urea; the second, third, fourth, fifth (methylene diamine), and sixth are unknown; the seventh is the remarkably interesting diazomethane discovered last year by H. v. Pechmann (*Ber.*, **27**, 1888). The last compound, dinitromethane, is known in the form of its salts, but appears to be incapable of existence in the free state. There is nothing expressed or implied in the existing theory of chemical structure to explain why dinitromethane is unstable while trinitromethane is stable, and mono- and tetranitromethane so stable as to admit of being distilled without decomposition. Chemists will form their own views as to the possibility or impossibility of such a series as this being completed. Whether there would be a concordance of opinion I will not venture to say; but any chemist who expressed either belief or disbelief with regard to any special member would, I imagine, have great difficulty in giving a scientific reason for the faith which is in him. At the most, he would have only the very unsafe guide of analogy to fall back upon. Perhaps by the time the British Association holds its next meeting at Ipswich it will have become possible to prove that one particular configuration of certain atoms is possible and another configuration impossible. Then will have been achieved that great advance for which we are waiting—the reunion of the two streams into which our science began to diverge shortly after the last Ipswich meeting.

The present position of structural chemistry may be summed up in the statement that we have gained an enormous insight into the anatomy of molecules, while our knowledge of their physiology is as yet in a rudimentary condition. In the course of the foregoing remarks I have endeavoured to indicate the direction in which our theoretical conceptions are most urgently pressing for extension. It is, perhaps, as yet premature to pronounce an opinion as to whether the next development is to be looked for from the stereochemical side; but it is not going too far to express once again the hope that the geometrical representation of valency will give us a deeper insight into the conditions which determine the stability of atomic configurations. The speculations of A. v. Baeyer, Wislicenus, Victor Meyer, Wunderlich, Bischoff, and others have certainly turned the attention of chemists towards a quarter from which a new light may eventually dawn.

THE PROGRESS OF SYNTHETICAL CHEMISTRY.

If, in my earnest desire to see the foundations of structural chemistry made more secure, I may have unwittingly given rise to the impression that I am depreciating its services as a scientific weapon, let me at once hasten to make amends by directing attention to the greatest of its triumphs, the synthesis of natural products, *i.e.* of compounds which are known to be produced by the vital processes of animals and plants.

Having been unable to find any recent list of the natural compounds which have been synthesised, I have compiled a set of tables which will, I hope, see the light at no very distant period. According to this census we have now realised about 180 such syntheses. The products of bacteria have been included in the list because these compounds are the results of vital activity in the same sense that alcohol is a product of the vital activity of the yeast plant. On the other hand the various uro-compounds resulting from the transformation in the animal economy of definite chemical substances administered for experimental purposes have been excluded, because I am confining my attention to natural products. Of course the importance of tracing the action of the living organism on compounds of known constitution from the physiological point of view cannot be overestimated. Such experiments will, without doubt, in time shed much light on the working of the vital laboratory.

The history of chemical synthesis has been so thoroughly dealt with from time to time that I should not have ventured to obtrude any further notice of this subject upon your patience were it not for a certain point which appeared to me of sufficient interest to merit reconsideration. It is generally stated that the formation of urea from ammonium cyanate by Wöhler in 1828 was the first synthesis of an organic compound. There can be no doubt that this discovery, which attracted much attention at the time, gave a serious blow to the current conceptions of organic chemistry, because urea was so obviously a product of the living animal. It will be found, however, that about the same time Henry Hennell, of Apothecaries' Hall, had really effected the synthesis of alcohol—that is to say, had synthesised this compound in the same sense that Wöhler had synthesised urea. The history is soon told. In 1826 Hennell (through Brande) communicated a paper to the Royal Society which appears in the *Philosophical Transactions* for that year.[1] In studying the compounds produced by the action of sulphuric acid on alcohol, and known as "oil of wine," he obtained sulphovinic acid, which had long been known, and gave fairly good analyses of this acid and of some of its salts, while expressing in the same paper very clear notions as to its chemical nature. Having satisfied himself that sulphovinic acid is a product of the action in question, he then proceeded to examine some sulphuric acid which had absorbed eighty times its volume of olefiant gas, and which had been placed at his disposal for this purpose by Michael Faraday. From this he also isolated sulphovinic acid. In another paper, communicated to the Royal Society in 1828,[2] he proves quantitatively that when sulphovinic acid is distilled with sulphuric acid and water the whole of the alcohol and sul-

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[1] "On the Mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the resulting compound," *Phil. Trans.*, 1826, p 240.

[2] "On the Mutual Action of Sulphuric Acid and Alcohol, and on the Nature of the Process by which Ether is formed," *Phil. Trans.*, 1828, p. 365.

phuric acid which united to form the sulphovinic acid are recovered. In the same paper he shows that he had very clear views as to the process of etherification. Hennell's work appears to have been somewhat dimmed by the brilliancy of his contemporaries who were labouring in the same field; but it is not too much to claim for him, after the lapse of nearly seventy years, the position of one of the pioneers of chemical synthesis. Of course in his time the synthesis was not complete, because he did not start from inorganic materials. The olefiant gas used by Faraday had been obtained from coal-gas or oil-gas. Moreover, in 1826–1828 alcohol was not generally regarded as a product of vital activity, and this is, no doubt, the reason why the discovery failed to produce the same excitement as the formation of urea. But the synthesis of alcohol from ethylene had, nevertheless, been accomplished, and this hydrocarbon occupied at that time precisely the same position as ammonium cyanate. The latter salt had not then been synthesised from inorganic materials, and the formation of urea, as Schorlemmer points out ("The Rise and Development of Organic Chemistry," p. 195), was also not a complete synthesis. The reputation of Wöhler, the illustrious friend and colleague of the more illustrious Liebig, will lose not a fraction of its brilliancy by the raising of this historical question. Science recognises no distinction of nationality, and the future historian of synthetical chemistry will not begrudge the small niche in the temple of fame to which Hennell is entitled.

Like many other great discoveries in science, the artificial formation of natural products began, as in the case of alcohol and urea, with observations arising from experiments not primarily directed to this end. It was not till the theory of chemical structure had risen to the rank of a scientific guide that the more complicated syntheses were rendered possible by more exact methods. We justly credit structural chemistry with these triumphant achievements. In arriving at such results any defects in the theory of structure are put out of consideration because—and this point must never be lost sight of—all doubt as to the possibility of this or that atomic grouping being stable is set aside at the outset by the actual occurrence of the compound in nature. The investigator starts with the best of all assurances. From the time of Wöhler and Hennell the course of discovery in this field has gone steadily on. The announcement of a new synthesis has ceased to produce that excitement which it did in the early days when the so-called "organic" compounds were regarded as products of a special vital force. The interest among the uninitiated now rises in proportion to the technical value of the compound. The present list of 180 odd synthetical products comprises, among the latest discoveries, gentisin, the colouring-matter of the gentian root (*Gentiana lutea*), which has been prepared by Kostanecki and Tambor, and caffeine, synthesised by Emil Fischer and Lorenz Ach, starting from dimethylurea and malonic acid.

I have allowed myself no time for those prophetic flights of the imagination which writers on this subject generally indulge in. When we know more about the structure of highly complex molecules, such as starch and albumin, we shall probably be able to synthesise these compounds. It seems to me more important just at present to come to an understanding as to what is meant by an organic synthesis. There appears to be an impression among many chemists that a synthesis is only effected when a compound is built up from simpler molecules. If the simpler molecules can be formed directly from their elements, then the synthesis is considered to be complete. Thus urea is a complete synthetical product, because we can make hydrogen cyanide from its elements; from this we can prepare a cyanate, and finally urea. In dictionaries and text-books we find synthetical processes generally separated from modes of formation, and the latter in their turn kept distinct from methods of preparation. The distinction between formation and preparation is obviously a good one, because the latter has a practical significance for the investigator. But the experience gained in drawing up the tables of synthesised compounds, to which I have referred, has resulted in the conclusion that the terms "synthesis" and "mode of formation" have been either unnecessarily confused or kept distinct without sufficient reason, and that it is impossible now to draw a hard-and-fast line between them. Some recent writers, such, for example, as Dr. Karl Elbs, in his admirable work on this subject ("Die synthetischen Darstellungsmethoden der Kohlenstoffverbindungen," Leipzig, 1889), have expanded the meaning of the word synthesis so as to comprise generally the building up of organic molecules by the combination of carbon with carbon, without reference to the circumstance whether the compound occurs as a natural product or not. But although this definition is sufficiently wide to cover the whole field of the production of carbon compounds from less complex molecules, it is in some respects too restricted, because it excludes such well-known cases as the formation of hydrogen cyanide from its elements, or of urea from ammonium cyanate. I should not consider the discussion of a mere question of terminology of sufficient importance to occupy the attention of this Section were it not for a matter of principle, and that a principle of the very greatest importance, which I believe to be associated with a clear conception of chemical synthesis. The great interest of all work in this field arises from our being able, by laboratory processes, to obtain compounds which are also manufactured in nature's laboratory—the living organism. It is in this direction that our science encroaches upon biology through physiology. Now, if we confine the notion of synthesis to the building up of molecules from simpler molecules or from atoms, we exclude one of nature's methods of producing many of these very compounds which we claim to have synthesised. There can be no manner of doubt that a large proportion, if not a majority, of the natural products which have been prepared artificially are not synthesised by the animal or plant in the sense of building up at all. They are the results of the breaking down—of the degradation—of complex molecules into simpler ones. I urge, therefore, that if in the laboratory we can arrive at one of these products by decomposing a more complex molecule by means of suitable reagents, we have a perfect right to call this a synthesis, provided always that the more complex molecule, which gives us our compound, can be in its turn synthesised, by no matter how many steps, from its constituent atoms. Thus oxalic acid has been directly synthesised from carbon dioxide by Kolbe and Drechsel by passing this gas over potassium or sodium amalgam heated to 360°. Whether the plant makes oxalic acid directly out of carbon dioxide we cannot at present state; if it does it certainly does not employ Kolbe and Drechsel's process. On the other hand this acid may, for all that is known, exist in the plant as a product of degradation. Many more complex acids, such as citric and tartaric, break down into oxalic acid when fused with potash. Both citric and tartaric acids can now be completely synthesised; therefore the formation of oxalic acid from these by potash fusion is a true synthesis.

The illustration given will make clear the point which I am urging. The distinction between a synthesis and a mode of formation vanishes when we can obtain a compound by the breaking down of a more complex molecule in all those cases where the latter can be completely built up. If we do not expand the meaning of synthesis so as to comprise such cases we are simply shutting the door in nature's face. It must be borne in mind that the actual yield of the compound furnished by the laboratory process does not come into consideration, because it may be generally asserted that in most cases the artifical processes are not the same as those which go on in the animal or plant. The information of real value to the physiologist which these syntheses give is the suggestion that such or such a compound may possibly result from the degradation of this or that antecedent compound, and not from a process of building up from simpler molecules.

THE BEARING OF CHEMICAL SYNTHESIS ON VITAL CHEMISTRY.

With these views—the outcome of structural chemistry—the chemist and physiologist may join hands and move fearlessly onwards towards the great mystery of vital chemistry. In considering the results of organic synthesis two questions always arise as it were spontaneously: How does nature produce these complicated molecules without the use of strong reagents and at ordinary temperatures? What bearing have our laboratory achievements on the mechanism of vitality? The light shed upon these questions by experimental investigation has as yet flickered only in fitful gleams. We are but dwellers in the outer gates, waiting for the guide who is to show us the bearing of modern research on the great problem which confronts alike the physicist, the chemist, and the biologist. The chemical processes that go on in the living organism are complex to an extent that is difficult to realise. Of the various compounds of animal or vegetable origin that have been produced synthetically some are of the nature of waste products, resulting from metabolic degradation; others are the result of zymolytic action within the organism; and others, again, are secondary products arising from the action of associated bacteria, the relationship between the bacteria and their host being as yet imperfectly understood. The answer to the question how nature produces complicated organic molecules will be much facilitated when the physiologist, by experiment and observation, shall have made possible a sound classification of these synthetical products based on their mode of origination in the organism.

The enlargement of the definition of organic synthesis which I have advocated has been rendered necessary by the consideration of certain questions which have arisen in connection with the present condition of chemical discovery in this field. What evidence is there that any one of the 180 compounds which have been prepared artificially is produced in the organism by a direct process of building up? Is not the opposite view quite as probable? May they not, from the simplest to the most complex, be products of the degradation of still more complex molecules? I venture to suggest—not without some temerity lest our colleagues of Sections I and K should treat me as an intruder—that this view should be given a fair trial. I am aware that the opposite view, especially as regards plant assimilation, has long been held, and especially since 1870, when v. Baeyer advanced his celebrated theory of the formic aldehyde origin of carbohydrates. It is but natural to consider that the formation of a complex molecule is the result of a building-up process. It must be remembered, however, that in the living organism there is always present a compound or mixture, or whatever we like to call it, of a highly complex proteid nature, which, although at present indefinite from the purely chemical point of view, is the essence of the vitality. Of course I refer to what biologists have called protoplasm. Moreover, it is perhaps necessary to state what is really nothing more than a truism, viz. that protoplasm is present in and forms a part of the organism from the very beginning of its existence—from the germ to the adult, and onwards to the end of life. Any special chemical properties pertaining to protoplasm are inseparable from the animal or plant until that period arrives which Kekulé has hinted at when we shall be able to "build up the formative elements of living organisms" in the laboratory (NATURE, vol. xviii. p. 212). But here I am afraid I am allowing the imagination to take a flight which I told you a few minutes ago that time would not admit of.

The view that requires pushing forward into a more prominent position than it has hitherto occupied is that all the chemical transformations in the organism—at any rate all the primary changes—are made possible only by the antecedent combination of the substances concerned with living protoplasmic materials. The carbon dioxide, water, &c., which the plant absorbs must have formed a compound or compounds with the protoplasmic material of the chloroplasts before starch, or sugar, or cellulose can be prepared. There is, on this view, no such process as *the direct combination* of dead molecules to build up a complex substance. Everything must pass through the vital mill. The protoplasmic molecule is vastly more complex than any of the compounds which we have hitherto succeeded in synthesising. It might take up and form new and unstable compounds with carbon dioxide or formic aldehyde, or sugar, or anything else, and our present methods of investigation would fail to reveal the process. If this previous combination and, so to speak, vitalisation of dead matter actually occurs, the appearance of starch as the first visible product of assimilation, as taught by Sachs, or the formation of a 12-carbon-atom sugar as the first carbohydrate, as shown by the recent researches of Horace Brown and G. H. Morris, is no longer matter for wonderment. The chemical equations given in physiological works are too purely chemical; the physiologists have, I am afraid, credited the chemists with too much knowledge—it would appear as though their intimate familiarity with vital processes had led them to undervalue the importance of their prime agent. In giving expression to these thoughts I cannot but feel that I am treating you to the strange spectacle of a chemist pleading from the physiologists for a little more vitality in the chemical functions of living organisms. The future development of vital chemistry rests, however, with the chemist and physiologists conjointly; the isolation, identification, and analysis of the products of vital activity, which has hitherto been the task of the chemist, is only the preliminary work of physiological chemistry leading up to chemical physiology.

PROTOPLASMIC THEORY OF VITAL SYNTHESIS.

The supposition that chemical synthesis in the organism is the result of the combination of highly complex molecules with simpler molecules, and that the unstable compounds thus formed then undergo decomposition with the formation of new products, may be provisionally called the protoplasmic theory of vital synthesis. From this standpoint many of the prevailing doctrines will have to be inverted, and the formation of the more complex molecules will be considered to precede the synthesis of the less complex. It may be urged that this view simply throws back the process of vital synthesis one stage and leaves the question of the origin of the most complex molecules still unexplained. I grant this at once; but in doing so I am simply acknowledging that we have not yet solved the enigma of life. We are in precisely the same position as is the biologist with respect to abiogenesis, or the so-called "spontaneous generation." To avoid possible misconception let me here state that the protoplasmic theory in no way necessitates the assumption of a special "vital force." All that is claimed is a peculiar, and at present to us mysterious, power of forming high-grade chemical combinations with appropriate molecules. It is not altogether absurd to suppose that this power is a special property of nitrogen in certain forms of combination. The theory is but an extension of the views of Kühne, Hoppe-Seyler, and others respecting the mode of action of enzymes. Neither is the view of the degradational origin of synthetical products in any way new.[1] I merely have thought it desirable to push it to its extreme limit in order that chemists may realise that there is a special chemistry of protoplasmic action, while the physiologists may exercise more caution in representing vital chemical transformations by equations which are in many cases purely hypothetical, or based on laboratory experiments which do not run parallel with the natural process. The chemical transformations which go on in the living organism are thus referred back to a peculiarity of protoplasmic matter, the explanation of which is bound up with the inner mechanism of the process of assimilation. If, as the protoplasmic theory implies, there must be combination of living protoplasm with appropriate compounds before synthesis is possible, then the problem resolves itself into a determination of the conditions which render such combination possible—*i.e.* the conditions of assimilation. It may be that here also light will come from the stereochemical hypothesis. The first step was taken when Pasteur found that organised ferments had the power of discriminating between physical isomerides; a similar selective power has been shown to reside in enzymes by the researches of Emil Fischer and his coadjutors. Fischer has quite recently expressed the view that the synthesis of sugars in the plant is preceded by the formation of a compound of carbon dioxide, or of formic aldehyde, with the protoplasmic material of the chloroplast, and similar views have been enunciated by Stohmann. The question has further been raised by van 't Hoff, as well as by Fischer, whether a stereochemical relationship between the living and dead compounds entering into combination is not an absolutely essential condition of all assimilation. The settlement of this question cannot but lead us onwards one stage towards the solution of the mystery that still surrounds the chemistry of the living organism.

RECENT DISCOVERIES OF GASEOUS ELEMENTS.

The past year has been such an eventful one in the way of startling discoveries that I must ask indulgence for trespassing a little further upon the time of the Section. It was only last year at the Oxford meeting of the British Association that Lord Rayleigh and Prof. Ramsay announced the discovery of a gaseous constituent of the atmosphere which had up to that time escaped detection. The complete justification of that announcement is now before the world in the paper recently published in the *Philosophical Transactions* of the Royal Society. The history of this brilliant piece of work is too recent to require much recapitulation. I need only remind you how, as the result of many years' patient determinations of the density of the gases oxygen and nitrogen, Lord Rayleigh established the fact that atmospheric nitrogen was heavier than nitrogen from chemical sources, and was then led to suspect the existence of a heavier gas in the atmosphere. He set to work to isolate this substance, and succeeded in doing so by the method of Cavendish. In the meantime Prof. Ramsay, quite independently, isolated the gas by removing the nitrogen by means of red-hot magnesium, and the

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[1] See, *e.g.*, Vines' "Lectures on the Physiology of Plants," pp. 145, 218, 227, 233, and 234. Practically all the great classes of synthetical products are regarded as the results of the destructive metabolism of protoplasm. A special plea for protoplasmic action has also been urged, from the biological side, by W. T. Thiselton-Dyer, *Journ. Chem. Soc.*, 1893; *Trans.* pp. 680-681

two investigators then combining their labours, followed up the subject, and have given us a memoir which will go down to posterity among the greatest achievements of an age renowned for its scientific activity.

The case in favour of argon being an element seems to be now settled by the discovery that the molecule of the gas is monatomic, as well as by the distinctness of its electric spark spectrum. The suggestion put forward soon after the discovery was announced, that the gas was an oxide of nitrogen, must have been made in complete ignorance of the methods by which it was prepared. The possibility of its being N_3 has been considered by the discoverers and rejected on very good grounds. Moreover, Peratoner and Oddo have been recently making some experiments in the laboratory of the University of Palermo with the object of examining the products of the electrolysis of hydrazoic acid and its salts. They obtained only ordinary nitrogen, not argon, and have come to the conclusion that the anhydride $N_3.N_3$ is incapable of existence, and that no allotropic form of nitrogen is given off. It has been urged that the physical evidence in support of the monatomic nature of the argon molecule, viz. the ratio of the specific heats, is capable of another interpretation—that argon is in fact an element of such extraordinary energy that its atoms cannot be separated, but are bound together as a rigid system which transmits the vibrational energy of a sound-wave as motion of translation only. If this be the state of affairs we must look to the physicists for more light. So far as chemistry is concerned, this conception introduces an entirely new set of ideas, and raises the question of the monatomic character of the mercury molecule which is in the same category with respect to the physical evidence. It seems unreasonable to invoke a special power of atomic linkage to explain the monatomic character of argon, and to refuse such a power in the case of other monatomic molecules, like mercury or cadmium. The chemical inertness of argon has been referred also to this same power of self-combination of its atoms. If this explanation be adopted it carries with it the admission that those elements of which the atoms composing the molecule are the more easily dissociated should be the more chemically active. The reverse appears to be the case if we bear in mind Victor Meyer's researches on the dissociation of the halogens, which prove that under the influence of heat the least active element, iodine, is the most easily dissociated. On the whole, the attempts to make out that argon is polyatomic by such forced hypotheses cannot at present be considered to have been successful, and the contention of the discoverers that its molecule is monatomic must be accepted as established.

In searching for a natural source of combined argon Prof. Ramsay was led to examine the gases contained in certain uranium and other minerals, and by steps which are now well known he has been able to isolate helium, a gas which was discovered by means of the spectroscope in the solar chromosphere during the eclipse of 1868 by Profs. Norman Lockyer and E. Frankland. In his address to the British Association in 1872 (*Reports*, 1872, p. lxxiv.) the late Dr. W. B. Carpenter said :—

"But when Frankland and Lockyer, seeing in the spectrum of the yellow solar prominences a certain bright line not identifiable with that of any known terrestrial flame, attribute this to a hypothetical new substance which they propose to call helium, it is obvious that their assumption rests on a far less secure foundation, until it shall have received that verification which, in the case of Mr. Crookes' researches on thallium, was afforded by the actual discovery of the new metal, whose presence had been indicated to him by a line in the spectrum not attributable to any substance then known."

It must be as gratifying to Profs. Lockyer and Frankland as it is to the chemical world at large to know that helium may now be removed from the category of solar myths and enrolled among the elements of terrestrial matter. The sources, mode of isolation, and properties of this gas have been described in the papers recently published by Prof. Ramsay and his colleagues. Not the least interesting fact is the occurrence of helium and argon in meteoric iron from Virginia, as announced by Prof. Ramsay in July (NATURE, vol. lii. p. 224). Like argon, helium is monatomic and chemically inert so far as the present evidence goes. The conditions under which this element exists in cleveite, uraninite, and the other minerals have yet to be determined.

Taking a general survey of the results thus far obtained, it seems that two representatives of a new group of monatomic elements characterised by chemical inertness have been brought to light. Their inertness obviously interposes great difficulties in the way of their further study from the chemical side; the future development of our knowledge of these elements may be looked for from the physicist and spectroscopist. Prof. Ramsay has not yet succeeded in effecting a combination between argon or helium and any of the other chemical elements. M. Moissan finds that fluorine is without action on argon. M. Berthelot claims to have brought about a combination of argon with carbon disulphide and mercury, and with "the elements of benzene, . . . with the help of mercury," under the influence of the silent electric discharge. Some experiments which I made last spring with Mr. R. J. Strutt with argon and moist acetylene submitted to the electric discharge, both silent and disruptive, gave very little hope of a combination between argon and carbon being possible by this means. The coincidence of the helium yellow line with the D_3 line of the solar chromosphere has been challenged, but the recent accurate measurements of the wavelength of the chromospheric line by Prof. G. E. Hale, and of the line of terrestrial helium by Mr. Crookes, leave no doubt as to their identity. Both the solar and terrestrial lines have now been shown to be double. The isolation of helium has not only furnished another link proving community of matter, and, by inference, of origin between the earth and sun, but an extension of the work by Prof. Norman Lockyer, M. Deslandres, and Mr. Crookes, has resulted in the most interesting discovery that a large number of the lines in the chromospheric spectrum, as well as in certain stellar spectra, which had up to the present time found no counterparts in the spectra of terrestrial elements, can now be accounted for by the spectra of gases contained with helium in these rare minerals. The question now confronts us, Are these gases members of the same monatomic inert group as argon and helium? Whether, and by what mechanism, a monatomic gas can give a complicated spectrum is a physical question of supreme interest to chemists, and I hope that a discussion of this subject with our colleagues of Section A will be held during the present meeting. That mercury is capable under different conditions of giving a series of highly complex spectra can be seen from the memoir by J. M. Eder and E. Valenta, presented to the Imperial Academy of Sciences of Vienna in July 1894. With respect to the position of argon and helium in the periodic system of chemical elements, it is, as Prof. Ramsay points out, premature to speculate until we are quite sure that these gases are homogeneous. It is possible that they may be mixtures of monatomic gases, and in fact the spectroscope has already given an indication that they contain some constituent in common. The question whether these gases are mixtures or not presses for an immediate answer. I will venture to suggest that an attack should be made by the method of diffusion. If argon or helium were allowed to diffuse fractionally through a long porous plug into an exhausted vessel there might be some separation into gases of different densities, and showing modifications in their spectra, on the assumption that we are dealing with mixtures composed of molecules of different weights.

NOTES.

THE *Times* of Tuesday last contained a letter, signed by Profs. M. Foster, E. Ray Lankester, and G. B. Howes (Hon. Secretaries to the Provisional Committee), with reference to the General Committee now being formed for the purpose of establishing a memorial of the late Prof. Huxley. The letter states that H.R.H. the Prince of Wales has been pleased to become the Honorary President of the Committee. No very active steps can be taken until after the autumn recess, when the General Committee will hold its first meeting, probably in October. The Honorary Secretaries will after that report the progress that has been made both in this country and abroad; and a list of the complete Committee and a statement of the subscriptions received will be published. Appended to the letter is a list of an enormous number of names of persons who have already signified their desire to serve on the Committee.

A MEMORIAL tablet in honour of Prof. Helmholtz has been affixed to the house, No. 8 Haditzstrasse, at Potsdam, where he was born, and it is stated that it is intended to erect a joint monument to the memory of Werner Siemens and Helmholtz in front of the Technische Hochschule at Charlottenburg.

PROF. RETSIUS and Dr. Bergh, of Copenhagen, have been elected Correspondants of the Paris Academy.

THE Berliner Akademie der Wissenschaften has, we understand, recently elected the following gentlemen as corresponding members:—Prof. W. V. Gümbel (München), Prof. A. von Zittell (München), Prof. A. Schrauf (Vienna), Prof. A. Cossa (Turin), Prof. A. Agassiz (Cambridge, Mass.), and Prof. E. Mascart (Paris).

THE quinquennial International Metric Congress, which is at present being held in Paris, under the presidency of Dr. Marey, was opened on the 4th inst. by M. Hanotaux, who delivered a brief address. On the 6th inst. the second session of the Congress took place, and M. Hirsch, of the Neuchâtel Observatory, was elected Secretary. The Secretary presented the report of the Committee on the work already done, and the present state of the International Bureau of Weights and Measures, and a series of metric standards which have been under consideration since the Congress of 1889 was sanctioned.

THE Swiss Naturforschende Gesellschaft has been holding its annual congress at Zermatt. The proceedings began on September 8, and concluded on the 11th. September 8 was devoted to the meetings of committees; the Sections met on September 10, and on the 9th and 11th inst. the general meetings took place.

THE death is announced of Dr. Sven Lovén, the distinguished Swedish naturalist. He was born, says the *Times*, at Stockholm in 1809, and received his education at the University of Lund, where he took the degree of Doctor of Philosophy. After attending lectures in Berlin in 1830-31, he devoted himself to the study of the maritime fauna of the coasts of Scandinavia. He also explored the Baltic and the North Seas, and conducted the first scientific expedition to Spitzbergen in 1837. He was the author of numerous scientific memoirs, all published by the Royal Swedish Academy of Sciences. Dr. Lovén was elected a member of the Academy of Stockholm in 1840, and Professor and Conservator of the Royal Museum of Natural History of that city in 1841. He was a member of the academies of Berlin and Munich, a corresponding member of the Institute of France, and in 1885 was elected a foreign member of the Royal Society of London.

THE death is recorded, at the age of eighty-one years, of Mr. James Carter, of Cambridge. For very many years Mr. Carter practised as a medical man, but found time to engage in the study of scientific and antiquarian subjects, and was especially interested in palæontology. He contributed many papers to the *Geological Magazine* and the *Quarterly Journal* of the Geological Society, and served for many years on the Councils of the Geological and Palæontological Societies.

THE *Kew Bulletin* has heard with regret of the death from dysentery in May last of Mr. F. H. Smiles, who had been attached to the Royal Survey Department of Siam. Mr. Smiles, who had already done some good botanical work, returned to Siam in December last with the intention of making further botanical collections, and it was confidently anticipated that he would have added considerably to the knowledge of the rich flora of Upper Siam.

THE death is announced of Mr. R. H. Tweddell, the well-known engineer; of Mr. E. F. C. Davis, president of the American Society of Mechanical Engineers; and of Mr. H. C. Hart, one of the first class technical officers of the engineer-in-chief's office, Post Office Telegraphs.

THE centenary of Jenner's first experiments in vaccination is to be celebrated next May by the Russian National Health Society. To commemorate the event the Society proposes (1) to offer four prizes for the best works upon vaccination; (2) to collect and publish materials for a history of the practice of vaccination in Russia, and a short history of the same in Western Europe; (3) to publish a Russian translation of Jenner's works, accompanied by his biography and portrait; (4) to organise an exhibition of objects connected with vaccination; (5) to hold a commemorative meeting on the day of the centenary.

THE annual joint meeting of the Swiss Geographical Societies will be held this year at St. Gall, on September 22 and 23. At this meeting a paper will be read by Dr. Hans Meyer on the "Snow Mountains of Equatorial Africa."

AN exhibition of agricultural machinery, similar to that held in May of the present year, is being arranged under the auspices of the Imperial and Royal Agricultural Society of Vienna, to take place in that city in May 1896. The exhibits will comprise not only agricultural machines as generally understood, but appliances used in all branches of industry connected with agriculture, such as breweries, and distilleries, and yeast, sugar, vinegar, and starch factories.

WE learn from the *Nation*, New York, that only one MS. was received in competition for the prize of 400 dollars given by Dr. Gould's *Astronomical Journal* "for the most thorough discussion of the theory of the rotation of the earth with reference to the recently discovered variations of latitude." The paper was sent by and the prize awarded to Prof. Newcomb. The other prize, of 200 dollars, was given to Mr. Paul S. Yendell, for the best series of determinations of maxima and minima of variable stars.

Science states that the Berliner Akademie der Wissenschaften has recently put aside over £1000 for the promotion of scientific work and research. Of this amount an appropriation of £100 has been made to Prof. Fuchs, of Berlin, to be devoted to the continuation of the publication of Dirichlet's works; £100 to Prof. Weierstrass, of Berlin, for the publication of his collected works; £75 to Prof. Gerhardt for the publication of the mathematical correspondence of Leibnitz, and £100 to Dr. Schauinsland for researches on the Fauna of the Pacific islands.

THE Göttingen Gesellschaft der Wissenschaften will, on February 1, 1897, award a prize of 500 marks for an anatomical research and description of the cavities of the body of the new-born child and their contents compared with those of the adult.

THE Academy of Sciences of Cracow proposes, as the subject for the Copernicus prizes, theories concerning the physical condition of the globe. Essays must be written in the Polish language, and reach the Academy before the end of 1898.

THE Orient Steam Navigation Company, Limited, announce their intention of sending one of their steamships to Vadsö, Varanger Fiord, Lapland, in August next, to enable observations to be made of the total eclipse of the sun on August 9, 1896. It is arranged for the vessel to leave London on July 21, to arrive at Vadsö on August 3, and to return from the latter place on the 10th, reaching London on August 17. Particulars as to the cost, &c., of the trip may be seen in our advertisement columns, or obtained from Messrs. Anderson, Anderson, and Co., 5 Fenchurch Avenue, E.C., or 16 Cockspur Street, S.W.

SEVERE thunderstorms again occurred in the southern and eastern parts of England early on Saturday morning, 7th instant, accompanied with heavy falls of hail and rain, and causing considerable damage. The disturbance was occasioned by the development of shallow depressions over the Bay of Biscay and the English Channel, and by the intense heat over the continent, the maximum shade temperature in some parts of France being considerably above 90°, while in the east of England a temperature of 85° was recorded. Rainfall exceeded an inch in London and other places, and amounted to 1·78 inches in Hampshire. During the height of the storm the lightning flashes averaged about twenty-five to the minute.

THE Shetland County Council, says the *Glasgow Herald*, has resolved to apply to the Secretary for Scotland for an order under the Wild Birds Protection Act of 1894, prohibiting the taking of the eggs of certain wild birds. The schedule proposed includes such birds as the white-tailed or sea eagle, great skua, Richardson's skua, Allan whimbrel, ember goose, &c. All these birds have become extremely rare, and it is stated that there has been recently a trade carrying on in their eggs for the American market, to the threatened extinction of the birds.

WE are asked to announce that with the September number the *American Journal of Psychology* will enter upon its seventh volume. The preceding volumes have been edited by President G. Stanley Hall (Clark University). For the future the editorial responsibility of the *Journal* will be shared by President Hall, Prof. E. C. Sanford (Clark University), and Prof. E. B. Titchener (Cornell University). A co-operative board has been formed, which includes the names of Prof. F. Angell, Prof. H. Beaunis, Prof. J. Delboeuf, Dr. A. Kirschmann, Prof. O. Kuelpe, Dr. A. Waller, F.R.S., and Prof. H. K. Wolfe. The *Journal* will be devoted exclusively to the interests of experimental psychology (psychophysiology, psychophysics, physiological psychology, &c.). Each number will contain, as heretofore, original articles, reviews and abstracts of current psychological books and monographs, and notes upon topics of immediate psychological importance. Contributions may be addressed to either of the three editors.

Science states that the Board of Scientific Directors of the New York Botanic Garden has recently resolved to authorise a topographical survey of the 250 acres of land in Bronx Park which have been set aside for the uses of the garden. All the trees in the park are to be labelled, and new varieties of seeds desirable for cultivation are to be secured.

THE Allahabad *Pioneer Mail* says that an experiment is now in progress in several of the larger gaols of the Punjab, which may have important results in the future. It has been one of the ordinary precautions in time of cholera epidemics to boil the drinking water supplied to the prisoners. To ascertain whether it might not be advisable always to boil the drinking water, the Lieutenant-Governor has ordered that a certain number of the prisoners should be given boiled, and an equal number unboiled, water, the results being reported at the end of the year. If these are as expected, the reduction in the fever death-rate should be followed by a similar reduction in the mortality from dysentery and diarrhœa.

WE learn from *Engineering* that an important undertaking has been inaugurated at Seattle, in the State of Washington, U.S.A. This city is situated on Elliott Bay, a thoroughly sheltered harbour, which communicates with the Pacific by the Straits of San Juan de Fuca. About two miles from the coast and behind the town is a fresh-water lake of considerable size, the water level of which is about 16 feet above high water in the bay. A ship canal between the lake and the sea has long been suggested, and the work has at last been definitely commenced. The bottom of the channel will be 80 feet, and the greatest depth of cutting will be 308 feet. Almost the whole of the work will, however, be carried out through comparatively high land, the amount of excavation required being estimated at 36,000,000 cubic yards. The material is mostly glacial drift, and it is proposed to use hydraulic nozzles to facilitate the work of excavation, the spoil being washed down by a jet of water issuing at high pressure from a nozzle, as in some of the Californian gold workings. A lock 400 feet long will be constructed at the sea entrance to the canal. The material excavated will be used for raising the level of low-lying ground along the sea front of the city.

M. ZACHAREWIEZ, Professor of Agriculture at Vaucluse, has found by experiment with different-coloured glasses that fruit is finest and earliest when grown under clear glass. Orange glass produces an increase of vegetation, but at the cost of the amount of fruit, of the size and of its forwardness. Violet glass causes the number of fruit to increase at the expense of the quality. Red, blue, and green glass are hurtful to all kinds of vegetation.

THE possibility of successfully boring for water in extensive areas of crystalline rocks has been demonstrated, we learn from the September number of *Natural Science*, at several places in Sweden. The experiments were suggested by certain conclusions of Nordenskiöld, based on the downward limit of surface variations of temperature and other physical considerations. He considered that vertical jointing of the rocks would not extend below 30 or 40 metres, and that at that depth extensive horizontal fissures must be formed. This has now been found to be the case, and from these horizontal fissures abundant water of great purity has been obtained. While these results are of practical importance (particularly with regard to the water-supply of small rocky islands), it also opens up a number of interesting general questions as to the flow and pressure of water in crystalline rocks.

IN our issue for August 15, we printed an abstract of a paper on "The Voyage of the *Antarctic* to Victoria Land," read by Mr. C. E. Borchgrevink at the recent International Geographical Congress, and now have to acknowledge the receipt of the journal and notes of the commander of the whaler *Antarctic*, in which Mr. Borchgrevink made his somewhat unpropitious voyage as a sailor before the mast, which the Secretary of the Royal Geographical Society of Australasia (Victorian Branch) has been good enough to send us. The pamphlet, which contains some highly interesting matter, is accompanied by a lithographed map, by Captain Leonard Kristensen, of the track taken by his vessel, and forms part of the *Transactions* of the above-named Society.

Natural Science for September contains extracts from the address delivered by the Rev. Canon A. M. Norman, F.R.S., as President of the recently held Museums Association at Newcastle, and deals with the progress of biology in that northern town. An article on "The Geology of Ipswich and its Neighbourhood," by Mr. Clement Reid, appears at an opportune moment, and will doubtless be consulted by many geologists visiting the British Association. Other contributions to the number are:—"Some Recent Insect Literature," "The Nucleolus," "The Rôle of Sex," and "The Alleged Miocene Man in Burma." The last-named article has reference to a paper by Dr. Noetling, published towards the close of last year, "On the Occurrence of Chipped (?) Flints in the Upper Miocene of Burma." The writer, Mr. R. D. Oldham, says in conclusion, "till more complete evidence has been produced it is impossible to accept the existence of man in either Miocene or Pliocene times as one of the established facts of geology."

WE are glad to note the reappearance of the *Bollettino Mensuale* of the reorganised Italian Meteorological Society. The bulletin is issued in a more convenient, small folio form, but in other respects it is similar to the former publication. The current number contains two important articles by Prof. L. Di Marchi, on the causes of the glacial epoch, and the dynamical conditions of thunderstorms, and an investigation of the effects of the earthquake at Florence on May 18 last, by C. Bassani.

AN examination of the gases liberated from certain of the sulphurous waters of the Pyrenees reveals, in the hands of M. Ch. Bouchard, the interesting fact that the formerly assumed nitrogen (from which the Spanish physicians have named these waters *azoades*) consists in part of free argon and helium. The collected gas was in each case, after treatment with potash and phosphoric anhydride, introduced into a Plücker tube containing magnesium wire. Under the action of the silent discharge the nitrogen rapidly disappeared by combination with magnesium, leaving a residue exhibiting the characteristic rays of both argon and helium for the gas derived from the waters of la Raillère, helium from the springs of Bois, and helium together with probably an unknown gas from the waters of lowest temperature at Bois.

THE use of magnesium wire and the silent discharge is due to MM. L. Troost and L. Ouvrard, who show that the magnesium vapour produced very rapidly combines with nitrogen under the conditions obtaining in the tubes. Further, the continued action of a powerful silent discharge, for some hours after the spectroscopic evidence proves the absence of nitrogen, results in a gradual diminution in intensity of the helium and argon rays. Finally a complete vacuum is produced, hence it appears that magnesium combines with argon and helium under these circumstances. Platinum appears to behave like magnesium towards argon in Plücker tubes with the silent discharge.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*, ♀) from India, presented by Mrs. Ball; an Emu (*Dromæus novæ-hollandiæ*) from Australia, presented by Mr. C. W. Williams; a Raven (*Corvus corax*), British, presented by Mr. W. Weeker; a Royal Python (*Python reginis*) from Dahomey, West Africa, presented by Mr. C. H. Harley-Moseley; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. C. Sampson; a —— Snake (*Phrynonax eutropis*), a —— Snake (*Phrynonax fasciatus*) from Trinidad, presented by Mr. R. R. Mole; a White-tailed Sea Eagle (*Haliætus albicilla*) from Scotland, two Diamond Snakes (*Morelia spilotes*) from Australia, deposited; eight Amherst Pheasants (*Thaumalea amherstiæ*), six Ring-necked Pheasants (*Phasianus torquatus*), two Japanese Pheasants (*Phasianus versicolor*), a Temminck's Tragopan (*Ceriornis temmincki*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE PROPER MOTION OF THE SUN.—In the September number of the *Bulletin Astronomique* M. Tisserand gives an interesting account of a method of determining the proper motion of the sun from stellar proper motions. Denoting by m and m' the values of the annual proper motions of the stars, c the space described by the sun in one year, this space being measured with the same unit as the distance (ρ) of the sun, and A and D the Right Ascension and Declination of the apex of the sun's way, the formulæ for reduction become

$$m \cos \delta = \frac{c}{\rho} \cos \mathrm{D} \sin (\alpha - \mathrm{A})$$

$$\frac{m'}{\cos \delta} = -\frac{c}{\rho} \sin \mathrm{D} + \frac{c}{\rho} \cos \mathrm{D} \tan \delta \,.\, \cos (\alpha - \mathrm{A}).$$

In the second equation the second term changes its sign with $\tan \delta$, ρ changes its value from star to star. Assuming that the mean of the values of this term will be small or zero, and that Σ represents the arithmetical mean, we have—

$$\Sigma\left(\frac{m'}{\cos \delta}\right) = -c \sin \mathrm{D}\, \Sigma\left(\frac{1}{\rho}\right).$$

Now, because sin D is positive, the mean values of the left-hand side of the equation ought to be negative. If there were no proper motion to the sun, they should be zero.

Using the catalogue of 1054 stellar proper motions, motions of M. Stumpe (*Astr. Nach.*, Nos. 2999–3000, year 1890), only those stars have been employed the declinations of which are comprised between − 30° and + 30°, and the proper motions less than $0''.64$.

The mean values for the sum above were then tabulated for every hour of Right Ascension. These were found to be all negative, as they ought to be, and they did not differ very much from one another. For 585 stars the mean value was $-0''.151$.

M. Tisserand further investigated the values obtained from another catalogue of 2641 stellar proper motions, by M. Bossert, in exactly the same way. Here the mean values were still found all to be negative, and not very different from one another. From 1537 proper motions the value obtained was $-0''.131$.

By taking only the proper motions of stars comprised between declinations ±15°, the value obtained does not differ materially from that given above. In the interval then of a century, for each hour of right ascension, the declinations of all the stars have diminished (in the mean) by quantities comprised between $10''$ and $20''$; and he says, "il nous semble que cela donne une preuve materielle frappante du mouvement du Soleil."

THE ROTATION OF VENUS.—A difficult problem in observational astronomy is the determination of the period of the rotation of Venus. M. Schiaparelli, whose powers of observations have been often put to the test, still thinks that the planet accomplishes one rotation in the same time that it takes to travel round the sun, or, in other words, the same hemisphere is always turned towards the sun. M. Leo Brunner, however, who has made during three months a great number of drawings, which appear to corroborate his statement, seems to be of quite a different opinion, for he says: "J'ai le plaisir de vous annoncer que je viens de découvrir la vraie période de rotation de Venus, qui ne diffère que de quelques minutes de celle de notre terre. Cette découverte est hors doute, car j'ai pu voir arriver et passer des taches plusieurs jours avec la plus grande distinction. Nul doute à cet egard." It must not be forgotten, however, that the observation of Venus is one attended by great difficulty. Even Brunner's drawings and those of Schiaparelli made of the planet at the same time are very different. There seems to be no doubt that the observations are all verging on the limit of visibility, and that the 224 days or the 24-hour period are just as probable as ever.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following appointments have been recently made by the governing bodies of the undermentioned colleges:—At St. John's, Mr. R. H. Adie, a Lecturer in Natural Science; at Magdalene, Mr. G. T. Manley, Lecturer in Mathematics; at Trinity, Mr. G. T. Walker, Lecturer in Mathematics, and Messrs. W. C. D. Whetham and J. W. Capstick, Lecturers in Natural Science; at Emmanuel, Mr. A. Eicholz, Lecturer in Natural Science; at Sidney Sussex, Mr. R. H. D. Mayall, Lecturer in Mathematics; at Selwyn, Mr. L. A. Borradaile, Lecturer in Natural Science.

ACCORDING to *Science*, Prof. Bonnet, Professor of Anatomy in the University of Giessen, has received a call to Greifswald; and Dr. M. Miyoshi has been appointed Professor of Botany in the University of Tokyo.

MR. CHAS. BERRY, horticultural lecturer to the East Suffolk County Council Technical Instruction Committee, has been appointed Instructor in Horticulture by the Devonshire County Council, and will enter upon his duties at the end of September.

THE prospectus of Day and Evening Classes at the Battersea Polytechnic Institute for the session 1895–6, has reached us, and contains full information respecting the numerous classes held at this well-appointed institution. Several new classes are to be formed, and special provision is made for the needs of students who are desirous of entering for the examination of London University, from the matriculation to the final B.Sc.

THE fourth annual report (1894–5) of the Department of Agriculture, Yorkshire College, Leeds, has been published, and shows clearly that a great deal of useful work has been carried on during the past twelve months, and has, on the whole, met with very satisfactory success. With one exception (that of the classes for elementary teachers) each branch has exhibited much growth. The lectures given to farmers and others were well attended, and the work of the lecturers was much assisted by the travelling libraries sent out by the Victoria University in connection with the various courses. A new departure was made by the institution of short lectures on poultry-keeping. At the close of the session examinations were held, at which 188 candidates from 26 centres presented themselves, and of this number 145 passed, 58 attaining distinction. The prospectus of the Courses in Agriculture, Session 1895-6, is now ready, and may be had on application to the Registrar.

The Agricultural Department of the University College of North Wales, Bangor, has just issued its prospectus for the approaching session, in which all information respecting classes, &c., is given. Arrangements have been made by which farms in the neighbourhood of the college may be made use of by the professors and their students for practical instruction. The prospectus can be obtained from the Secretary.

The *Technical World* says: "One of the most interesting experiments undertaken by the Durham College of Science is the provision of a series of agricultural stations, of which there are now about sixty in Northumberland, Cumberland, and Durham. At these stations practical instruction is given by means of experiment and demonstration in the science of agriculture. Manures are supplied to the stations from the college, where they are analysed and blended as may be required for the particular experiment, and the resultant crops are afterwards tested under the direction of the Professor of Agriculture. These experiments give valuable opportunities to students to observe the varying results obtained under the different conditions of soil and climate in the various districts of the North, and also provide useful data for agriculturists therein."

A new technical school was opened at Runcorn on August 31, by Sir John T. Brunner, M.P. The school was erected at a cost of £4200, and contains eleven class-rooms and a lecture-hall.

In view of the forthcoming opening of the Medical Schools, the current issues of our contemporaries, the *Lancet* and *British Medical Journal*, are devoted almost exclusively to particulars likely to be of service to medical students. The *Chemical News* for September 6 is likewise a "student's number," and contains much information respecting the various schools of chemistry.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, September 2.—M. Fizeau in the chair.—The work of 1895 at Mont Blanc Observatory, by M. J. Janssen. Determinations of the intensity of gravity have been made with very delicate instruments at Grands-Mulets (3050 m.) and at Chamounix by M. Bigourdan. It is hoped to carry out a similar determination on the summit of Mont Blanc next year. All the parts of the 33 cm. parallactic telescope have been conveyed to the site at the head of the glacier where it is to be erected.—On the presence of argon and of helium in certain mineral waters, by M. Ch. Bouchard. (See Notes, p. 487).—On the combination of magnesium with argon and with helium, by MM. L. Troost and L. Ouvrard. (See Notes, p. 487).—On a continuous group of transformations with twenty-eight parameters which occurs in the theory of deformation of surfaces, by M. Paul Staeckel.—Researches on the combinations of mercuric cyanide with bromides, by M. Raoul Varet. Thermochemical data are given for a number of compounds of the general type $2HgCy_2.MBr_2.xH_2O$. It is shown that in solution these substances yield but a slight isopurpurate reaction, and slightly redden litmus. With heat the effect is increased. The substances possess a similar constitution to the chlorocyanides, the cyanogen remaining mostly in combination with the mercury. Rather a greater proportion of the cyanogen passes over to the second metal than in the case of the chlorocyanides. A slight evolution of heat occurs in the change, a result contrary to what would be expected from the character of the iodo-compounds.—On the formation of hydrogen selenide, by M. H. Pélabon. Liquid selenium absorbs hydrogen selenide. Carrying out experiments on the formation of hydrogen selenide in relation to temperature with the smallest excess of selenium in order to avoid this source of error, it is found that the formula of Gibbs and Duhem,

$$\log \frac{p_1}{p_2} = \frac{M}{T} + N \log T + S,$$

accurately represents the experimental results (p_1 and p_2 represent the partial pressures of H and SeH_2, T is the abs. temp. of experiment, log means Napierian log, M, N, and S are constants). The ratio $\rho = \frac{p_2}{p_1 + p_2}$ has a maximum value at a temperature $t = \frac{M}{N} - 273$. With values of the constants calculated from the experimental results, $t = 575°$, the experimental maximum agrees with this result. The molecular heat of formation calculated by Duhem's formula with the found values for the above constants is − 17380 Cal., Fabre found − 18000 Cal. The difference is not great, and may be readily accounted for when it is remembered that (1) in this formula hydrogen and hydrogen selenide have been assumed to be perfect gases; (2) the formula has been applied beyond the limits of temperature of the experiments from which M and N are determined.—Action of carbonic acid, water, and alkalis on cyanuric acid and its dissolved sodium and potassium salts, by M. Paul Lemoult. A heat of neutralisation paper in which the decomposition of cyanuric acid slowly occurring in presence of bases is shown to agree with the equation $C_3N_3O_3H_3$ diss. + $3H_2O$ + Aq = $3CO_2$ diss. + $3NH_3$ diss. + 200 Cal.—The eclipsoscope, an apparatus for viewing the chromosphere and solar protuberances, by M. Ch. V. Zenger.—M. Ch. V. Zenger sends another note relative to the possibility of predicting great seismic and atmospheric disturbances during the passage of periodic swarms of shooting-stars when great activity of the solar surface is observed at the same time.

BOOKS, PAMPHLET, and SERIALS RECEIVED.

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