THURSDAY, SEPTEMBER 14, 1893.

THE MECHANICS OF FLUIDS.

Hydrostatics and Elementary Hydrokinetics. By George M. Minchin, M.A., Professor of Applied Mathematics in the Royal Indian Engineering College, Coopers Hill. (Oxford: at the Clarendon Press, 1892.)

A WORK on this subject which should incorporate the latest developments has long been wanted; and Prof. Minchin has performed a very useful service in providing a treatise of a convenient size for purposes of instruction.

The first chapter starts with some general theorems on the distribution of strain and stress in the interior of a body, which to our way of thinking had better have been relegated to Chapters iii. or iv., by which time the student would be able to appreciate their importance. Mr. Minchin, however, justifies his method in eloquent language, but his simile of the danger of leaving uncaptured fortresses in the rear partakes of ante-Napoleonic ideas; as Napoleon proved it makes all the difference whether the foe is stationary or mobile.

We are pleased to see the author's practical protest against the banishment of the notation (we cannot dispense with the idea) of the Differential Calculus, traditional in our elementary treatises. A French schoolboy acquires a working knowledge of the Differential Calculus episodically, in the course of his studies of elementary algebra and trigonometry.

Mr. Minchin postulates at the outset a *perfect* fluid, that is a fluid devoid of viscosity. This is necessary when we come to the Motion of Fluids; but the theorems of Hydrostatics are true of all fluids, however viscous, such as tar, or even pitch; a fluid from its general definition is not capable of coming to rest till the *normality* of the stress has been attained.

The word *intensity* is prefixed by the author when it is wished to indicate that a stress is estimated per unit area; thus, for instance, 150 pounds on the square inch he calls the "intensity of the pressure." But this is contrary to our ordinary language, where "intensity" is never employed. Mr. Minchin had better have adopted another word, "thrust," to express total pressure or push against a given area, leaving the words stress and pressure, as in common usage, to imply that they are estimated per unit area, square foot or inch, metre or centimetre.

This would not be the work of a modern college professor if the author did not explain at some length that the world has been calling things by their wrong names; thus it is maintained that the expression above "a pressure of 150 pounds on the square inch" is inaccurate, and should always be replaced by "an intensity of pressure of 150 pounds' *weight* on the square inch."

This is a counsel of perfection which a careful search would probably show is not always observed by the author himself; and it is invariably ignored and rejected by practical men, including his own engineering colleagues.

Thus Prof. Hearson, R.N., in a recent examination paper at the Naval College, Greenwich, asks for the calculation of the resistance of a train in "pounds per ton weight"; but his M.A. colleague would edit this into "pounds' weight per ton mass."

The Coopers Hill student will have to be as careful to recollect the expression appropriate for the class-room he is attending, as the Chairman of the House of Representatives in America, according to the story, in addressing the rival members of Illi*noi* and Illi*noise*.

The use of the word "weight" to designate only the accidental quality of a body due to its position on the surface of the Earth is much insisted upon by a certain school of our writers; but this temporary fad will soon pass away, we hope, as it seems to be tainted with the ancient heresy of the existence of bodies possessing positive levitation, such as the fire or inflammable air said to have been employed in Archytas's pigeon, or the rarefied dew with which Bishop Wilkins proposed to fill a number of egg-shells, and thereby fly in the air.

For instance, what is the weight of a ton (mass) of hydrogen; must we say that it is about—13 tons?

Prof. Oliver Lodge would banish the word "hundredweight" from our language; but what has he to offer the architect in exchange?

Pressures on foundations in architecture are most conveniently measured in cwt. per square foot, from the simple fact that the average weight of a cubic foot of brickwork is one hundredweight.

If the architect of the Tower of Pisa had made a calculation in accordance with the modern formula for the resistance of foundations in earth,

$$p = w\,h\left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2,$$

in cwt. per sq. foot, at a depth of h feet in earth of density w cwt. per cubic foot, ϕ denoting the angle of repose of the earth, he would have found that his depth of 22 feet, with $w = 0{\cdot}8$ and $\phi = 22°$, would bear only 84 cwt. per square foot; while the pressure due to the weight of the tower mounted up to 145 cwt. per square foot.

Students owe a debt of gratitude to Prof. Minchin for having almost entirely banished the old-fashioned mystifications concerning

$$W = s\mathrm{V} \text{ and } W = g\rho\mathrm{V};$$

and he very clearly points out that the pressure at a depth z in liquid of density ρ is not given by ρz gravitation units, but by $g\rho z$ absolute units.

But the introduction of the new term "*specific weight*" to designate what has hitherto been called the *heaviness* (or *density*) of a substance is to be deprecated, especially as the author is careful to explain that he does not mean *specific gravity* by *specific weight*.

But the German for specific gravity is *spezifische gewicht*, so that confusion is sure to arise; much the same as with the word *masseinheit*, which means unit *of measure*, and not *unit of mass*, as it has been incorrectly translated.

It is doubtful whether any advantage is gained by the introduction of absolute units into a statical subject; they are never used in experimental and practical work; but if the experimenter wishes to express his numerical results in a cosmopolitan form, he can multiply his gravitation results by the local value of g, as the last operation of all.

Unfortunately, in the C.G.S. system selected by scientific men, the units are so minute that they are only suitable for the most delicate phenomena of the physical laboratory, such as Capillarity; and numbers run very high in ordinary dynamical problems.

Millions of *boles* of impulse would be required to flick a sixpence across the counter; and the answer "millions," which Albert Smith said he received from the stoker when he asked how many degrees of temperature there were in the stoke-hold, would not be wrong if he had asked what pressure the boilers carried; "fifteen millions" might be the answer of the scientific stoker of to-day, trained in the use of the C.G.S. system.

Another banishment from this treatise to be grateful for, is that of "the whole pressure of a fluid on a curved surface."

If, however, this whole pressure is divided by the surface, we obtain the *average* pressure over the surface, a distinct mechanical motion, sometimes useful; with this resetting the "visionary problems of pure mathematics" on whole pressure might be allowed to survive, as some of them embody elegant geometrical applications.

Generally throughout the work Mr. Minchin has secured the assistance of his colleague Mr. Stocker, the Professor of Physics, for the experimental illustrations and diagrams, and we meet with many novel and ingenious experiments, for instance in the illustration of Boyle's Law in Fig. 57.

This gives a flavour of the Physical Laboratory to the book, and not that of the Engineering Theatre, except for the elegant geometrical treatment of the Line of Thrust in a Reservoir Dam. The Hydraulic Press of Fig. 7 could hardly serve to lift a girder of the Britannia Bridge, or squeeze a steel forging with a thrust of thousands of tons.

The equilibrium and stability of a floating body is illustrated in Fig. 49 by what looks like a champagne cork, and not by the cross-section of an ironclad or Atlantic steamer, with compartments bilged and full of water to illustrate the effect of petroleum or liquid cargo, or the unfortunate capsizing of the *Victoria*.

The diagram of a floating body in the ordinary mathematical treatise, where it is not like a cinder or a potato, but a vague idea of the cross-section of a ship, has the metacentre placed somewhere up the mast.

Prof. Minchin reduces this metacentric height to more reasonable figures, 5 or 6 feet; but even this is excessive, as H.M.S. *Prince Consort*, with a metacentric height of 6 feet, was a notorious bad roller; vessels of the greatest size are plying successfully with a metacentric height of under 1 foot; and we read a day or two ago of one of the largest modern steamers becoming unstable when being undocked.

The question of the stability of a ship involves the two antagonistic qualities of "stiffness" and "steadiness."

A "steady" vessel has a small initial metacentric height, and "stiffness" under sail is secured by making the metacentre rise rapidly as the ship heels.

The whole theory of the geometry of the ship is one of great mathematical interest; and the valuable compilation of all the best recent work on this subject, made by Sir E. J. Reed in his "Stability of Ships," deserves to be better known among mathematicians.

Chapter vi., on Gases, is one which will excite great admiration, from the way in which the leading parts of Thermodynamics are introduced; the most recent theories have been incorporated and illustrated numerically and experimentally; here the valuable assistance of Prof. Stocker is acknowledged. In this part of the subject we think that a simplification would be effected by pointing out that with the gravitation units employed in § 48, the quantity k in the equation $p = k\rho$ is the "height of the homogeneous atmosphere."

Hydraulic and Pneumatic Machines are carefully described and illustrated in Chapter vii. Fig. 71 of the Fire Engine is curious as illustrating the continuity of mathematical diagrams, as it might have been copied from the one given in Hero's Pneumatics B.C. 120, as invented by Ctesibius.

The hydraulic ram (*bélier hydraulique*), Fig. 73, is here attributed to Whitehurst, of Derby (1772). This will raise a protest in France, where Montgolfier is considered the inventor; but, on the other hand, Mr. Minchin gives Mariotte a half share in the discovery of Boyle's law.

Chapter viii., on "Molecular Forces and Capillarity," is very complete but rather formidable, as it does not shirk the difficult theories of Laplace on Molecular Pressure. The author must utilise in the next edition the scale invented by Mr. C. V. Boys, for drawing with accuracy the various capillary curves.

In the two hydrodynamical Chapters, ix. and x., there may appear some need for the use of the absolute units; but considering that the motion discussed is due to gravity, the only effect of a change from gravitation to absolute units is to remove g from the denominator of certain terms to the numerator of the remainder in the equations.

The use of hyperbolic functions would simplify the expressions on the last page of the book, in the discussion of Kelland's state of wave motion.

Judiciously selected examples are introduced in small sets, to illustrate the principles at easy stages; these are printed in smaller type, and the book is thereby kept within a handy size; at the expense, however, of the eyesight of some readers.

A. G. GREENHILL.

LETTERS TO THE EDITOR.

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Palæozoic Glaciation in the Southern Hemisphere.

THE interest evinced in the above subject in so many quarters, and the evident ignorance of what has been done in the matter, is my excuse for asking space for some notes on my personal researches.

South Africa.—In July, 1872, while journeying through Bushmanland, at Mr. Niekerk's farm "Welgevonden," near Prieska, on the Orange River, I observed extensive accumulations of pebbles and boulders loosely piled, many of them striated, scored, and facetted—in fact, unmistakably ice-marked. One of the boulders I took to Cape Town, and deposited it in the South African Museum. This was the first discovery of glaciation in Cape Colony, and it attracted some attention at the time (*vide Cape Monthly Magazine*, &c.). While crossing Bushmanland, the boundaries of this conglomerate were jotted down, and they were delineated on my Sketch

Geological Map of Cape Colony, 1873, published by Stanford, London.

In the southern portion of Cape Colony no formation has excited so much interest, or proved so inscrutable a puzzle to the earlier geologists, as the zone of rock named "Porphyry" and "Trap Conglomerate" by the late A. E. Bain, "Trappean Ash" by Wylie, "Metamorphic Rock" by Pinchin, &c., and called "Bushman Graves" by the Boers. In Natal the same formation occurs, and Dr. Sutherland (of that colony) was the first to consider it as possibly of glacial origin, but he obtained no direct evidence to support that view.

As the names applied to this singular conglomerate were all misleading in my Sketch Geological Map of 1875, I named it the "Dwyka Conglomerate," on account of the excellent and characteristic sections exposed where the river of that name cuts through it.

While at Matjes Fontein, Cape Colony, in June, 1885, I obtained the first evidences of glaciation in this southern extension of the conglomerate among the loose pebbles, and more abundant evidence at Prince Albert, close by. In my report of 1886 to the Cape Government, the full extent of these conglomerates in South Africa is shown. Incidental reference and sections of the conglomerate occur in my report to the Cape Government dated 1879. The full extent of the conglomerate is also shown in my Sketch Geological Map of South Africa of 1887, published by Sands and McDougall, Melbourne.

Australia.—In 1887 I obtained indubitable evidence of glaciation in the conglomerate of Worragee, near Beechworth, Victoria, and placed well-striated pebbles and boulders in the local museum and in the Technological Museum, Melbourne. These were the first glaciated stones discovered in the palæozoic conglomerates of Victoria. Shortly afterwards, on visiting Bacchus Marsh and the Wild Duck Creek, I obtained abundant and unchallengeable testimony to the glacial origin of these conglomerates also for the first time, although their glacial origin was suspected thirty years ago by Sir A. Selwyn and the late Mr. Daintree. A paper on the subject was read before the Royal Society of Victoria in 1887, and several localities besides the above described. Another was read before the Australasian Association Meeting, December, 1890. A special report on the Wild Duck Creek conglomerate, prepared in 1891 for the Geological Survey Department, was published in 1892.

Tasmania.—In October, 1892, I once more encountered this remarkable conglomerate at the base of Mount Reid, near Strahan, and at an elevation of 3000 feet above sea-level. At this site it corresponds in a remarkable manner with both the Dwyka conglomerate of South Africa and the Wild Duck Creek conglomerate of Victoria. At the same time, and at a few miles' distance, I discovered around Lake Kora very extensive and marvellously well developed evidences of modern glaciation on a large scale. These discoveries were made public through the press at Hobart and at Melbourne in the beginning of November following, and a paper and plan has been submitted to the Royal Society of Melbourne, and read. The whole of my reports and maps have been supplied to the Geological Society, Burlington House. E. J. DUNN.

Melbourne, July 15.

Astronomical Photography.

LORD RAYLEIGH, in his letter (August 24), raises the interesting question of the adaptability of the plate to the object-glass. This is a novel idea, and I hope with him that we shall have the opinion of Captain Abney or some other authority on the question, or that it will be settled experimentally whether the use of an object-glass corrected for visual work will give, with properly prepared plates, results approximating those obtained with the photographic object-glass. In the case of Cambridge Observatory, there is already an object-glass of nearly twice the area of the proposed photographic telescope, so that it is quite possible as good results might be obtained with the Newall telescope as with the proposed one.

With the collodion process, where the curve of sensibility of the photographed spectrum had a well-defined summit, the photographic object-glass corrected for that part left very little to be desired. Now, the curves of sensibility of the different kind of plates vary extremely. We have long flat curves, or curves with two maxima; in fact, there is such a range now that it is a matter of surprise to me that any object-glass produces such good results as are obtained. Some years ago, after reading Dr. H. W. Vogel's "Photography of Coloured Objects," I thought that astronomers would be driven to the use of the only instrument that will use any and every plate—the Reflector; or if they would use the object-glass, that they would have to first find the most sensitive plate, and then make their object-glass to suit it. They should be made to suit each other. If this can be done by a variation of the photographic process without paying too dearly for it in the loss of sensitiveness, a great deal will be gained in many ways.

The great doubt in my mind is whether it is possible to get rid of the blue rays without the use of screens.

In any case, the object-glass can never properly use all the available light in the way the Reflector does, and it is a matter of extreme surprise to me that, notwithstanding the magnificent results obtained by the Reflector in astronomical photography astronomers still seem to prefer the expensive object-glass.

Ealing, September 11. A. A. COMMON.

The Greatest Rainfall in Twenty-four hours.

As a resident of Dehra Dún, I was interested in a paragraph at p. 297 of NATURE for July 27, 1893, saying that the *Indian Planters' Gazette* had recorded a rainfall of 48 inches at Dehra Dún on the night of January 24, 1893. As 48 inches is considerably more than half our average yearly rainfall (86 inches). I have looked up the official returns of the Meteorological Reporter to the Government of India. They give for the rainfall recorded at 8 a.m. on January 24, 1893, 0·26 inches only, 1·07 inch being the recorded fall on the same date at Mussoorie, on the hill range 11 miles off. I have examined the Dehra Dún rainfall records since January 1, 1867, and find that the largest amount recorded for any one day since that date is 11·60 inches, which is given for July 30, 1890. It is possible that the correspondent referred to wrote 4·8 inches, but even that amount, though not an uncommon fall for the monsoon season between June and September inclusive, would be a heavy fall for January. The highest recorded fall for any day in January is 2·84 inches on January 26, 1883.

J. S. GAMBLE.

Imperial Forest School, Dehra Dún, Aug. 22.

[The paragraph in question was taken from the *Ceylon Observer*. We append it as it appeared in our issue for July 27, together with a remark we made at the time.—ED.]

"If the *Indian Planters' Gazette* of 28 Jan., 1893, is correct, the following paragraph establishes a still higher record. On page 59 one reads: 'Our Dera Doon correspondent writes on January 24, 1893: last night we had 48 inches of rain, and all the hills are covered with snow. It is still raining.'" For this to have any scientific value, however, it must be known who were the observers, and by what means the rainfall was gauged.

Wasps.

OF late much has been written about the seasonal prevalence of wasps, and the mischief, in several places, wrought by them. May not, however, their use in keeping down many forms of insect pests be set off as some sort of palliative? Wasps are exterminators of aphides, and although the season has been favourable to insect-life, next to no damage has been done to the hop-bines or the corn or pulse crops of Worcestershire or Herefordshire by these latter pests—frequent destroyers of crops.

Is it suggestible that the excessive wasp prevalence is attributable in some measure to the abundance of their insect prey, just as has recently happened in Scotland, in the instance of the multiplication of the short-eared or "Woodcock" owl, owing to the plague of field voles? The owl is a winter immigrant, usually leaving in spring. "Nests in ordinary seasons are of rare occurrence in Great Britain, but owing to the vast increase of their favourite food—the field vole—these owls have not only arrived in increased numbers, but have remained and bred in Scotland all over the affected districts, laying from eight to thirteen eggs, and rearing large broods," instead of the few eggs these owls have hitherto been accredited with laying.

I am a fruit-grower. Much damage has this year been done to the fruit; not, however, by the wasp tribe, but by hungry birds, the fruit having even been attacked in an unripe state. According to my experience wasps do not become household pests till the falling-off of insect prey towards autumn.

Worcester, September 1. J. LLOYD BOZWARD.

THE AMERICAN ASSOCIATION.

MADISON, Wisconsin, at which the forty-second meeting of the American Association for the Advancement of Science was held, August 17 to 22, is a beautiful little University town, surrounded by clear, glacial lakes, and is the capital of the State of Wisconsin.

Several causes conspired to reduce the attendance of members at this meeting—the distraction of the World's Fair at Chicago, the financial stringency, and the remoteness of the place of meeting from the sea-board, where most of the members reside; but it was characterised by an earnest tone and an excellent quality of scientific work.

At the opening session the retiring President, Prof. Joseph Le Conte, gracefully introduced his successor, Prof. William Harkness, by remarking that while he represented geology, the president-elect represented astronomy: one the oldest, the other among the youngest of sciences; one concerned with the universe of space, the other with the universe of time; one with the law of gravitation, the other with that of evolution; one with the divine method of sustentation, the other with the divine method of creation of the universe.

Addresses of welcome followed by Major Corscot, General Lucius Fairchild, chairman of the local committee, and President C. K. Adams, of the University of Wisconsin, where the meeting was held. The latter gave a brief account of the use of the University, which has always made science prominent, and remarked that we are doubtless on the eve of wonderful discoveries. Physics and chemistry bring us near to the ultimate analysis of matter.

President Harkness, in replying, referred among other things to the British Association for the Advancement of Science as the pioneer of all such organisations. The reports of the condition of science at its organisation, over sixty years ago, were still valuable, and the early star catalogues made under its auspices were a valuable contribution to advancing science. The matter of nomenclature of electrical units was settled by the British Association, and the names, watt, ohm, ampere volt, now universally adopted, originated there.

Thursday afternoon was occupied with the addresses of the several vice-presidents, some of which will be printed in full in later numbers of NATURE. The generally high order of these addresses was matter of comment among members. The subjects presented were "Variations of Latitude," by C. L. Doolittle; "Phenomena of the Time Infinitesimal," by E. L. Nichols; "Twenty-five Years' Progress in Analytical Chemistry," by Edward Hart; "Training in Engineering Science," by S. W. Robinson; "Geological Time as indicated by the Sedimentary Rocks in North America," by C. D. Walcott; "Rise of the Mammalia," by H. F. Osborn; "Evolution and Classification," by C. E. Bessey; "The Biloxi Indians of Louisiana," by J. O. Dorsey; "The Mutual Relations of Science and Stock Breeding," by Mrs. H. Brewer.

The annual address by the retiring president, Prof. Joseph Le Conte, in the evening, on "Present State of Science on the subject of the origin of mountain ranges," was a masterly presentation of that difficult problem by an authority recognised as such throughout the world. The evening sessions were held in the capitol.

The mornings and afternoons of Friday, Monday, and Tuesday were occupied with reading of papers in the several sections.

On Friday evening Dr. Daniel G. Brinton lectured on "The Earliest Men," reviewing the latest discoveries of anthropologists. He localises the first habitat of man in southern Europe or northern Africa, or on the continuation of these latitudes in western or central southern Asia. Man seems to have been evolved *per saltum* from the highest anthropoid animal in the glacial, or possibly just before the glacial epoch, giving an antiquity of 50,000 to 100,000 years. The earliest men, so far as can be ascertained, walked erect, had full foreheads, red hair, and blue or gray eyes, were about of the same size and general appearance as now, perhaps were not even hairy, were kind to each other, social and artistic, had some sort of language, and knew how to make fire. Dr. Brinton's lecture, startling to the uninitiated by the boldness of his conjectures, derived added interest from his subsequent election as president of the association. As an anthropologist and anthropological writer, he has long occupied a front rank. He is a resident of Philadelphia, and a graduate of Yale in the class of 1858. He is a physician by profession, and a native of Chester County, Pennsylvania, where he was born in 1837.

The social features of the meeting were thoroughly delightful. The excursion to which Saturday was devoted deserves special mention, both for its pleasant relaxation and for the scientific interest of the region visited. Taking the cars of the Milwaukee and St. Paul railroad, a favourite tourist route, well known in England as well as in America, which is the only railroad leading to the scenic wonderland selected by the local committee as the best exhibit they could make to their guests, the association first passed through the remarkable driftless area ten or fifteen miles from the city. This is a region much studied by geologists as one which escaped the ice covering which extended over all the rest of the country during the glacial epoch. A ride of an hour and a half brought the train to Kilbourn city, where steamboats were taken up the Wisconsin river a distance of several miles, through "the dells" of that river, which are an expression of erosion resulting from a diversion of the Wisconsin river from its pre-glacial channel by the ice, and by the massive moraines which it produced. The rocks are Cambrian sandstone, and they show false bedding on a magnificent scale.

The places for several subsequent meetings of the association seem to be pretty clearly indicated, though no appointments were absolutely made. The new building of the Brooklyn Institute furnishes a good occasion for a meeting at Brooklyn next year, especially since that is now the only large city in the United States and Canada which has never been visited, if we except San Francisco, to which cordial invitations for a meeting in 1895 have already been received. The policy of the association, ever since its reorganisation at Buffalo in 1866, has been to hold decennial meetings at that city, so that 1896 also seems to be thus provided for.

The officers elected for next year are—president, Daniel G. Brinton, Media, Pa.; vice-presidents (Section A), Geo. C. Comstock, Madison, Wis.; (B) William A. Rogers, Waterville, Me.; (C) Thomas H. Norton, Cincinnati, O.; (D) Mansfield Merriman, South Bethlehem, Pa.; (E) Samuel Calvin, Iowa City, Iowa; (F) Samuel H. Scudder, Cambridge, Mass.; (G) Lucien M. Underwood, Greencastle, Ind.; (H) Franz Boas, Worcester, Mass.; (I) Henry Farquhar, Washington, D.C.; permanent secretary, F. W. Putnam, Cambridge, Mass.; general secretary, H. L. Fairchild, Rochester, N.Y.; secretary of the council, J. I. Howe, Louisville, Ky.; secretaries of the sections; (A) W. W. Beman, Ann Arbor, Mich.; (B) Benjamin W. Snow, Madison, Wis.; (C) S. M. Babcock, Madison, Wis.; (D) John H. Kinealy, St. Louis, Mo.; (E) Wm. M. Davis, Cambridge, Mass.; (F) Wm. Libbey, jun., Princeton, N.J.; (G) Charles R. Barnes, Madison, Wis.; (H) Alexander F. Chamberlain, Worcester, Mass.; (I) Manly Miles, Lansing, Mich.; treasurer, Wm. Lilly, Manch Chunk, Pa.

In Section A (Astronomy and Mathematics) most of the papers were as usual highly technical. The president of the section, Prof. Doolittle, carried on the line of thought presented in his annual address, by a paper on "Latitude Determination at Bethlehem in 1892–3," in which he stated that the fluctuation in latitude thus far noticed does not exceed about 0.4", being therefore less than fifty feet.

An interesting session was held at the Observatory, where the astronomer in charge, Prof. George C. Comstock, read a paper on "A Determination of the Constant of Aberration," and exhibited the instrument employed. It is a modified form of the Loewy prism apparatus, attached to a six-inch equatorial telescope. The principal element of the apparatus is a system of mirrors so placed before the objective as to reflect into the telescope images of the stars which are to be observed. As in the case of a sextant, images of two stars are simultaneously visible, and the apparatus may be regarded as a large reflecting instrument employed like a sextant for the measurement of the angular distance between stars, but subject to the limitation that the distances to be measured must differ but little from 120.° What is thus lost in range of application is compensated by the high degree of precision attainable with the apparatus, a discussion of nearly a thousand observations indicating 0"·3 as the probable error for a single measured distance.

A preliminary discussion of a portion of these observations published in 1892 furnished for the value of the constant of aberration 20"·494. A more rigorous discussion of the whole body of data, taking into account a possible annual variation in the amount of the atmospheric refraction, furnishes a value differing from the preceding by less than a thousandth of a second of arc; but this result cannot be considered definitive, since a comparison of the measured distances with values computed from the known right ascensions and declinations of the stars, indicates the existence in the observations of a systematic error depending upon the amount by which the measured distance differs from 120°. Reasons for supposing the error to be of subjective origin were indicated.

A discussion of the data thus corrected furnishes as the value of the constant of aberration $20''{\cdot}445 \pm 0''{\cdot}010$

As subsidiary results of this investigation it appears that the variation in the amount of the refraction from winter to summer is better represented by Regnault's value of the co-efficient of expansion of air, 0·003670, than by the values adopted in the tables of Bessel and the Pulkowa Observatory. Also, the observations are in very close agreement with the absolute values of the Pulkowa refractions, but indicate sensible corrections to Bessel's tables.

Section B (Physics) was prolific of good scientific work. The stereopticon views, with which vice-president Nichols illustrated his annual address, were a revelation of the astounding resources of photography in depicting phenomena of infinitesimal time, the alternating electric current with light and dark intervals clearly depicted, the flight of a bullet and its attendant sound waves shown as if at rest. Prof. Nichols does not think that he has yet reached the limit of these investigations. Although some of the exposures could only have been for a few millionths of a second, they were always long enough to secure a negative.

Of equal, if not superior, merit was the delicate and accurate apparatus for measuring expansions, exhibited by Profs. E. W. Morley and Wm. A. Rogers, called the Morley interferential comparator. In a paper read before the section, Prof. Morley explained that he had first described the proposed apparatus before a meeting of the Civil Engineers' Club of Cleveland, and afterwards at the Toronto meeting of this association in 1889. It was first used in a simplified form, in an experiment on the magnetic field, by Profs. Morley and Eddy, which was reported to the association at the Indianapolis meeting in 1890. The present paper was designed to recall to mind the principle of the apparatus and method, as an introduction to a paper by Prof. Rogers, in which several series of experiments with it were detailed, and also as a preparation for an exhibition of one of two forms of the apparatus which have been constructed for use in measuring expansions. These have been constructed by Prof. Rogers, with the aid of a small grant from the research fund of this association. It will measure the expansion of a metallic bar five or ten times as accurately as by old methods, being only limited by the accuracy with which temperature can be measured. It consists of two metallic bars, one of steel and one of bronze, with mirrors at each end, so adjusted that any change in adjustment is indicated by interference fringes of sodium light; 90,000 such fringes to the inch may be readily distinguished and counted. The mirrors are probably the most delicate ever made, being plain within two millionths of an inch, thus far exceeding in accuracy the best objectives of the largest telescopes.

Prof. Rogers followed with a paper in which he said that preliminary to the actual use of the interferential comparator in physical measurements, it was necessary to establish three points with great certainty.

(1) Does the value of the relative change per degree in the length of steel and bronze bars of metal, expressed in terms of wave lengths, remain constant? (2) Does the relative length of the two bars compared remain constant at the same temperature after the mirrors have been subjected to extreme temperatures? (3) Does this relative remain constant after the positions of the mirrors have been changed by means of the adjusting screws provided?

As a result of many experiments, an affirmative answer can be given to the two first inquiries. The change for each degree Centigrade was proved to be 38·31 fringes of sodium light for the steel bar, and 64·23 fringes for the bronze bar of Bailey's metal. When the observed differences in length were reduced to 5·1, the point at which the two bars had nearly the same length, it was found that the average probable error in a single comparison was about 0·72 of a single wave length, including all observations at wide ranges of temperature.

The answer to the third inquiry was less satisfactory, as occasional changes of ten fringes were obtained. The source of this error has, however, been found. In the new vacuo apparatus, the mirrors have been matched with great exactness. It was then found that the previous matching had been defective. Prof. Morley has computed the maximum effect of this error in changing the apparent relative lengths of the two bars, and has found it to be fifteen fringes.

The following are a few of the problems to the solution of which the apparatus has been applied:—

(1) The determination of the effect of slow changes in temperature upon the relative lengths of the two bars compared.

(2) The cooling effect of evaporation from a body of water placed near one of the bars.

(3) Measurement of slow changes in the bars compared due to the near presence of the observer.

(4) Measurement of the effect of obscure rays of heat stored in large masses of matter in close proximity.

(5) Measurement of the effect of flexure in changing the length of one of the bars.

(6) Measurement of changes in length produced by placing one of the bars in a magnetic field.

(7) Measurement of the heating effect of a current passed through one of the bars.

(8) Determination of the time required for the complete dissipation of a given amount of heat quickly applied to the bars.

(9) Proof that air is practically a non-conductor of heat.

(10) Determination of the value of 100 mikrons in terms of wave lengths of sodium and mercury fringes.

Prof. Alexander Macfarlane read a paper on the addition or composition of physical quantities, treating of one uniform method of the addition or composition of scalar quantities at different points, of vector quantities at the same point, of vector quantities at different points, of finite rotations round intersecting axes, of finite rotations round non-intersecting axes, and finally of screw motions. The screw motions compounded are not infinitesimal, but may be of any magnitude.

Profs. Macfarlane and G. W. Pierce contributed a paper on the electric strength of solid, liquid, and gaseous dielectrics, in which it was maintained that for a stratum of air or other gas between two parallel metal plates the electrostatic gradient when the spark passes is less the greater the distance between the plates; but for paraffined or beeswaxed paper this gradient is constant; it is also constant for paraffin oil or kerosene. The anomalous behaviour of the gaseous dielectric appears to be due to the greater freedom of motion of the molecules.

Mr. Joseph O. Thompson read a paper on "Fatigue in the Elasticity of Stretching." He remarked that attention was first called to the phenomena of elastic fatigue by Lord Kelvin some twenty-eight years ago. He used the elasticity of torsion in his experiments, and demonstrated that in some cases fatigue diminished the slide modulus as much as 6 per cent. Prof. Thompson's paper called attention to the fact hitherto undiscovered that a similar fatigue can be observed in the elasticity of stretching. Its influence in diminishing the Young's modulus amounted in these experiments to less than $\frac{1}{4}$ of 1 per cent. The wires used were 23m. long, and the metals in which the phenomenon was observed were silver, steel, and brass.

Messrs. F. Bedell, K. B. Miller, and W. F. Wagner contributed an elaborate mathematical paper on "Irregularities in Alternate Current Curves."

At the meeting of Section C (Chemistry) the notable feature was the presentation of Prof. Morley's final determination of the atomic weight of oxygen, giving results obtained by four distinct methods of investigation and with a degree of accuracy that will render this a final determination of this weight, correct to the third decimal figure. Three years ago Prof. Morley submitted a preliminary report, in which an account was given of the determination of the ratio of densities of oxygen and hydrogen as 15·884, correct within one part in four thousand. It has since been found that an accident happened to the apparatus during the last experiment of the series, which ought therefore to have been rejected. If this were now to be done, the value would become 15·882.

Two years ago some account was given of a series of determinations of the quantities of water produced from weighed quantities of oxygen and of hydrogen. Twelve experiments were made. In one the quantity of water produced was not determined, owing to accident. From the weights of hydrogen and oxygen consumed, the atomic weight of oxygen was found to be 15·8794, with a mean error of one part in 16,000 for a single experiment. From the quantities of hydrogen used and of water produced, the value obtained was 15·8792, with a mean error of one part in 7500 for a single determination.

At the present meeting, Prof. Morley reported the result of twenty determinations of the absolute density of oxygen, and twenty of that of hydrogen. The ratio of these densities found was 15·882.

If now the ratio of the volumes in which oxygen and hydrogen combine is substantially that found in these experiments, the atomic weight of oxygen computed from the densities would be 15·882 from the former series of determinations (or 15·880, if the correction is allowable), and 15·880 from the present series, we should then have :—

15·879, from ratio of H to O
15·879 ,, ,, H to H_2O
15·882 [or 15·880] ratio of densities (*a*)
15·880 from ,, ,, (*b*)

as the result so far of Prof. Morley's work.

But the later work of Scott has attained a high degree of excellence, and gives a value of the ratio of the volumes in which the gases combine, which is considerably higher than that used in this computation. Prof. Morley explained that he had himself published every experiment which he had ever made on this point, and that they had a mean error of only one part in 26,000. Since no source of constant error had yet been pointed out, he had great confidence in the accuracy of his own experiments. He, however, intended to make another series of determinations with the apparatus used before, and one with a new apparatus now constructing.

He also mentioned three other series of determinations which he is now carrying on; two are determinations of the absolute density of hydrogen, and one a determination of that of oxygen; in these a very small mean error is attainable.

Among the other papers which attracted special attention, were one on "The Constitution of Paraldehyde and Metaldehyde," by W. R. Orndorff and John White; and one on "Solubility of Lead Oxide in the normal tartrates and other normal organic salts, with observations on the rotary power of the solutions thus obtained," by L. Kahlenberg and H. W. Hillyer.

In Section D (Mechanical Science and Engineering) the number of papers was small, owing to the increasing tendency of engineers to support special technical associations.

Messrs. Wm. S. Rogers, S. W. Robinson, and J. Burkitt Webb contributed useful notes on different topics; while the secretary of the section, Prof. D. S. Jacobus, read three papers describing ingenious apparatus devised and used by him at the Stevens Institute of Technology at Hoboken, N.J.

Among the papers read, we note one by Prof. J. J. Stevenson on "the use of the term Catskill," in which he offered strong objection to the application of this term to the whole series of rocks from the Hamilton to the lower carboniferous, as has been recently advocated. Since the group is well defined below, and since the geographical term Catskill represents conditions which prevailed over an extended area only during the latter part of the upper Devonian period, Prof. Stevenson thinks that the term should be restricted as defined by Vonuxem.

Mr. J. A. Holmes gave an interesting description of a map and section of the stratified rocks of the coastal plain of southern North Carolina. Mr. William Hallock reported the results of further observations of temperature in the deep well at Wheeling, W.Va. Since 1891 this well has filled with water by leaking below the surface. Temperature determinations have been made in the water, which are practically identical with the determinations made when the well was filled with air two years ago, showing that there is not an appreciable circulation of water in a hole five inches in diameter. Down to 3200 feet the gradient is 1° F. to 81·5 feet, and near the bottom 1° F. to each 60 feet.

Dr. C. R. Van Hise, referring to the "character of the folds in Marquette iron district," called attention to the fact that what has been considered a synclinal is really a great synclinorium, having a nearly east-west axis, and having both the north and the south limbs pushed under, producing a complex fold with overturned minor folds, and comparable to some of those which Heim has described from the Alps.

Prof. C. D. Walcott exhibited beautiful specimens of trilobites which he had collected from the Utica shale of New York, on which the antennæ and legs were remarkably well preserved. Mr. F. P. Gulliver exhibited beautiful papier maché models, one of the sand plain at Newtonville, Mass., and a second showing the theoretical conditions at the time of its formation.

A paper entitled "Additional Facts Bearing on the Unity of the Glacial Period," was read by Prof. G. F. Wright, consecutively with one by Frank Leverett on "Changes of Drainage in the Rock River Basin in Illinois." The latter is important as affording means of estimating the amount of erosion in inter-glacial compared with that of post-glacial time. The wide pre-glacial channel of the Rock is followed to the Green River Basin near Inlet Swamp, when it is choked up by accumulations of drift. The change to the present course is located early in the glacial period, since the present valley can be shown to have been opened to about its present size and depth prior to the formation of the kettle moraine of the Green Bay lobe, the gravels which occupy the new course of the river being derived from the ice-sheet at the time the moraine was forming near the head waters of the river. These gravels are traceable up to the head of the moraine as a moraine-headed terrace. It is found that the post-glacial erosion in the river valley is only one-half that accomplished in inter-glacial time, and whereas the post-glacial erosion is mainly in gravel and sand, the inter-glacial erosion was mainly in rock strata. This seems to Mr. Leverett to warrant the use of the term epoch rather than episode to characterise these time relations.

Mr. Warren Upham, in his paper on "Tertiary and Quarternary Stream Erosion in North America," argued from stream erosion that an epeirogenic uplift preceded and probably produced the glacial epoch.

Section F (Zoology) having been severed from botany by the new amendment to the constitution, had comparatively few papers. The president, Prof. H. F. Osborn, carried on the line of thought contained in his annual address, by a paper on "The Mammals of the Upper Cretaceous," in which he proposed a system of classification and evolution materially differing from that of Prof. Marsh, which has so long held its ground. Prof. Osborn's studies lead him to more confidence in the belief that early forms are in many cases pretty highly specialised, and that evolution by degradation plays a pretty important part in biological investigation. This is quite in harmony with the statement of the president-elect of the association, Dr. Brinton, in his public address on "The Earliest Men," above noted, to the effect that the evolution of man appears to have been *per saltum*.

Section G (Botany) was organised at this meeting by division of the old section of biology, and considered a large number of papers of technical interest. Among the contributors were Arthur, Beal, Galloway, Dr. and Mrs. Britton, Barnes, Halstead, MacMillan, Coville. Dr. Britton discussed the question of nomenclature.

Probably the proceedings of the Botanical Club were even more interesting to botanists than those of the section, inasmuch as the club organised the Botanical Society of America with twenty-five charter members. Dr. Arthur exhibited to the club two very interesting pieces of apparatus, one a rotatory machine in which a germinating seed may be placed and subjected for hours or days to centrifugal force instead of gravitation. This apparatus gives the interesting result that the roots grow in the direction of the centrifugal force, and the leaves opposed to it. The other apparatus, called an auxanometer, shows by ingenious automatic action the rate of growth of plants.

Section H (Anthropology) furnished the largest number of papers. The first paper read in the section, by Washington Matthews, on "Songs of Sequence of the Navajos," was illustrated by reproductions of the songs by the phonograph. Dr. Joseph Jastrow gave an account of the system of psychologic investigation now pursued at the World's Fair. The recent discoveries resulting from excavations at the ancient argillite quarries on Geddes' Run, near the Delaware River, were presented by H. C. Mercer; and Ernest Volk made some observations in regard to the use of argillite by prehistoric people, as illustrated by explorations in the Delaware Valley. H. N. Rust read several papers on California Indians and implements. Prof. G. F. Wright presented a summary of the evidence in favour of the existence of glacial man in America, which commanded general attention because of the personal abuse to which Prof. Wright has recently been subjected. The subject was discussed at some length, and Prof. Wright's conclusions were violently attacked by Mr. McGee. Dr. Brinton read a paper on the "Mexican Calendar System," which he pronounces an anomaly, having no relation to the period either of solar or lunar revolution. It consists of 20 × 13, or 260 days. The 20 is a double digital basis. The 13 seems inexplicable.

The excursion of this section on Monday afternoon gave an opportunity to visit a group of effigy mounds just across Lake

Mendota, about four miles from the University. These mounds are of different shapes, that of the panther predominating, though birds and conical mounds are found also.

Section I (Economic Science and Statistics) had but few papers to consider, of which that of Mr. Henry Farquhar, on "Relations of Production and Price of Silver and Gold," introduced the topic of most general interest just now. The fallacy of attempting to maintain a silver standard of value was very apparent from the paper and the ensuing discussion. Improvements in metallurgy reduce the cost and vastly increase the production of silver, while that of gold remains almost stationary, there being really hardly any metallurgy of gold.

WM. H. HALE.

BRITISH ASSOCIATION.

NOTTINGHAM, SEPTEMBER 13.

THE meeting of the Association, which commences to-day, will take place mainly in the University College. In this building all the Sections, with the exception of the geographical, economical, and anthropological, will assemble. The Sections representing the experimental sciences will be accommodated in lecture theatres built and furnished for the express purpose of illustrating and demonstrating these sciences. Every convenience will therefore be afforded in the meeting rooms for the proper illustration of the papers which will be communicated. Further, the students' laboratories, which are in immediate connection with these theatres, will furnish most convenient exhibition rooms for the illustrative apparatus, specimens, and diagrams during the week of meeting, and when they are not required for illustration in the sectional room. The College will thus become the scientific headquarters during the meeting. It will in addition furnish convenient sectional committee rooms, sectional secretaries' rooms, anthropometric laboratory, ladies' boudoir, smoking-room, convenient retiring rooms, and a large luncheon buffet in the attached public lending library.

It is interesting to compare the facilities now offered for the meeting with those afforded during the preceding meeting in 1866. A temporary exhibition building then stood on the College site, and was used for the *conversazione*, but no suitable meeting rooms existed in the town for housing Sections A, B, C, D, and G. It will scarcely be necessary to inform those interested in the advancement of science that the existence of the College is due to the public spirit of the inhabitants of Nottingham, who willingly voted public money to establish the College, and who now mainly support it from the local rates. That such a bold experiment has met with the full success which it deserved, members of the Association who visit the town will learn and see for themselves. They will find that the initial success is leading to further success, and that outside support from the Government, from the Drapers' Company, and from other sources, is now being accorded with an ungrudging hand. It may be said with truth that since the Association last met in Nottingham, the town has become in a very important sense a centre for the advancement of science, and fully deserves all the encouragement and impetus which will be given to its comparatively new scientific work and aims by the visit of the Association.

It may be added that the Sections which meet outside the College are also accommodated in halls which were non-existent at the previous meeting in Nottingham, and that the evening meetings will take place in a large hall, which is new in the same sense. This will give some idea of the rapid progress which the town has made during the last quarter of a century.

Coming, as the Nottingham meeting does, between meetings at the venerable University towns of Edinburgh and Oxford, the status of the University College of the town must necessarily suffer by comparison. But it will be found that Nottingham, like the other provincial towns which have recently founded colleges in their midst, is by no means altogether at a disadvantage as regards its higher education by making a late start. In the matter of buildings and equipments it has benefited by the experience of its predecessors; and the absence of the fetters of an ancient *régime* has left it free to adapt its curriculum and methods to the needs of the present day.

With respect to the prospective work of the meeting, it may be stated that it promises to be fully up to the average in importance and in interest. A general statement of the papers to be brought forward, and of the discussions in the different Sections, has appeared in NATURE from time to time, and it is unnecessary to repeat the announcement of these in detail. It will be sufficient to remind members that in Section A questions of great interest and importance are put down for discussion; that in Section B, M. Moissan will d monstrate the preparation and properties of fluorine, a demonstration of absolutely unique interest, since this is the first opportunity afforded in this country of seeing these remarkable experiments. The President of the Section C and his colleagues have been most energetic in securing the attendance of distinguished foreign geologists, and in procuring numerous papers of local geological interest, in addition to discussions on points of general importance. In Section D, which will have the advantage of securing the special interest and support of the President of the Association, there will undoubtedly be good discussion of important biological problems, not only by Englishmen, but also by eminent continental biologists, who are guests of the town. In Section E the travellers are mustering in force, and will have their tales to tell of widely distant parts of the earth's surface; the photographs and paintings prepared in Antarctic regions will be of special interest in this section. Economic problems of the day are to be discussed in Section F. Section G will be represented by many eminent engineers, both English and foreign, and the experimental illustration of many of the papers, rendered possible by the meeting being held in a well-equipped engineering theatre, will add interest to the proceedings. In Section H the paper by Dr. Hans Hildebrand, and the description of the Glastonbury marsh village by Mr. Bulleid, with the discussions which they will undoubtedly give rise to, would, if they stood alone, constitute a tempting programme to anthropologists.

The efforts put forth in the town itself to make the gathering pleasant and successful will perhaps be best appreciated by reference to the local programme and maps now being issued to members. The townsmen have vindicated their character for hospitality by privately entertaining in their homes nearly 400 of their visitors. An ample list of hotels and lodgings, with a suitable map, has been issued for some weeks, some of the hotels binding themselves to a special tariff to members who engage their rooms through the local committee. The garden parties, excursions, and entertainments will be seen to have been so arranged as to leave no irksome leisure to be filled in by those who have done their duty to their Sections; and the scheme for privately engaging the Theatre Royal for the last Wednesday night will, it is hoped, justify by its success its boldness and originality.

With a programme of work of varied special and general interest and importance; with a universal desire on the part of the townsmen to do everything in their power to secure the comfort of their guests, and to afford pleasure and recreation to them; with the social element of the scientific gathering secured by the promise of attendance of men of science from all parts of our own country and from abroad; and, above all, with the promise of fine autumnal weather in a healthy, picturesque, and accessible town with most interesting surroundings, it will be strange indeed if the Nottingham meeting of 1893 should not become a record meeting, remembered by the pleasure and satisfaction it has given, if not by the largeness of the number who attend it.

Frank Clowes.

Inaugural Address by J. S. Burdon-Sanderson, M.A., M.D., LL.D., D.C.L., F.R.S., F.R.S.E., Professor of Physiology in the University of Oxford, President.

We are assembled this evening as representatives of the sciences—men and women who seek to advance knowledge by scientific methods. The common ground on which we stand is that of belief in the paramount value of the end for which we are striving, of its inherent power to make men wiser, happier, and better; and our common purpose is to strengthen and encourage one another in our efforts for its attainment. We have come to learn what progress has been made in departments of knowledge which lie outside of our own special scientific interests and occupations, to widen our views, and to correct whatever misconceptions may have arisen from the necessity which limits each of us to his own field of study; and, above all, we are here for the purpose of bringing our divided energies into effectual and combined action.

Probably few of the members of the Association are fully aware of the influence which it has exercised during the last half-century and more in furthering the scientific development of this country. Wide as is the range of its activity, there has been no great question in the field of scientific inquiry which it has failed to discuss; no important line of investigation which it has not promoted; no great discovery which it has not welcomed. After more than sixty years of existence it still finds itself in the energy of middle life, looking back with satisfaction to what it has accomplished in its youth, and forward to an even more efficient future. One of the first of the national associations which exist in different countries for the advancement of science, its influence has been more felt than that of its successors because it is more wanted. The wealthiest country in the world, which has profited more—vastly more—by science than any other, England stands alone in the discredit of refusing the necessary expenditure for its development, and cares not that other nations should reap the harvest for which her own sons have laboured.

It is surely our duty not to rest satisfied with the reflection that England in the past has accomplished so much, but rather to unite and agitate in the confidence of eventual success. It is not the fault of governments, but of the nation, that the claims of science are not recognised. We have against us an overwhelming majority of the community, not merely of the ignorant, but of those who regard themselves as educated, who value science only in so far as it can be turned into money; for we are still in great measure—in greater measure than any other—a nation of shopkeepers. Let us who are of the minority—the remnant who believe that truth is in itself of supreme value, and the knowledge of it of supreme utility—do all that we can to bring public opinion to our side, so that the century which has given Young, Faraday, Lyell, Darwin, Maxwell, and Thomson to England, may before it closes see us prepared to take our part with other countries in combined action for the full development of natural knowledge.

Last year the necessity of an imperial observatory for physical science was, as no doubt many are aware, the subject of a discussion in Section A, which derived its interest from the number of leading physicists who took part in it, and especially from the presence and active participation of the distinguished man who is at the head of the National Physical Laboratory at Berlin. The equally pressing necessity for a central institution for chemistry, on a scale commensurate with the practical importance of that science, has been insisted upon in this Association and elsewhere by distinguished chemists. As regards biology I shall have a word to say in the same direction this evening. Of these three requirements it may be that the first is the most pressing. If so, let us all, whatever branch of science we represent, unite our efforts to realise it, in the assurance that if once the claim of science to liberal public support is admitted, the rest will follow.

In selecting a subject on which to address you this evening, I have followed the example of my predecessors in limiting myself to matters more or less connected with my own scientific occupations, believing that in discussing what most interests myself I should have the best chance of interesting you. The circumstance that at the last meeting of the British Association in this town, Section D assumed for the first time the title which it has since held, that of the Section of Biology, suggested to me that I might take the word "biology" as my starting-point, giving you some account of its origin and first use, and of the relations which subsist between biology and other branches of natural science.

Origin and Meaning of the Term "Biology."

The word "biology," which is now so familiar as comprising the sum of the knowledge which has as yet been acquired concerning living nature, was unknown until after the beginning of the present century. The term was first employed by Treviranus, who proposed to himself as a life-task the development of a new science, the aim of which should be to study the forms and phenomena of life, its origin and the conditions and laws of its existence, and embodied what was known on these subjects in a book of seven volumes, which he entitled "Biology, or the Philosophy of Living Nature." For its construction the material was very scanty, and was chiefly derived from the anatomists and physiologists. For botanists were entirely occupied in completing the work which Linnæus had begun, and the scope of zoology was in like manner limited to the description and classification of animals. It was a new thing to regard the study of living nature as a science by itself, worthy to occupy a place by the side of natural philosophy, and it was therefore necessary to vindicate its claim to such a position. Treviranus declined to found this claim on its useful applications to the arts of agriculture and medicine, considering that to regard any subject of study in relation to our bodily wants—in other words to utility—was to narrow it, but dwelt rather on its value as a discipline and on its surpassing interest. He commends biology to his readers as a study which, above all others, "nourishes and maintains the taste for simplicity and nobleness; which affords to the intellect ever new material for reflection, and to the imagination an inexhaustible source of attractive images."

Being himself a mathematician as well as a naturalist, he approaches the subject both from the side of natural philosophy and from that of natural history, and desires to found the new science on the fundamental distinction between living and non-living material. In discussing this distinction, he takes as his point of departure the constancy with which the activities which manifest themselves in the universe are balanced, emphasising the impossibility of excluding from that balance the vital activities of plants and animals. The difference between vital and physical processes he accordingly finds, not in the nature of the processes themselves, but in their co-ordination; that is, in their adaptedness to a given purpose, and to the peculiar and special relation in which the organism stands to the external world. All of this is expressed in a proposition difficult to translate into English, in which he defines life as consisting in the reaction of the organism to external influences, and contrasts the uniformity of vital reactions with the variety of their exciting causes.[1]

The purpose which I have in view in taking you back as I have done to the beginning of the century, is not merely to commemorate the work done by the wonderfully acute writer to whom we owe the first scientific conception of the science of life as a whole, but to show that this conception, as expressed in the definition I have given you as its foundation, can still be accepted as true. It suggests the *idea of organism* as that to which all other biological ideas must relate. It also suggests, although perhaps it does not express it, that *action* is not an attribute of the organism but of its essence—that if, on the other hand, protoplasm is the basis of life, life is the basis of protoplasm. Their relations to each other are reciprocal. We think of the visible structure only in connection with the invisible process. The definition is also of value as indicating at once the two lines of inquiry into which the science has divided by the natural evolution of knowledge. These two lines may be easily educed from the general principle from which Treviranus started, according to which it is the fundamental characteristic of the organism that all that goes on in it is to the advantage of the whole. I need scarcely say that this fundamental conception of organism has at all times presented itself

[1] "Leben besteht in der Gleichförmigkeit der Reaktionen bei ungleichförmigen Einwirkungen der Aussenwelt."—Treviranus, *Biologie oder Philosophie der lebenden Natur*, Göttingen, 1802, vol. i. p. 83.

to the minds of those who have sought to understand the distinction between living and non-living. Without going back to the true father and founder of biology, Aristotle, we may recall with interest the language employed in relation to it by the physiologists of three hundred years ago. It was at that time expressed by the term *consensus partium*—which was defined as the concurrence of parts in action, of such a nature that each does *quod suum est*, all combining to bring about one effect "as if they had been in secret council," but at the same time *constanti quadam naturæ lege.*[1] Prof. Huxley has made familiar to us how a century later Descartes imagined to himself a mechanism to carry out this *consensus*, based on such scanty knowledge as was then available of the structure of the nervous system. The discoveries of the early part of the present century relating to reflex action and the functions of sensory and motor nerves, served to realise in a wonderful way his anticipations as to the channels of influence, afferent and efferent, by which the *consensus* is maintained; and in recent times (as we hope to learn from Prof. Horsley's lecture on the physiology of the nervous system) these channels have been investigated with extraordinary minuteness and success.

Whether with the old writers we speak about *consensus*, with Treviranus about *adaptation*, or are content to take *organism* as our point of departure, it means that, regarding a plant or an animal as an organism, we concern ourselves primarily with its activities, or, to use the word which best expresses it, its energies. Now the first thing that strikes us in beginning to think about the activities of an organism is that they are naturally distinguishable into two kinds, according as we consider the action of the whole organism in its relation to the external world or to other organisms, or the action of the parts or organs in their relation to each other. The distinction to which we are thus led between the *internal* and *external* relations of plants and animals has of course always existed, but has only lately come into such prominence that it divides biologists more or less completely into two camps—on the one hand those who make it their aim to investigate the actions of the organism and its parts by the accepted methods of physics and chemistry, carrying this investigation as far as the conditions under which each process manifests itself will permit; on the other, those who interest themselves rather in considering the place which each organism occupies, and the part which it plays in the economy of nature. It is apparent that the two lines of inquiry, although they equally relate to what the organism *does*, rather than to what it *is*, and therefore both have equal right to be included in the one great science of life, or biology, yet lead in directions which are scarcely even parallel. So marked, indeed, is the distinction, that Prof. Haeckel some twenty years ago proposed to separate the study of organisms with reference to their place in nature under the designation of "œcology," defining it as comprising "the relations of the animal to its organic as well as to its inorganic environment, particularly its friendly or hostile relations to those animals or plants with which it comes into direct contact."[2] Whether this term expresses it or not, the distinction is a fundamental one. Whether with the œcologist we regard the organism in relation to the world, or with the physiologist as a wonderful complex of vital energies, the two branches have this in common, that both studies fix their attention, not on stuffed animals, butterflies in cases, or even microscopical sections of the animal or plant body—all of which relate to the framework of life—but on life itself.

The conception of biology which was developed by Treviranus as far as the knowledge of plants and animals which then existed rendered possible, seems to me still to express the scope of the science. I should have liked, had it been within my power, to present to you both aspects of the subject in equal fulness; but I feel that I shall best profit by the present opportunity if I derive my illustrations chiefly from the division of biology to which I am attached—that which concerns the *internal* relations of the organism, it being my object not to specialise in either direction, but as Treviranus desired to do, to regard it as part—surely a very important part—of the great science of nature.

The origin of life, the first transition from non-living to living, is a riddle which lies outside of our scope. No seriously-minded person, however, doubts that organised nature as it now presents itself to us has become what it is by a process of gradual perfecting or advancement, brought about by the elimination of those organisms which failed to obey the fundamental principle of adaptation which Treviranus indicated. Each step, therefore, in this evolution is a reaction to external influences, the motive of which is essentially the same as that by which from moment to moment the organism governs itself. And the whole process is a necessary outcome of the fact that those organisms are most prosperous which look best after their own welfare. As in that part of biology which deals with the internal relations of the organism, the interest of the individual is in like manner the sole motive by which every energy is guided. We may take what Treviranus called *selfish* adaptation—*Zweckmässigkeit für sich selber*—as a connecting link between the two branches of biological study. Out of this relation springs another which I need not say was not recognised until after the Darwinian epoch—that I mean, which subsists between the two evolutions, that of the race and that of the individual. Treviranus, no less distinctly than his great contemporary Lamarck, was well aware that the affinities of plants and animals must be estimated according to their developmental value, and consequently that classification must be founded on development; but it occurred to no one what the real link was between descent and development; nor was it, indeed, until several years after the publication of the "Origin" that Haeckel enunciated that "biogenetic law," according to which the development of any individual organism is but a memory, a recapitulation by the individual of the development of the race—of the process for which Fritz Müller had coined the excellent word "phylogenesis"; and that each stage of the former is but a transitory reappearance of a bygone epoch in its ancestral history. If, therefore, we are right in regarding ontogenesis as dependent on phylogenesis the origin of the former must correspond with that of the latter; that is, on the power which the race or the organism at every stage of its existence possesses of profiting by every condition or circumstance for its own advancement.

From the short summary of the connection between different parts of our science you will see that biology naturally falls into three divisions, and these are even more sharply distinguished by their methods than by their subjects; namely, *Physiology*, of which the methods are entirely experimental; *Morphology*, the science which deals with the forms and structure of plants and animals, and of which it may be said that the body is anatomy, the soul, development; and finally, *Œcology*, which uses all the knowledge it can obtain from the other two, but chiefly rests on the exploration of the endless varied phenomena of animal and plant life as they manifest themselves under natural conditions. This last branch of biology—the science which concerns itself with the external relations of plants and animals to each other, and to the past and present conditions of their existence—is by far the most attractive. In it those qualities of mind which especially distinguish the naturalist find their highest exercise, and it represents more than any other branch of the subject what Treviranus termed the "philosophy of living nature." Notwithstanding the very general interest which several of its problems excite at the present moment I do not propose to discuss any of them, but rather to limit myself to the humbler task of showing that the fundamental idea which finds one form of expression in the world of living beings regarded as a whole—the prevalence of the best—manifests itself with equal distinctness, and plays an equally essential part in the internal relations of the organism in the great science which treats of them—Physiology.

Origin and Scope of Modern Physiology.

Just as there was no true philosophy of living nature until Darwin, we may with almost equal truth say that physiology did not exist as a science before Johannes Müller. For although the sum of his numerous achievements in comparative anatomy and physiology, notwithstanding their extraordinary number and importance, could not be compared for merit and fruitfulness with the one discovery which furnished the key to so many riddles, he, no less than Darwin, by his influence on his successors was the beginner of a new era.

Müller taught in Berlin from 1833 to 1857. During that time a gradual change was in progress in the way in which biologists regarded the fundamental problem of life. Müller him-

[1] Bausner, *De Consensu Partium Humani Corporis*, Amst., 1556, Præf. ad lectorem, p. 4.

[2] These he identifies with "those complicated mutual relations which Darwin designates as conditions of the struggle for existence." Along with chorology—the distribution of animals—œcology constitutes what he calls *Relations-physiologie*. Haeckel, "Entwickelungsgang u. Aufgaben der Zoologie," *Jenaische Zeitschr.* vol. v. 1869, p. 353.

self, in common with Treviranus and all the biological teachers of his time, was a vitalist, *i.e.* he regarded what was then called the *vis vitalis*—the *Lebenskraft*—as something capable of being correlated with the physical forces; and as a necessary consequence held that phenomena should be classified or distinguished, according to the forces which produced them, as vital or physical, and that all those processes—that is groups or series of phenomena in living organisms—for which, in the then very imperfect knowledge which existed, no obvious physical explanation could be found, were sufficiently explained when they were stated to be dependent on so-called vital laws. But during the period of Müller's greatest activity times were changing, and he was changing with them. During his long career as professor at Berlin he became more and more objective in his tendencies, and exercised an influence in the same direction on the men of the next generation, teaching them that it was better and more useful to observe than to philosophise; so that, although he himself is truly regarded as the last of the vitalists—for he was a vitalist to the last—his successors were adherents of what has been very inadequately designated the mechanistic view of the phenomena of life. The change thus brought about just before the middle of this century was a revolution. It was not a substitution of one point of view for another, but simply a frank abandonment of theory for fact, of speculation for experiment. Physiologists ceased to theorise because they found something better to do. May I try to give you a sketch of this era of progress?

Great discoveries as to the structure of plants and animals had been made in the course of the previous decade, those especially which had resulted from the introduction of the microscope as an instrument of research. By its aid Schwann had been able to show that all organised structures are built up of those particles of living substance which we now call cells, and recognise as the seats and sources of every kind of vital activity. Hugo Mohl, working in another direction, had given the name "protoplasm" to a certain hyaline substance which forms the lining of the cells of plants, though no one as yet knew that it was the essential constituent of all living structures—the basis of life no less in animals than in plants. And, finally, a new branch of study—histology—founded on observations which the microscope had for the first time rendered possible, had come into existence. Bowman, one of the earliest and most successful cultivators of this new science, called it physiological anatomy,[1] and justified the title by the very important inferences as to the secreting function of epithelial cells and as to the nature of muscular contraction, which he deduced from his admirable anatomical researches. From structure to function, from microscopical observation to physiological experiment, the transition was natural. Anatomy was able to answer some questions, but asked many more. Fifty years ago physiologists had microscopes but had no laboratories. English physiologists—Bowman, Paget, Sharpey—were at the same time anatomists, and in Berlin Johannes Müller, along with anatomy and physiology, taught comparative anatomy and pathology. But soon that specialisation which, however much we may regret its necessity, is an essential concomitant of progress, became more and more inevitable. The structural conditions on which the processes of life depend, had become, if not known, at least accessible to investigation; but very little indeed had been ascertained of the nature of the processes themselves—so little indeed that if at this moment we could blot from the records of physiology the whole of the information which had been acquired, say in 1840, the loss would be difficult to trace—not that the previously known facts were of little value, but because every fact of moment has since been subjected to experimental verification. It is for this reason that, without any hesitation, we accord to Müller and to his successors Brücke, du Bois-Reymond, Helmholtz, who were his pupils, and Ludwig, in Germany, and to Claude Bernard[2] in France, the title of founders of our science. For it is the work which they began at that remarkable time (1845–55), and which is now being carried on by their pupils or their pupils' pupils in England, America, France, Germany, Denmark, Sweden, Italy, and even in that youngest contributor to the advancement of science, Japan, that physiology has been gradually built up to whatever completeness it has at present attained.

What were the conditions which brought about this great advance which coincided with the middle of the century? There is but little difficulty in answering the question. I have already said that the change was not one of doctrine, but of method. There was, however, a leading idea in the minds of those who were chiefly concerned in bringing it about. That leading notion was that, however complicated may be the conditions under which vital energies manifest themselves, they can be split into processes which are identical in nature with those of the non-living world, and, as a corollary to this, that the analysing of a vital process into its physical and chemical constituents, so as to bring these constituents into measurable relation with physical or chemical standards, is the only mode of investigating them which can lead to satisfactory results.

There were several circumstances which at that time tended to make the younger physiologists (and all of the men to whom I have just referred were then young) sanguine, perhaps too sanguine, in the hope that the application of experimental methods derived from the exact sciences would afford solutions of many physiological problems. One of these was the progress which had been made in the science of chemistry, and particularly the discovery that many of the compounds which before had been regarded as special products of vital processes could be produced in the laboratory, and the more complete knowledge which had been thereby acquired of their chemical constitutions and relations. In like manner, the new school profited by the advances which had been made in physics, partly by borrowing from the physical laboratory various improved methods of observing the phenomena of living beings, but chiefly in consequence of the direct bearing of the crowning discovery of that epoch (that of the conservation of energy) on the discussions which then took place as to the relations between vital and physical forces; in connection with which it may be noted that two of those who (along with Mr. Joule and your President at the last Nottingham meeting) took a prominent part in that discovery—Helmholtz and J. R. Mayer—were physiologists as much as they were physicists. I will not attempt even to enumerate the achievements of that epoch of progress. I may, however, without risk of wearying you, indicate the lines along which research at first proceeded, and draw your attention to the contrast between then and now. At present a young observer who is zealous to engage in research finds himself provided with the most elaborate means of investigation, the chief obstacle to his success being that the problems which have been left over by his predecessors are of extreme difficulty, all of the easier questions having been worked out. There were then also difficulties, but of an entirely different kind. The work to be done was in itself easier, but the means for doing it were wanting, and every investigator had to depend on his own resources. Consequently the successful men were those who, in addition to scientific training, possessed the ingenuity to devise and the skill to carry out methods for themselves. The work by which du Bois-Reymond laid the foundation of animal electricity would not have been possible had not its author, besides being a trained physicist, known how to do as good work in a small room in the upper floor of the old University building at Berlin as any which is now done in his splendid laboratory. Had Ludwig not possessed mechanical aptitude, in addition to scientific knowledge, he would have been unable to devise the apparatus by which he measured and recorded the variations of arterial pressure (1848), and verified the principles which Young had laid down thirty years before as to the mechanics of the circulation. Nor, lastly, could Helmholtz, had he not been a great deal more than a mere physiologist, have made those measurements of the time-relations of muscular and nervous responses to stimulation, which not only afford a solid foundation for all that has been done since in the same direction, but has served as models of physiological experiment, and as evidence that perfect work was possible and was done by capable men, even when there were no physiological laboratories.

Each of these examples relates to work done within a year or two of the middle of the century.[1] If it were possible to enter

[1] The first part of the *Physiological Anatomy* appeared in 1843. It was concluded in 1856.

[2] It is worthy of note that these five distinguished men were merely contemporaries: Ludwig graduated in 1839, Bernard in 1843, the other three between those dates. Three survive—Helmholtz, Ludwig, du Bois-Reymond.

[1] The "Untersuchungen über thierische Electricität" appeared in 1848; Ludwig's researches on the circulation, which included the first description of the "kymograph" and served as the foundation of the "graphic method" in 1847; Helmholtz's research on the propagation in motor nerves in 1851.

more fully on the scientific history of the time, we should, I think, find the clearest evidence, first, that the foundation was laid in anatomical discoveries, in which it is gratifying to remember that English anatomists (Allen Thomson, Bowman, Goodsir, Sharpey) took considerable share; secondly, that progress was rendered possible by the rapid advances which, during the previous decade, had been made in physics and chemistry, and the participation of physiology in the general awakening of the scientific spirit which these discoveries produced. I venture, however, to think that, notwithstanding the operation of these two causes, or rather combinations of causes, the development of our science would have been delayed had it not been for the exceptional endowments of the four or five young experimenters whose names I have mentioned, each of whom was capable of becoming a master in his own branch, and of guiding the future progress of inquiry.

Just as the affinities of an organism can be best learned from its development, so the scope of a science may be most easily judged of by the tendencies which it exhibits in its origin. I wish now to complete the sketch I have endeavoured to give of the way in which physiology entered on the career it has since followed for the last half-century, by a few words as to the influence exercised on general physiological theory by the progress of research. We have seen that no real advance was made until it became possible to investigate the phenomena of life by methods which approached more or less closely to those of the physicist, in exactitude. The methods of investigation being physical or chemical, the organism itself naturally came to be considered as a complex of such processes, and nothing more. And in particular the idea of adaptation, which, as I have endeavoured to show, is not a consequence of organism, but its essence, was in great measure lost sight of. Not, I think, because it was any more possible than before to conceive of the organism otherwise than as a working together of parts for the good of the whole, but rather that, if I may so express it, the minds of men were so occupied with new facts that they had not time to elaborate theories. The old meaning of the term "adaptation" as the equivalent of "design" had been abandoned, and no new meaning had yet been given to it, and consequently the word "mechanism" came to be employed as the equivalent of "process," as if the constant concomitance or sequence of two events was in itself a sufficient reason for assuming a mechanical relation between them. As in daily life so also in science, the misuse of words leads to misconceptions. To assert that the link between *a* and *b* is mechanical, for no better reason than that *b* always follows *a*, is an error of statement, which is apt to lead the incautious reader or hearer to imagine that the relation between *a* and *b* is understood, when in fact its nature may be wholly unknown. Whether or not at the time which we are considering, some physiological writers showed a tendency to commit this error, I do not think that it found expression in any generally accepted theory of life. It may, however, be admitted that the rapid progress of experimental investigation led to too confident anticipations, and that to some enthusiastic minds it appeared as if we were approaching within measurable distance of the end of knowledge. Such a tendency is, I think, a natural result of every signal advance. In an eloquent Harveian oration, delivered last autumn by Dr. Bridges, it was indicated how, after Harvey's great discovery of the circulation, men were too apt to found upon it explanations of all phenomena whether of health or disease, to such an extent that the practice of medicine was even prejudicially affected by it. In respect of its scientific importance the epoch we are considering may well be compared with that of Harvey, and may have been followed by an undue preference of the new as compared with the old, but no more permanent unfavourable results have shown themselves. As regards the science of medicine we need only remember that it was during the years between 1845 and 1860 that Virchow made those researches by which he brought the processes of disease into immediate relation with the normal processes of cell-development and growth, and so, by making pathology a part of physiology, secured its subsequent progress and its influence on practical medicine. Similarly in physiology, the achievements of those years led on without any interruption or drawback to those of the following generation; while in general biology, the revolution in the mode of regarding the internal processes of the animal or plant organism which resulted from these achievements, prepared the way for the acceptance of the still greater revolution which the Darwinian epoch brought about in the views entertained by naturalists of the relations of plants and animals to each other and to their surroundings.

It has been said that every science of observation begins by going out botanising, by which, I suppose, is meant that collecting and recording observations is the first thing to be done in entering on a new field of inquiry. The remark would scarcely be true of physiology, even at the earliest stage of its development, for the most elementary of its facts could scarcely be picked up as one gathers flowers in a wood. Each of the processes which go to make up the complex of life requires separate investigation, and in each case the investigation must consist in first splitting up the process into its constituent phenomena, and then determining their relation to each other, to the process of which they form part, and to the conditions under which they manifest themselves. It will, I think, be found that even in the simplest inquiry into the nature of vital processes some such order as this is followed. Thus, for example, if muscular contraction be the subject on which we seek information, it is obvious that, in order to measure its duration, the mechanical work it accomplishes, the heat wasted in doing it, the electro-motive forces which it develops, and the changes of form associated with these phenomena, special modes of observation must be used for each of them, that each measurement must be in the first instance separately made, under special conditions, and by methods specially adapted to the required purpose. In the synthetic part of the inquiry the guidance of experiment must again be sought for the purpose of discriminating between apparent and real causes, and of determining the order in which the phenomena occur. Even the simplest experimental investigations of vital processes are beset with difficulties. For, in addition to the extreme complexity of the phenomena to be examined and the uncertainties which arise from the relative inconstancy of the conditions of all that goes on in the living organism, there is this additional drawback, that, whereas in the exact sciences experiment is guided by well-ascertained laws, here the only principle of universal application is that of adaptation, and that even this cannot, like a law of physics, be taken as a basis for deductions, but only as a summary expression of that relation between external exciting causes and the reactions to which they give rise, which, in accordance with Treviranus' definition, is the essential character of vital activity.

The Specific Energies of the Organism.

When in 1826 J. Müller was engaged in investigating the physiology of vision and hearing he introduced into the discussion a term "specific energy," the use of which by Helmholtz[1] in his physiological writings has rendered it familiar to all students. Both writers mean by the word energy, not the "capacity of doing work," but simply *activity*, using it in its old-fashioned meaning, that of the Greek word from which it is derived. With the qualification "specific," it serves, perhaps, better than any other expression to indicate the way in which adaptation manifests itself. In this more extended sense the "specific energy" of a part or organ—whether that part be a secreting cell, a motor cell of the brain or spinal cord, or one of the photogenous cells which produce the light of the glowworm, or the protoplasmic plate which generates the discharge of the torpedo—is simply the special action which it *normally* performs, its norma or rule of action being in each instance the *interest of the organism* as a whole of which it forms part, and the exciting cause some influence outside of the excited structure, technically called a stimulus. It thus stands for a characteristic of living structures which seems to be universal. The apparent exceptions are to be found in those bodily activities which, following Bichat, we call vegetative, because they go on, so to speak, as a matter of course; but the more closely we look into them the more does it appear that they form no exception to the general rule, that every link in the chain of living action, however uniform that action may be, is a response to an antecedent influence. Nor can it well be doubted that, as every living cell or tissue is called upon to act in the interest of the whole, the organism must be capable of influencing every part so as to regulate its action. For, although there are some instances in which the channels of this influence are as yet unknown, the tendency of recent investigations has been to diminish the number of such instances. In general there is no difficulty in determining both the nature of the central influence exercised and the relation

1 "Handb. der physiologischen Optik," 1886, p. 233. Helmholtz uses the word in the plural—the "energies of the nerves of special sense."

between it and the normal function. It may help to illustrate this relation to refer to the expressive word *Auslösung* by which it has for many years been designated by German writers. This word stands for the performance of function by the "letting off" of "specific energies." Carrying out the notion of "letting off" as expressing the link between action and reaction, we might compare the whole process to the mode of working of a repeating clock (or other similar mechanism), in which case the pressure of the finger on the button would represent the external influence or stimulus, the striking of the clock, the normal reaction. And now may I ask you to consider in detail one or two illustrations of physiological reaction—of the *letting off of specific energy?*

The repeater may serve as a good example, inasmuch as it is, in biological language, a highly differentiated structure, to which a single function is assigned. So also in the living organism, we find the best examples of specific energy where Müller found them, namely, in the most differentiated, or, as we are apt to call them, the *highest* structures. The retina, with the part of the brain which belongs to it, together constitute such a structure, and will afford us therefore the illustration we want, with this advantage for our present purpose, that the phenomena are such as we all have it in our power to observe in ourselves. In the visual apparatus the principle of *normality* of reaction is fully exemplified. In the physical sense the word "light" stands for ether vibrations, but in the sensuous or subjective sense for sensations. The swings are the stimulus, the sensations are the reaction. Between the two comes the link, the "letting off," which it is our business to understand. Here let us remember that the man who first recognised this distinction between the physical and the physiological was not a biologist, but a physicist. It was Young who first made clear (though his doctrine fell on unappreciative ears) that, although in vision the external influences which give rise to the sensation of light are infinitely varied, the responses need not be more than three in number, each being, in Müller's language, a "specific energy" of some part of the visual apparatus. We speak of the organ of vision as *highly differentiated*, an expression which carries with it the suggestion of a distinction of rank between different vital processes. The suggestion is a true one; for it would be possible to arrange all those parts or organs of which the bodies of the *higher* animals consist in a series, placing at the lower end of the series those of which the functions are continuous, and therefore called vegetative; at the other, those highly specialised structures, as, *e.g.*, those in the brain, which in response to physical light produce physiological, that is subjective, light; or, to take another instance, the so-called motor cells of the surface of the brain, which in response to a stimulus of much greater complexity produce voluntary motion. And just as in civilised society an individual is valued according to his power of doing one thing well, so the high rank which is assigned to the structure, or rather to the "specific energy" which it represents, belongs to it by virtue of its specialisation. And if it be asked how this conformity is manifested, the answer is, by the quality, intensity, duration, and extension of the response, in all which respects vision serves as so good an example, that we can readily understand how it happened that it was in this field that the relation between response and stimulus was first clearly recognised. I need scarcely say that, however interesting it might be to follow out the lines of inquiry thus indicated, we cannot attempt it this evening. All that I can do is to mention one or two recent observations which, while they serve as illustrations, may perhaps be sufficiently novel to interest even those who are at home in the subject.

Probably every one is acquainted with some of the familiar proofs that an object is seen for a much longer period than it is actually exposed to view; that the visual reaction lasts much longer than its cause. More precise observations teach us that this response is regulated according to laws which it has in common with all the higher functions of an organism. If, for example, the cells in the brain of the torpedo are "let off"—that is, awakened by an external stimulus—the electrical discharge, which, as in the case of vision, follows after a certain interval, lasts a certain time, first rapidly increasing to a maximum of intensity, then more slowly diminishing. In like manner, as regards the visual apparatus, we have, in the response to a sudden invasion of the eye by light, a rise and fall of a similar character. In the case of the electrical organ, and in many analogous instances, it is easy to investigate the time relations of the successive phenomena, so as to represent them graphically. Again, it is found that in many physiological reactions, the period of rising "energy" (as Helmholtz called it) is followed by a period during which the responding structure is not only inactive, but its capacity for energising is so completely lost that the same exciting cause which a moment before "let off" the characteristic response is now without effect. As regards vision, it has long been believed that these general characteristics of physiological reaction have their counterpart in the visual process, the most striking evidence being that in the contemplation of a lightning flash—or better, of an instantaneously illuminated white disc[1]—the eye seems to receive a double stroke, indicating that, although the stimulus is single and instantaneous, the response is reduplicated. The most precise of the methods we until lately possessed for investigating the wax and wane of the visual reaction, were not only difficult to carry out but left a large margin of uncertainty. It was therefore particularly satisfactory when M. Charpentier, of Nancy, whose merits as an investigator are perhaps less known than they deserve to be, devised an experiment of extreme simplicity which enables us, not only to observe, but to measure with great facility both phases of the reaction. It is difficult to explain even the simplest apparatus without diagrams; you will, however, understand the experiment if you will imagine that you are contemplating a disc, like those ordinarily used for colour mixing; that it is divided by two radial lines which diverge from each other at an angle of 60°; that the sector which these lines enclose is white, the rest black; that the disc revolves slowly, about once in two seconds. You then see, close to the front edge of the advancing sector, a black bar, followed by a second at the same distance from itself but much fainter. Now the scientific value of the experiment consists in this, that the angular distance of the bar from the black border is in proportion to the frequency of the revolutions—the faster the wider. If, for example, when the disc makes half a revolution in a second the distance is ten degrees, this obviously means that when light bursts into the eye, the extinction happens one-eighteenth of a second after the excitation.[2]

The fact thus demonstrated, that the visual reaction consequent on an instantaneous illumination exhibits the alternations I have described, has enabled M. Charpentier to make out another fact in relation to the visual reaction which is, I think, of equal importance. In all the instances, excepting the retina, in which the physiological response to stimulus has a definite time-limitation, and in so far resembles an explosion—in other words, in all the higher forms of specific energy, it can be shown experimentally that the process is propagated from the part first directly acted on to other contiguous parts of similar endowment. Thus in the simplest of all known phenomena of this kind, the electrical change, by which the leaf of the Dionæa plant responds to the slightest touch of its sensitive hairs, is propagated from one side of the leaf to the other, so that in the opposite lobe the response occurs after a delay which is proportional to the distance between the spot excited and the spot observed. That in the retina there is also such propagation has not only been surmised from analogy, but inferred from certain observed facts. M. Charpentier has now been able by a method which, although simple, I must not attempt to describe, not only to prove its existence, but to measure its rate of progress over the visual field.

There is another aspect of the visual response to the stimulus of light which, if I am not trespassing too long on your patience, may, I think, be interesting to consider. As the relations between the sensations of colour and the physical properties of the light which excites them, are among the most certain and invariable in the whole range of vital reactions, it is obvious that they afford as fruitful a field for physiological investigation as those in which white light is concerned. We have on one side physical facts, that is, wave-lengths or vibration-rates; on the other, facts in consciousness—namely, sensations of colour—so simple that notwithstanding their subjective character there is no difficulty in measuring either their intensity or their duration. Between these there are *lines of influence*, neither physical nor psychological, which pass from the former to the latter through the visual apparatus (retina, nerve, brain).

[1] The phenomenon is best seen when, in a dark room, the light of a luminous spark is thrown on to a white screen with the aid of a suitable lens.

[2] Charpentier, "Réaction oscillatoire de la Rétine sous l'influence des excitations lumineuses," *Archives de Physiol.*, vol. xxiv. p. 541, and *Propagation de l'action oscillatoire*, &c., p. 362.

It is these lines of influence which interest the physiologist. The structure of the visual apparatus affords us no clues to trace them by. The most important fact we know about them is that they must be at least three in number.

It has been lately assumed by some that vision, like every other specific energy, having been developed progressively, objects were seen by the most elementary forms of eye only in chiaroscuro, that afterwards some colours were distinguished, eventually all. As regards hearing it is so. The organ which, on structural grounds, we consider to represent that of hearing in animals low in the scale of organisation—as, *e.g.*, in the Ctenophora—has nothing to do with sound,[1] but confers on its possessor the power of judging of the direction of its own movements in the water in which it swims, and of guiding these movements accordingly. In the lowest vertebrates, as, *e.g.*, in the dogfish, although the auditory apparatus is much more complicated in structure, and plainly corresponds with our own, we still find the particular part which is concerned in hearing scarcely traceable. All that is provided for is that sixth sense, which the higher animals also possess, and which enables them to judge of the direction of their own movements. But a stage higher in the vertebrate series we find the special mechanisms by which we ourselves appreciate sounds beginning to appear—not supplanting or taking the place of the imperfect organ, but added to it. As regards hearing, therefore, a new function is acquired without any transformation or fusion of the old into it. We ourselves possess the sixth sense, by which we keep our balance and which serves as the guide to our bodily movements. It resides in the part of the internal ear which is called the labyrinth. At the same time we enjoy along with it the possession of the cochlea, that more complicated apparatus by which we are able to hear sounds and to discriminate their vibration-rates.

As regards vision, evidence of this kind is wanting. There is, so far as I know, no proof that visual organs which are so imperfect as to be incapable of distinguishing the forms of objects, may not be affected differently by their colours. Even if it could be shown that the least perfect forms of eye possess only the power of discriminating between light and darkness, the question whether in our own such a faculty exists separately from that of distinguishing colours is one which can only be settled by experiment. As in all sensations of colour the sensation of brightness is mixed, it is obvious that one of the first points to be determined is whether the latter represents a "specific energy" or merely a certain combination of specific energies which are excited by colours. The question is not whether there is such a thing as white light, but whether we possess a separate faculty by which we judge of light and shade—a question which, although we have derived our knowledge of it chiefly from physical experiment, is one of eye and brain, not of wave-lengths or vibration-rates, and is therefore essentially physiological.

There is a German proverb which says, "Bei Nacht sind alle Katzen grau." The fact which this proverb expresses presents itself experimentally when a spectrum projected on a white surface is watched, while the intensity of the light is gradually diminished. As the colours fade away they become indistinguishable as such, the last seen being the primary red and green. Finally they also disappear, but a gray band of light still remains, of which the most luminous part is that which before was green.[2] Without entering into details, let us consider what this tells us of the specific energy of the visual apparatus. Whether or not the faculty by which we see gray in the dark is one which we possess in common with animals of imperfectly developed vision, there seems little doubt that there are individuals of our own species who, in the fullest sense of the expression, have no eye for colour; in whom all colour sense is absent; persons who inhabit a world of gray, seeing all things as they might have done had they and their ancestors always lived nocturnal lives. In the theory of colour vision, as it is commonly stated, no reference is made to such a faculty as we are now discussing.

Prof. Hering, whose observations as to the diminished spectrum I referred to just now, who was among the first to subject the vision of the *totally* colour-blind to accurate examination, is of opinion, on that and on other grounds, that the sensation of light and shade is a specific faculty. Very recently the same view has been advocated on a wide basis by a distinguished psychologist, Prof. Ebbinghaus.[1] Happily, as regards the actual experimental results relating to both these main subjects, there seems to be a complete coincidence of observation between observers who interpret them differently. Thus the recent elaborate investigations of Captain Abney[2] (with General Festing), representing graphically the results of his measurements of the subjective values of the different parts of the diminished spectrum, as well as those of the fully illuminated spectrum as seen by the totally colour-blind, are in the closest accord with the observations of Hering, and have, moreover, been substantially confirmed in both points by the measurements of Dr. König in Helmholtz' laboratory at Berlin.[3] That observers of such eminence as the three persons whom I have mentioned, employing different methods and with a different purpose in view, and without reference to each other's work, should arrive in so complicated an inquiry at coincident results, augurs well for the speedy settlement of this long-debated question. At present the inference seems to be that such a specific energy as Hering's theory of vision postulates actually exists, and that it has for associates the colour-perceiving activities of the visual apparatus, provided that these are present; but that whenever the intensity of the illumination is below the chromatic threshold—that is, too feeble to awaken these activities—or when, as in the totally colour-blind, they are wanting, it manifests itself independently; all of which can be most easily understood on such a hypothesis as has lately been suggested in an ingenious paper by Mrs. Ladd Franklin,[4] that each of the elements of the visual apparatus is made up of a central structure for the sensation of light and darkness, with collateral appendages for the sensations of colour—it being, of course, understood that this is a mere diagrammatic representation, which serves no purposes beyond that of facilitating the conception of the relation between the several "specific energies."

Experimental Psychology.

Resisting the temptation to pursue this subject further, I will now ask you to follow me into a region which, although closely connected with the subjects we have been considering, is beset with greater difficulties—the subject in which, under the name of Physiological or Experimental Psychology, physiologists and psychologists have of late years taken a common interest—a borderland not between fact and fancy, but between two methods of investigation of questions which are closely related, which here, though they do not overlap, at least interdigitate. It is manifest that, quite irrespectively of any foregone conclusion as to the dependence of mind on processes of which the biologist is accustomed to take cognisance, mind must be regarded as one of the "specific energies" of the organism, and should on that ground be included in the subject-matter of physiology. As, however, our science, like other sciences, is limited not merely by its subject but also by its method, it actually takes in only so much of psychology as is experimental. Thus sensation, although it is psychological, and the investigation of its relation to the special structures by which the mind keeps itself informed of what goes on in the outside world, have always been considered to be in the physiological sphere. And it is by anatomical researches relating to the minute structure and to the development of the brain, by observation of the facts of disease, and, above all, by physiological experiment, that those changes in the ganglion cells of the brain and spinal cord which are the immediate antecedents of every kind of bodily action have been traced. Between the two—that is, between sensation and the beginning of action—there is an intervening region which the physiologist has hitherto willingly resigned to psychology, feeling his incompetence to use the only instrument by which it can be explored—that of introspection. This consideration enables us to understand the course which the new study (I will not claim for it the title of a new science, regard-

[1] Verworn, "Gleichgewicht u. Otolithenorgan," *Pflüger's Archiv.*, vol. l., p. 423; also Ewald's Researches on the Labyrinth as a Sense-organ ("Ueber das Endorgan des Nervus octavus," Wiesbaden. 1892).

[2] Hering, "Untersuch. eines total Farbenblinden," *Pflüger's Arch.*, vol. xlix., 1891, p. 563.

[1] Ebbinghaus, "Theorie des Farbensehens," *Zeitschr. f. Psychol.*, vol. v., 1893, p. 145.

[2] Abney and Festing, Colour Photometry, Part III. *Phil. Trans.*, vol. clxxxiii., A, 1891, p. 531.

[3] König, "Ueber den Helligkeitwerth der Spectralfarben bei verschiedener absoluter Intensität," *Beiträge zur Psychologie*, &c., "Festschrift zu H. von Helmholtz, 70, Geburtstage," 1891, p. 309.

[4] Christine Ladd Franklin, "Eine neue Theorie der Lichtempfindungen," *Zeitschr. für Psychologie*, vol iv., 1893, p. 211; see also the Proceedings of the last Psychological Congress in London, 1892.

ing it as merely a part of the great science of life) has hitherto followed, and why physiologists have been unwilling to enter on it. The study of the less complicated internal relations of the organism has afforded so many difficult problems that the most difficult of all have been deferred; so that although the psycho-physical method was initiated by E. H. Weber in the middle of the present century, by investigations[1] which formed part of the work done at that epoch of discovery, and although Prof. Wundt, also a physiologist, has taken a larger share in the more recent development of the new study, it is chiefly by psychologists that the researches which have given to it its importance as a new discipline have been conducted.

Although, therefore, experimental psychology has derived its methods from physical science, the result has been not so much that physiologists have become philosophers, as that philosophers have become experimental psychologists. In our own universities, in those of America, and still more in those of Germany, psychological students of mature age are to be found who are willing to place themselves in the dissecting-room side by side with beginners in anatomy, in order to acquire that exact knowledge of the framework of the organism without which no man can understand its working. Those, therefore, who are apprehensive lest the regions of mind should be invaded by the *insaniens sapientia* of the laboratory, may, I think, console themselves with the thought that the invaders are for the most part men who before they became laboratory workers had already given their allegiance to philosophy; their purpose being not to relinquish definitively, but merely to lay aside for a time, the weapons in the use of which they had been trained, in order to learn the use of ours. The motive that has encouraged them has not been any hope of finding an experimental solution of any of the ultimate problems of philosophy, but the conviction that, inasmuch as the relation between mental stimuli and the mental processes which they awaken is of the same order with the relation between every other vital process and its specific determinant, the only hope of ascertaining its nature must lie in the employment of the same methods of comparative measurement which the biologist uses for similar purposes. Not that there is necessarily anything scientific in mere measurement, but that measurement affords the only means by which it can be determined whether or not the same conformity in the relation between stimulus and reaction which we have accepted as the fundamental characteristic of life, is also to be found in mind, notwithstanding that mental processes have no known physical concomitants. The results of experimental psychology tend to show that it is so, and consequently that in so far the processes in question are as truly functions of organism as the contraction of a muscle, or as the changes produced in the retinal pigment by light.

I will make no attempt even to enumerate the special lines of inquiry which during the last decade have been conducted with such vigour in all parts of the world, all of them traceable to the influence of the Leipzig school; but will content myself with saying that the general purpose of these investigations has been to determine with the utmost attainable precision the nature of psychical relations. Some of these investigations begin with those simpler reactions which more or less resemble those of an automatic mechanism, proceeding to those in which the resulting action or movement is modified by the influence of auxiliary or antagonistic conditions, or changed by the simultaneous or antecedent action on the reagent of other stimuli, in all of which cases the effect can be expressed quantitatively; others lead to results which do not so readily admit of measurement. In pursuing this course of inquiry the physiologist finds himself as he proceeds more and more the *coadjutor* of the psychologist, less and less his *director*; for whatever advantage the former may have in the mere *technique* of observation, the things with which he has to do are revealed only to introspection, and can be studied only by methods which lie outside of his sphere. I might in illustration of this refer to many recent experimental researches—such, for example, as those by which it has been sought to obtain exact data as to the physiological concomitants of pleasure and of pain, or as to the influence of weariness and recuperation, as modifiers of psychological reactions. Another outwork of the mental citadel which has been invaded by the experimental method is that of memory. Even here it can be shown that in the comparison of transitory as compared with permanent memory—as, for example, in the getting off by heart of a wholly uninteresting series of words, with subsequent oblivion and reacquisition—the labour of acquiring and reacquiring may be measured, and consequently the relation between them; and that this ratio varies according to a simple numerical law.

[1] Weber's researches were published in Wagner's *Handwörterbuch*, I think, in 1849.

I think it not unlikely that the only effect of what I have said may be to suggest to some of my hearers the question, What is the use of such inquiries? Experimental psychology has, to the best of my knowledge, no technical application. The only satisfactory answer I can give is that it has exercised, and will exercise in future, a helpful influence on the science of life. Every science of observation, and each branch of it, derives from the peculiarities of its methods certain tendencies which are apt to predominate unduly. We speak of this as specialisation, and are constantly striving to resist its influence. The most successful way of doing so is by availing ourselves of the counteracting influence which two opposite tendencies mutually exercise when they are simultaneous. He that is skilled in the methods of introspection naturally (if I may be permitted to say so) looks at the same thing from an opposite point of view to that of the experimentalist. It is, therefore, good that the two should so work together that the tendency of the experimentalist to imagine the existence of mechanism where none is proved to exist—of the psychologist to approach the phenomena of mind too exclusively from the subjective side—may mutually correct and assist each other.

Phototaxis and Chemiotaxis.

Considering that every organism must have sprung from a unicellular ancestor, some have thought that unless we are prepared to admit a deferred epigenesis of mind, we must look for psychical manifestations even among the lowest animals, and that as in the protozoon all the vital activities are blended together, mind should be present among them not merely potentially but actually, though in diminished degree.

Such a hypothesis involves ultimate questions which it is unnecessary to enter upon: it will, however, be of interest in connection with our present subject to discuss the phenomena which served as a basis for it—those which relate to what may be termed the behaviour of unicellular organisms and of individual cells, in so far as these last are capable of reacting to external influences. The observations which afford us most information are those in which the stimuli employed can be easily measured, such as electrical currents, light, or chemical agents in solution.

A single instance, or at most two, must suffice to illustrate the influence of light in directing the movements of freely moving cells, or, as it is termed, phototaxis. The rod-like purple organism called by Engelmann *Bacterium photometricum*,[1] is such a light-lover that if you place a drop of water containing these organisms under the microscope, and focus the smallest possible beam of light on a particular spot in the field, the spot acts as a light trap and becomes so crowded with the little rodlets as to acquire a deep port-wine colour. If instead of making his trap of white light, he projected on the field a microscopic spectrum, Engelmann found that the rodlets showed their preference for a spectral colour, which is absorbed when transmitted through their bodies. By the aid of a light trap of the same kind, the very well-known spindle-shaped and flagellate cell of Euglena can be shown to have a similar power of discriminating colour, but its preference is different. This familiar organism advances with its flagellum forwards, the sharp end of the spindle having a red or orange eye point. Accordingly, the light it loves is again that which is most absorbed—viz., the blue of the spectrum (line F).

These examples may serve as an introduction to a similar one in which the directing cause of movement is not physical but chemical. The spectral light trap is used in the way already described; the organisms to be observed are not coloured, but bacteria of that common sort which twenty years ago we used to call *Bacterium termo*, and which is recognised as the ordinary determining cause of putrefaction. These organisms do not care for life, but are great oxygen-lovers. Consequently, if you illuminate with your spectrum a filament of a confervoid alga, placed in water containing bacteria, the assimilation of carbon and consequent disengagement of oxygen is most active in the part of the filament which receives the red rays (B to C). To

[1] Engelmann, "Bacterium photometricum," *Onderzoek. Physiol Lab. Utrecht*, vol. vii. p. 200; also Ueber Licht-u. Farbenperception niederster Organismen, *Pflüger's Arch*, vol. xxix. p. 387.

this part, therefore, where there is a dark band of absorption, the bacteria which want oxygen are attracted in crowds. The motive which brings them together is their desire for oxygen. Let us compare other instances in which the source of attraction is food.

The plasmodia of the myxomycetes, particularly one which has been recently investigated by Mr. Arthur Lister,[1] may be taken as a typical instance of what may be called the chemical allurement of living protoplasm. In this organism, which in the active state is an expansion of labile living material, the delicacy of the reaction is comparable to that of the sense of smell in those animals in which the olfactory organs are adapted to an aquatic life. Just as, for example, the dogfish is attracted by food which it cannot see, so the plasmodium of Badhamia becomes aware, as if it smelled it, of the presence of its food—a particular kind of fungus. I have no diagram to explain this, but will ask you to imagine an expansion of living material, quite structureless, spreading itself along a wet surface; that this expansion of transparent material is bounded by an irregular coast-line; and that somewhere near the coast there has been placed a fragment of the material on which the Badhamia feeds. The presence of this bit of *Stereum* produces an excitement at the part of the plasmodium next to it. Towards this centre of activity streams of living material converge. Soon the afflux leads to an outgrowth of the plasmodium, which in a few minutes advances towards the desired fragment, envelopes, and incorporates it.

May I give you another example also derived from the physiology of plants? Very shortly after the publication of Engelmann's observations of the attraction of bacteria by oxygen, Pfeffer made the remarkable discovery that the movements of the antherozoids of ferns and of mosses are guided by impressions derived from chemical sources, by the allurement exercised upon them by certain chemical substances in solution—in one of the instances mentioned by sugar, in the other by an organic acid. The method consisted in introducing the substance to be tested, in any required strength, into a minute capillary tube closed at one end, and placing it under the microscope in water inhabited by antherozoids, which thereupon showed their predilection for the substance, or the contrary, by its effect on their movements. In accordance with the principle followed in experimental psychology, Pfeffer[2] made it his object to determine, not the relative effects of different doses, but the smallest perceptible increase of dose which the organism was able to detect, with this result—that, just as in measurements of the relation between stimulus and reaction in ourselves we find that the sensational value of a stimulus depends, not on its absolute intensity, but on the ratio between that intensity and the previous excitation, so in this simplest of vital reagents the same so-called psycho-physical law manifests itself. It is not, however, with a view to this interesting relation that I have referred to Pfeffer's discovery, but because it serves as a centre around which other phenomena, observed alike in plants and animals, have been grouped. As a general designation of reactions of this kind Pfeffer devised the term Chemotaxis, or, as we in England prefer to call it, Chemiotaxis. Pfeffer's contrivance for chemiotactic testing was borrowed from the pathologists, who have long used it for the purpose of determining the relation between a great variety of chemical compounds or products, and the colourless corpuscles of the blood. I need, I am sure, make no apology for referring to a question which, although purely pathological, is of very great biological interest—the theory of the process by which, not only in man, but also, as Metschnikoff has strikingly shown, in animals far down in the scale of development, the organism protects itself against such harmful things as, whether particulate or not, are able to penetrate its framework. Since Cohnheim's great discovery in 1867 we have known that the central phenomenon of what is termed by pathologists *inflammation* is what would now be called a chemiotactic one; for it consists in the gathering together, like that of vultures to a carcase, of those migratory cells which have their home in the blood stream and in the lymphatic system, to any point where the living tissue of the body has been injured or damaged, as if the products of disintegration which are set free where such damage occurs were attractive to them.

The fact of chemiotaxis, therefore, as a constituent phenomenon of the process of inflammation, was familiar in pathology long before it was understood. Cohnheim himself attributed it to changes in the channels along which the cells moved, and this explanation was generally accepted, though some writers, at all events, recognised its incompleteness. But no sooner was Pfeffer's discovery known than Leber,[1] who for years had been working at the subject from the pathological side, at once saw that the two processes were of similar nature. Then followed a variety of researches of great interest, by which the importance of chemiotaxis in relation to the destruction of disease-producing microphytes was proved, by that of Buchner[2] on the chemical excitability of leucocytes being among the most important. Much discussion has taken place, as many present are aware, as to the kind of wandering cells, or leucocytes, which in the first instance attack morbific microbes, and how they deal with them. The question is not by any means decided. It has, however, I venture to think, been conclusively shown that the process of destruction is a chemical one, that the destructive agent has its source in the chemiotactic cells—that is, cells which act under the orders of chemical stimuli. Two Cambridge observers, Messrs. Kanthack and Hardy,[3] have lately shown that, in the particular instance which they have investigated, the cells which are most directly concerned in the destruction of morbific *bacilli*, although chemiotactic, do not possess the power of incorporating bacilli or particles of any other kind. While, therefore, we must regard the relation between the process of devitalising and that of incorporating as not yet sufficiently determined, it is now no longer possible to regard the latter as essential to the former.

There seems, therefore, to be very little doubt that chemiotactic cells are among the agents by which the human or animal organism protects itself against infection. There are, however, many questions connected with this action which have not yet been answered. The first of these are chemical ones—that of the nature of the attractive substance and that of the process by which the living carriers of infection are destroyed. Another point to be determined is how far the process admits of adaptation to the particular infection which is present in each case, and to the state of liability or immunity of the infected individual. The subject is therefore of great complication. None of the points I have suggested can be settled by experiments in glass tubes such as I have described to you. These serve only as indications of the course to be followed in much more complicated and difficult investigations—when we have to do with acute diseases as they actually affect ourselves or animals of similar liability to ourselves, and find ourselves face to face with the question of their causes.

It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health England was at one time far ahead of all other countries, and still retains its superiority; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific inquiries, have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris, to obtain what England ought long ago to have provided. For to us, from the spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. At the beginning of this address I urged the claims of pure science. If I could, I should feel inclined to speak even more strongly of the application of science to the discovery of the causes of acute diseases. May I express the hope that the effort which is now being made to establish in England an institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present? And now may I ask your attention for a few moments more to the subject that more immediately concerns us?

Conclusion.

The purpose which I have had in view has been to show that there is one principle—that of adaptation—which separates

[1] Lister, "On the Plasmodium of *Badhamia utricularis*, &c." *Annals of Botany*, No. 5, June, 1888.

[2] Pfeffer, *Untersuch. a. d. botan. Institute z. Tübingen*, vol. i., part 3, 1884.

[1] Leber, "Die Anhäufung der Leucocyten am Orte des Entzündungsreizes," &c., *Die Entstehung der Entzündung*, &c., pp. 423-464. Leipzig, 1891.

[2] Buchner, "Die chem. Reizbarkeit der Leucocyten," &c., *Berliner klin Woch.*, 1890, No. 17.

[3] Kanthack and Hardy, "On the Characters and Behaviour of the Wandering Cells of the Frog," Proceedings of the Royal Society, vol. lii., p. 267.

biology from the exact sciences, and that in the vast field of biological inquiry the end we have is not merely, as in natural philosophy, to investigate the relation between the phenomenon and the antecedent and concomitant conditions on which it depends, but to possess this knowledge in constant reference to the interest of the organism. It may perhaps be thought that this way of putting it is too teleological, and that in taking, as it were, as my text this evening so old-fashioned a biologist as Treviranus, I am yielding to a retrogressive tendency. It is not so. What I have desired to insist on is that *organism* is a fact which encounters the biologist at every step in his investigations; that in referring it to any general biological principle, such as adaptation, we are only referring it to itself, not explaining it; that no explanation will be attainable until the conditions of its coming into existence can be subjected to experimental investigation so as to correlate them with those of processes in the non-living world.

Those who were present at the meeting of the British Association at Liverpool, will remember that then, as well as at some subsequent meetings, the question whether the conditions necessary for such an inquiry could be realised was a burning one. This is no longer the case. The patient endeavours which were made about that time to obtain experimental proof of what was called *abiogenesis*, although they conduced materially to that better knowledge which we now possess of the conditions of life of bacteria, failed in the accomplishment of their purpose. The question still remains undetermined; it has, so to speak, been adjourned *sine die*. The only approach to it lies at present in the investigation of those rare instances in which, although the relations between a living organism and its environment ceases as a watch stops when it has not been wound, these relations can be re-established—the process of life reawakened—by the application of the required stimulus.

I was also desirous to illustrate the relation between physiology and its two neighbours on either side, natural philosophy (including chemistry) and psychology. As regards the latter, I need add nothing to what has already been said. As regards the former, it may be well to notice that although physiology can never become a mere branch of applied physics or chemistry, there are parts of physiology wherein the principles of these sciences may be applied directly. Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation; and a century before, Borelli successfully examined the mechanisms of locomotion and the action of muscles, without reference to any, excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question—to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Prof. Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the "specific energy" of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—*i.e.* differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular field by Mr. Langley, who has made the subject of "specific energy" of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess.

There is, therefore, no doubt as to the advantages which physiology derives from the exact sciences. It could scarcely be averred that they would benefit in anything like the same degree from closer association with the science of life. Nevertheless, there are some points in respect of which that science may have usefully contributed to the advancement of physics or of chemistry. The discovery of Graham as to the characters of colloid substances, and as to the diffusion of bodies in solution through membranes, would never have been made had not Graham "ploughed," so to speak, "with our heifer." The relations of certain colouring matters to oxygen and carbon dioxide would have been unknown, had no experiments been made on the respiration of animals and the assimilative process in plants; and, similarly, the vast amount of knowledge which relates to the chemical action of ferments must be claimed as of physiological origin. So also there are methods, both physical and chemical, which were originally devised for physiological purposes. Thus the method by which meteorological phenomena are continuously recorded graphically, originated from that used by Ludwig (1847) in his "Researches on the Circulation"; the mercurial pump, invented by Lothar Meyer, was perfected in the physiological laboratories of Bonn and Leipzig; the rendering the galvanometer needle aperiodic by damping was first realised by du Bois-Reymond—in all of which cases invention was prompted by the requirements of physiological research.

Let me conclude with one more instance of a different kind, which may serve to show how, perhaps, the wonderful ingenuity of contrivance which is displayed in certain organised structures—the eye, the ear, or the organ of voice—may be of no less interest to the physicist than to the physiologist. Johannes Müller, as is well known, explained the compound eye of insects on the theory that an erect picture is formed on the convex retina by the combination of pencils of light, received from different parts of the visual field through the eyelets (ommatidia) directed to them. Years afterwards it was shown that in each eyelet an image is formed which is reversed. Consequently, the mosaic theory of Müller was for a long period discredited on the ground that an erect picture could not be made up of "upside-down" images. Lately the subject has been reinvestigated, with the result that the mosaic theory has regained its authority. Prof. Exner[1] has proved photographically that behind each part of the insect's eye an erect picture is formed of the objects towards which it is directed. There is, therefore, no longer any difficulty in understanding how the whole field of vision is mapped out as consistently as it is imaged on our own retina, with the difference, of course, that the picture is erect. But behind this fact lies a physical question—that of the relation between the erect picture which is photographed and the optical structure of the crystal cones which produce it—a question which, although we cannot now enter upon it, is quite as interesting as the physiological one.

With this history of a theory which, after having been for thirty years disbelieved, has been reinstated by the fortunate combination of methods derived from the two sciences, I will conclude. It may serve to show how, though physiology can never become a part of natural philosophy, the questions we have to deal with are cognate. Without forgetting that every phenomenon has to be regarded with reference to its useful purpose in the organism, the aim of the physiologist is not to inquire into final causes, but to investigate processes. His question is ever How, rather than Why.

May I illustrate this by a simple, perhaps too trivial, story, which derives its interest from its having been told of the childhood of one of the greatest natural philosophers of the present century?[2] He was even then possessed by that insatiable curiosity which is the first quality of the investigator; and it is related of him that his habitual question was "What is the *go* of it?" and if the answer was unsatisfactory, "What is the particular go of it?" That North Country boy became Prof. Clerk Maxwell. The questions he asked are those which in our various ways we are all trying to answer.

[1] Exner, "Die Physiologie der facettirten Augen von Krebsen u. Insecten," Leipzig, 1891.
[2] "Life of Clerk Maxwell" (Campbell and Garnett), p. 28.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY R. T. GLAZEBROOK, M.A., F.R.S., PRESIDENT OF THE SECTION.

BEFORE dealing with the subject which I hope to bring to your notice this morning, I wish to express my deep regret for the circumstances which have prevented Prof. Clifton, who had accepted the nomination of the Council, from being your President this year.

It was specially fitting that he who has done so much for this college, and particularly for this laboratory in which we meet, should take the chair at Nottingham. The occasions on which we see him are all too seldom; and we who come frequently to these meetings were looking forward to help and encouragement in our work, derived from his wide experience. You would desire, I feel sure, that I should convey to him the expressions of your sympathy. For myself I must ask that you will pass a lenient judgment on my efforts to fill his place.

Let me commence, then, with a brief retrospect of the past year and the events which concern our Section.

From the days of Galileo the four satellites of Jupiter have been objects of interest to the astronomer. Their existence was one of the earliest of the discoveries of the telescope; they proved conclusively that all the bodies of the solar system did not move round the earth. The year which has passed since our last meeting is memorable for the discovery of a fifth satellite. It is a year to day (September 13-14, 1892) since Prof. Barnard convinced himself that he had seen with the great telescope of the Lick Observatory this new member of our system as a star of the thirteenth magnitude, revolving round the planet in 11 hours 57 minutes 23 seconds.[1]

The conference on electrical standards, held at our meeting last year, has had important results. The resolutions adopted at Edinburgh were communicated to the Standards Committee of the Board of Trade. A supplementary report accepting these resolutions was agreed to by that Committee (November 29, 1892), and presented to the President of the Board of Trade. The definitions contained in this report will be made the basis of legislation throughout the world. They have been accepted by France, Germany, Austria, and Italy. The congress at Chicago, which has just been held, has ratified them, and thus we may claim that your Committee, co-operating with the leaders of physical science in other lands, has secured international agreement on these fundamental points.

Among the physical papers of the year I would mention a few as specially calling for notice. Mr. E. H. Griffiths's re-determination of the value of the mechanical equivalent of heat has just been published (*Phil. Trans.* vol. clxxxiv.), and is a monumental work. With untiring energy and great ability he struggled for five years against the difficulties of his task, and has produced results which, with the exception of one group of experiments, do not differ by more than 1 part in 10,000; while the results of that one excepted group differ from the mean only by 1 part in 4000.

The number of ergs of work required to raise one gramme of water 1° C. at 15° C. is $4{\cdot}198 \times 10^7$. Expressed in foot-pounds and Fahrenheit degrees, the value of J is 779·97. The value obtained by Joule from his experiments on the friction of water, when corrected in 1880 by Rowland so as to reduce his readings to the air thermometer, is 778·5 at $12^{\circ}{\cdot}7$ C. The result at this temperature of Rowland's own valuable research is 780·1. Another satisfactory outcome of Mr. Griffiths's work is the very exact accordance between the scale of temperature as determined by the comparison of his platinum thermometer with the air thermometer, which was made by Callendar and himself in 1890, and that of the nitrogen thermometer of the Bureau International at Sèvres.

Another great work now happily complete is Rowland's "Table of Standard Wave-lengths" (*Phil. Mag.*, July, 1893). Nearly a thousand lines have been measured with the skill and accuracy for which Rowland has made himself famous; and in this table we see the results achieved by the genius which designed the concave grating and the mechanical ingenuity which contrived the almost perfect screw.

Those of us who have seen Mr. Higgs's wonderful photographs of the solar spectrum, taken with a Rowland grating, will rejoice to know that his map also is now finished.

Lord Rayleigh's paper on "The Intensity of Light reflected from Water and Mercury at nearly perpendicular incidence," (*Phil. Mag.*, October, 1892), combined with the experiments on reflexion from liquid surfaces in the neighbourhood of the polarising angle (*Phil. Mag.*, January, 1892), establishes results of the utmost importance to optical theory. "There is thus," Lord Rayleigh concludes, "no experimental evidence against the rigorous application of Fresnel's formulæ"—for the reflexion of polarised light—"to the ideal case of an abrupt transition between two uniform transparent media."

Prof. Dewar has, during the year, continued his experiments on the liquefaction of oxygen and nitrogen on a large scale. To a physicist perhaps the most important results of the research are the discovery of the magnetic properties of liquid oxygen, and the proof of the fact that the resistance of certain pure metals vanishes at absolute zero (*Phil. Mag.*, October, 1892). The last discovery is borne out by Griffiths and Callendar's experiments with their platinum thermometers (*Phil. Mag.*, December, 1892).

Mr. Williams's article on "The Relation of the Dimensions of Physical Quantities to Directions in Space" (*Phil. Mag.*, September, 1892) has led to an interesting discussion. Some of his deductions will be noticed later.

The title-page of the first edition of Maxwell's "Electricity and Magnetism" bears the date 1873. This year, 1893, we welcome a third edition, edited by Maxwell's distinguished successor, and enriched by a supplementary volume, in which Prof. J. J. Thomson describes some of the advances made by electrical science in the last twenty years. The subject matter of this volume might well serve as a text for a Presidential Address.

The choice of a subject on which to speak to-day has been no easy task. The field of physics and mathematics is a wide one. There is one matter, however, to which for a few minutes I should like to call your attention, inadequately though it be. Optical theories have, since the year 1876, when I first read Sir George Stokes's "Report on Double Refraction" (British Association Report, 1862), had a special interest for me, and I think the time has come when we may with advantage review our position with regard to them, and sum up our knowledge.[1]

That light is propagated by an undulatory motion through a medium which we call the ether is now an established fact, although we know but little of the nature or constitution of the ether. The history of this undulatory theory is full of interest, and has, it appears to me, in its earlier stages been not quite clearly apprehended. Two theories have been proposed to account for optical phenomena. Descartes was the author of the one, the emission theory. Hooke, though his work was very incomplete, was the founder of the undulatory theory. In his "Micrographia," 1664, page 56, he asserts that light is a quick and short vibratory motion, "propagated every way through an homogeneous medium by direct or straight lines extended every way like rays from the centre of a sphere. . . . Every pulse or vibration of the luminous body will generate a sphere which will continually increase and grow bigger, just after the same manner, though indefinitely swifter, as the waves or rings on the surface do swell into bigger and bigger circles about a point on it"; and he gives on this hypothesis an account of reflexion, refraction, dispersion, and the colours of thin plates. In the same work, page 58, he describes an experiment practically identical with Newton's famous prism experiment, published in 1672. Hooke used for a prism a glass vessel about two feet long, filled with water, and inclined so that the sun's rays might enter obliquely at the upper surface and traverse the water. "The top surface is covered by an opacous body, all but a hole through which the sun's beams are suffered to pass into the water, and are thereby refracted" to the bottom of the glass, "against which part if a paper be expanded on the outside there will appear all the colours of the rainbow—that is, there will be generated the two principal colours, scarlet and blue, with all the intermediate ones which arise from the composition and diluting of these two." But Hooke could make no use of his own observation; he attempted to substantiate from it his own theory of colours, and

[1] "In general," he says, "the satellite has been faint. . . . On the 13th, however, when the air was very clear, it was quite easy."—NATURE, October 20, 1892.

[1] This address was in the printer's hands when I saw Sir George Stokes's paper on "The Luminiferous Ether," NATURE, July 27. Had I known that so great a master of my subject had dealt with it so lately, my choice might have been different; under the circumstances it was too late to change.

wrote pure nonsense in the attempt; and though his writings contain the germ of the theory, and in the light of our present knowledge it seems possible that he understood it more thoroughly than his contemporaries believed, yet his reasoning is so utterly vague and unsatisfactory that there is little ground for surprise that he convinced but few of its truth.

And then came Newton. It is claimed for him, and that with justice, that he was the true founder of the emission theory. In Descartes' hands it was a vague hypothesis. Newton deduced from it by rigid reasoning the laws of reflexion and refraction; he applied it with wondrous ingenuity to explain the colours of thin and of thick plates and the phenomena of diffraction, though in doing this he had to suppose a mechanism which he must have felt to be almost impossible; a mechanism which in time, as it was applied to explain other and more complex phenomena, became so elaborate that, in the words of Verdet, referring to a period one hundred years later, "all that is necessary to overturn this laborious scaffolding is to look at it and try to understand it."

But though Newton may with justice be called the founder of the emission theory, it is unjust to his memory to state that he accepted it as giving a full and satisfactory account of optics as they were known to him. When he first began his optical work he realised that facts and measurements were needed, and his object was to furnish the facts. He may have known of Hooke's theories. The copy of the "Micrographia" now at Trinity College was in the Library while Newton was working with his prism in rooms in college, and may have been consulted by him. An early note-book of his contains quotations from it. Still there was nothing in the theories but hypotheses unsupported by facts, and these would have no charm for Newton. The hypotheses in the main are right. Light is due to wave motion in an all-pervading ether; the principle of interference, vaguely foreshadowed by Hooke ("Micrographia," p. 66), was one which a century later was to remove the one difficulty which Newton felt. For there was one fact which Hooke's theory could not then explain, and till that explanation was given the theory must be rejected; the test was crucial, the answer was decisive.

Newton tells us repeatedly what the difficulty was. In reply to a criticism of Hooke's in 1672, he writes: "For to me the fundamental supposition itself seems impossible, namely, that the waves of vibrations of any fluid can, like the rays of light, be propagated in straight lines without continual and very extravagant spreading and bending into the quiescent medium where they are terminated by it. I mistake if there be not both experiment and demonstration to the contrary. . . . For it seems impossible that any of those motions or pressions can be propagated in straight lines without the like spreading every way into the shadowed medium."

Nor was there anything in the controversy with Hooke, which took place about 1675, to shake this belief. Hooke had read his paper describing his discovery of diffraction. He had announced it two years earlier, and there is no doubt in my mind that this was an original discovery, and not, as Newton seemed to imply soon after, taken from Grimaldi; but his paper does not remove the difficulty. Accordingly we find in the "Principia" Newton's attempted proof (lib. ii. prop. 42) that "motus omnis per fluidum propagatus divergit a recto tramite in spatia immota"—a demonstration which has convinced but few and leaves the question unsolved as before.

Again, in 1690 Huygens published his great "Traité de la Lumière," written in 1678. Huygens had clearer views than Hooke on all he wrote; many of his demonstrations may be given now as completely satisfactory, but on the one crucial matter he was fatally weak. He, rather than Hooke, is the true founder of the undulatory theory, for he showed what it would do if it could but explain the rectilinear propagation. The reasoning of the latter part of Huygens's first chapter becomes forcible enough when viewed in the light of the principle of interference enunciated by Young, November 12, 1801, and developed, independently of Young, by Fresnel in his great memoir on "Diffraction" in 1815; but without this aid it was not possible for Huygens's arguments to convince Newton, and hence in the "Opticks" (2nd edit., 1717) he wrote the celebrated Query 28: "Are not all hypotheses erroneous in which light is supposed to consist in pressure or motion propagated through a fluid medium? If it consisted in motion propagated either in an instant or in time it would bend into the shadow. For pressure or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies outside the shadow." These were his last words on the subject. They prove that he could not accept the undulatory theory; they do not prove that he believed the emission theory to give the true explanation. Yet, in spite of this, I think that Newton had a clearer view of the undulatory theory than his contemporaries, and saw more fully than they did what that theory could achieve if but the one difficulty were removed.

This was Young's belief, who writes (*Phil. Trans.*, November 12, 1801):—"A more extensive examination of Newton's various writings has shown me that he was in reality the first who suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally believed; and that a variety of arguments have been advanced as if to meet him which may be found in a nearly similar form in his own works." I wish to call attention to this statement, and to bring into more prominent view the grounds on which it rests, to place Newton in his true position as one of the founders of the undulatory theory.

The emission theory in Newton's hands was a dynamical theory; he traced the motion of material particles under certain forces, and found their path to coincide with that of a ray of light; and in the "Principia," prop. xcvi., Scholium, he calls attention to the similarity between these particles and light. The particles obey the laws of reflexion and refraction; but to explain why some of the incident light was reflected and some refracted Newton had to invent his hypothesis of fits of easy reflexion and transmission. These are explained in the "Opticks," book iii., props. xi., xii., and xiii. (1704), thus:—

"Light is propagated from luminous bodies in time, and spends about seven or eight minutes of an hour in passing from the sun to the earth.

"Every ray of light in its passage through any refracting surface is put into a certain transient constitution or state, which in the progress of the ray returns at equal intervals, and disposes the ray at each return to be easily transmitted through the next refracting surface, and between the returns to be easily reflected by it.

"*Definition.*—The return of the disposition of any ray to be reflected I will call its fit of easy reflexion, and those of the disposition to be transmitted its fits of easy transmission, and the space it passes between every return and the next return the interval of its fits.

"The reason why the surfaces of all thick transparent bodies reflect part of the light incident on them and refract the rest is that some rays at their incidence are in their fits of easy reflexion, some in their fits of easy transmission."

Such was Newton's theory. It accounts for some or all of the observed facts; but what causes the fits? Newton, in the "Opticks," states that he does not inquire; he suggests, for those who wish to deal in hypotheses, that the rays of light striking the bodies set up waves in the reflecting or refracting substance which move faster than the rays and overtake them. When a ray is in that part of a vibration which conspires with its motion, it easily breaks through the refracting surface—it is in a fit of easy transmission; and, conversely, when the motion of the ray and the wave are opposed, it is in a fit of easy reflexion.

But he was not always so cautious At an earlier date (1675) he sent to Oldenburg, for the Royal Society, an "Hypothesis explaining the Properties of Light"; and we find from the journal book that "these observations so well pleased the society that they ordered Mr. Oldenburg to desire Mr. Newton to permit them to be published." Newton agreed, but asked that publication should be deferred till he had completed the account of some other experiments which ought to precede those he had described. This he never did, and the hypothesis was first printed in Birch's "History of the Royal Society," vol. iii., pp. 247, 262, 272, &c.; it is also given in Brewster's "Life of Newton," vol. i., App. II., and in the *Phil. Mag.*, September, 1846, pp. 187-213.

"Were I," he writes in this paper, "to assume an hypothesis, it should be this, if propounded more generally, so as not to assume what light is further than that it is something or other capable of exciting vibrations of the ether. First, it is to be assumed that there is an ethereal medium, much of the same constitution with air, but far rarer, subtiller, and more strongly elastic. . . . In the second place, it is to be supposed that the ether is a vibrating medium, like air, only the vibrations far more swift and minute ; those of air made by a man's ordinary voice succeeding at more than half a foot or a foot distance, but those of ether at a less distance than the hundred-thousandth part of an inch. And as in air the vibrations are some larger than others, but yet all equally swift . . . so I suppose the ethereal vibrations differ in bigness but not in swiftness. . . . In the fourth place, therefore, I suppose that light is neither ether nor its vibrating motion, but something of a different kind propagated from lucid bodies. They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies at great distances one after the other, but yet without any sensible interval of time. . . . To avoid dispute and make this hypothesis general, let every man here take his fancy ; only, whatever light be, I would suppose it consists of successive rays differing from one another in contingent circumstances, as bigness, force, or vigour, like as the sands on the shore . . . and, further, I would suppose it diverse from the vibrations of the ether. . . . Fifthly, it is to be supposed that light and ether mutually act upon one another." It is from this action that reflexion and refraction come about ; "æthereal vibrations are therefore," he continues, "the best means by which such a subtile agent as light can shake the gross particles of solid bodies to heat them. And so, supposing that light impinging on a refracting or reflecting ethereal superficies puts it into a vibrating motion, that physical superficies being by the perpetual appulse of rays always kept in a vibrating motion, and the ether therein continually expanded and compressed by turns, if a ray of light impinge on it when it is much compressed, I suppose it is then too dense and stiff to let the ray through, and so reflects it ; but the rays that impinge on it at other times, when it is either expanded by the interval between two vibrations or not too much compressed and condensed, go through and are refracted. . . . And now to explain colours. I suppose that as bodies excite sounds of various tones and consequently vibrations, in the air of various bignesses, so when the rays of light by impinging on the stiff refracting superficies excite vibrations in the ether, these rays excite vibrations of various bignesses . . . therefore, the ends of the capillamenta of the optic nerve which front or face the retina being such refracting superficies, when the rays impinge on them they must there excite these vibrations, which vibrations (like those of sound in a trumpet) will run along the aqueous pores or crystalline pith of the capillamenta through the optic nerves into the sensorium (which light itself cannot do), and there, I suppose, affect the sense with various colours, according to their bigness and mixture—the biggest with the strongest colours, reds and yellows ; the least with the weakest, blues and violets ; the middle with green ; and a confusion of all with white."

The last idea, the relation of colour to the bigness of wave-length, is put even more plainly in the "Opticks," Query 13 (ed. 1704) :—"Do not several sorts of rays make vibrations of various bignesses, which according to their bignesses excite sensations of various colours . . . and, particularly, do not the most refrangible rays excite the shortest vibrations for making a sensation of deep violet ; the least refrangible the largest for making a sensation of deep red ?"

The whole is but a development of a reply, written in 1672, to a criticism of Hooke's on his first optical paper, in which Newton says : "It is true that from my theory I argue the corporeity of light, but I do it without any absolute positiveness, as the word perhaps intimates, and make it at most a very plausible consequence of the doctrine, and not a fundamental supposition. Certainly," he continues, "my hypothesis has a much greater affinity with his own [Hooke's] than he seems to be aware of, the vibrations of the ether being as useful and necessary in this as in his."

Thus Newton, while in the "Opticks" he avoided declaring himself as to the mechanism by which the fits of easy reflexion and transmission were produced, has in his earlier writings developed a theory practically identical in many respects with modern views, though without saying that he accepted it. It was an hypothesis ; one difficulty remained, it would not account for the rectilinear propagation, and it must be rejected till it did.

Light is neither ether nor its vibrating motion ; it is energy which, emitted from luminous bodies, is carried by wave motion in rays, and falling on a reflecting surface sets up fresh waves by which it is in part transmitted and in part reflected. Light is not material, but Newton nowhere definitely asserts that it is. He "argues the corporeity of light, but without any absolute positiveness." In the "Principia," writing of his particles, his words are : "Harum attractionum haud multum dissimiles sunt Lucis reflexiones et refractiones" ; and the Scholium concludes with "Igitur ob analogiam quæ est inter propagationem radiorum lucis et progressum corporum, visum est propositiones sequentes in usus opticos subjungere ; interea de natura radiorum (utrum sint corpora necne) nihil omnino disputans, sed trajectorias corporum trajectoriis radiorum persimiles solummodo determinans." [1]

No doubt Newton's immediate successors interpreted his words as meaning that he believed in the corpuscular theory, conceived, as Herschel says, by Newton, and called by his illustrious name. Men learnt from the "Principia" how to deal with the motion of small particles under definite forces. The laws of wave motion were obscure, and till the days of Young and Fresnel there was no second Newton to explain them. There is truth in Whewell's words ("Inductive Sciences," ii. chap. x.) : "That propositions existed in the 'Principia' which proceeded on this hypothesis was with many ground enough for adopting the doctrine." Young's view, already quoted, appears to me more just ; and I see in Newton's hypothesis the first clear indication of the undulatory theory of light, the first statement of its fundamental laws.

Three years later (1678) Huygens wrote his "Traité de la Lumière," published in 1690. He failed to meet the main difficulty of the theory, but in other respects he developed its consequences to a most remarkable degree. For more than a century after this there was no progress, until in 1801 the principle of interference was discovered by Young, and again independently a few years later by Fresnel, whose genius triumphed over the difficulties to which his predecessors had succumbed, and, by combining the principles of interference and transverse vibrations, established an undulatory theory as a fact, thus making Newton's theory a *vera causa*.

There is, however, a great distinction between the emission theory as Newton left it and Fresnel's undulatory theory. The former was dynamical, though it could explain but little : the particles of light obeyed the laws of motion, like particles of matter. The undulatory theory of Huygens and Fresnel was geometrical or kinematical : the structure of the ether was and is unknown ; all that was needed was that light should be due to the rapid periodic changes of some vector property of a medium capable of transmitting transverse waves. Fresnel, it is true, attempted to give a dynamical account of double refraction, and of the reflexion and refraction of polarised light, but the attempt was a failure ; and not the least interesting part of Mr. L. Fletcher's recent book on double refraction ("The Optical Indicatrix") is that in which he shows that Fresnel himself in the first instance arrived at his theory by purely geometrical reasoning, and only attempted at a later date to give it its dynamical form. "If we reflect," says Stokes ("Report on Double Refraction," *Brit. Assoc. Report*, 1862, p. 254), "on the state of the subject as Fresnel found it and as he left it, the wonder is, not that he failed to give a rigorous dynamical theory, but that a single mind was capable of effecting so much." Every student of optics should read Fresnel's great memoirs.

But the time was coming when the attempt to construct a dynamical theory of light could be made. Navier, in 1821, gave the first mathematical theory of elasticity. He limited himself to isotropic bodies, and worked on Boscovitch's hypothesis as to the constitution of matter. Poisson followed on the same lines, and the next year (1822) Cauchy wrote his first memoir on elasticity. The phenomena of light afforded a means of testing this theory of elasticity, and accordingly the first mechanical conception of the ether was that of Cauchy and Neumann, who conceived it to consist of distinct hard particles acting upon one another with forces in the line joining them, which vary as some function of the distances between the particles. It was now possible to work out a mechanical theory of light which should be a necessary consequence of these hy-

[1] The reflexions and refractions of light are not very unlike these attractions. Therefore, because of the analogy which exists between the propagation of rays of light and the motion of bodies, it seemed right to add the following propositions for optical purposes, not at all with any view of discussing the nature of rays (whether they are corporeal or not), but only to determine paths of particles which closely resemble the paths of rays.—"Principia," lib. i., sect. xiv., prop. xcvi., Scholium.

potheses. Cauchy's and the earlier theories do not represent the facts either in an elastic solid or in the ether. At present we are not concerned with the cause of this; we must recognise it as the first attempt to explain on a mechanical basis the phenomena observed. According to his theory in its final form, there are, in an isotropic medium, two waves which travel with velocities $\sqrt{A/\rho}$ and $\sqrt{B/\rho}$, A and B being constants and ρ the density. Adopting Cauchy's molecular hypothesis, there must be a definite relation between A and B.

A truer view of the theory of elasticity is given by Green in his paper read before the Cambridge Philosophical Society in 1837. This theory involves the two constants, but they are independent, and to account for certain optical effects A must either vanish or be infinite. The first supposition was, until a few years since, thought to be inconsistent with stability; the second leads to consequences which in part agree with the results of optical experiment, but which differ fatally from those results on other points. And so the first attempt to construct a mechanical theory of light failed. We have learnt much from it. At the death of Green the subject had advanced far beyond the point at which Fresnel left it. The causes of the failure are known, and the directions in which to look for modifications have been pointed out.

Now I believe that the effort to throw any theory into mechanical form, to conceive a model which is a concrete representation of the truth, to arrive at that which underlies our mathematical equations wherever possible, is of immense value to every student. Such a course, I am well aware, has its dangers. It may be thought that we ascribe to the reality all the properties of the model, that, in the case of the ether, we look upon it as a collection of gyrostatic molecules and springs, or of pulleys and indiarubber bands, instead of viewing it from the standpoint of Maxwell, who hoped, writing of his own model, "that by such mechanical fictions, anyone who understands the provisional and temporary character of his hypothesis will find himself helped rather than hindered in his search after the true interpretation of the phenomena." Prof. Boltzmann, in his most interesting paper on "The Methods of Theoretical Physics" (*Phil. Mag.*, July, 1893) has quoted these words, and has expressed far more ably than I can hope to do the idea I wish to convey.

The elastic solid theory, then, has failed; but are we therefore without any mechanical theory of light? Are we again reduced to merely writing down our equations, and calling some quantity which appears in them the amplitude of the light vibration, and the square of that quantity the intensity of the light? Or can we take a further step? Let us inquire what the properties of the ether must be which will lead us by strict reasoning to those equations which we know represent the laws of the propagation of light.

These equations resemble in many respects those of an elastic solid; let us, then, for a moment identify the displacement in a light-wave with an actual displacement of a molecule of some medium having properties resembling that of a solid. Then this medium must have rigidity or quasi-rigidity in order that it may transmit transverse waves; at the same time it must be incapable of transmitting normal waves, and this involves the supposition that the quantity A which appears in Green's equations must vanish or be infinite. To suppose it infinite is to recur to the incompressible solid theory; we will assume, therefore, that it is zero. Reflexion and refraction show us that the ether in a transparent medium such as glass differs in properties from that in air. It may differ either (1) in density or effective density,[1] or (2) in rigidity or effective rigidity. The laws of double refraction, and the phenomena of the scattering of light by small particles, show us that the difference is, in the main, in density or effective density; the rigidity of the ether does not greatly vary in different media. Dispersion, absorption, and anomalous dispersion all tell us that in some cases energy is absorbed from the light vibrations by the matter through which they pass, or, to be more general, by something very intimately connected with the matter.

We do not know sufficient to say what that action must be; we can, however, try the consequences of various hypotheses. Guided by the analogy of the motion of a solid in a fluid, let us assume that the action is proportional to the acceleration of the ether particles relative to the matter, and, further, that under certain circumstances some of the energy of the ether particles is transferred to the matter, thus setting them in vibration. If such action be assumed, the actual density of the ether may be the same in all media, the mathematical expression for the forces will lead to the same equations as those we obtain by supposing that there is a variation of density, and since it is clearly reasonable to suppose that this action between matter and ether is, in a crystal a function of the direction of vibration, the apparent or effective density of the ether in such a body will depend on the direction of displacement.

Now these hypotheses will conduct us by strict mathematical reasoning to laws for the propagation, reflexion and refraction, double refraction and polarisation, dispersion, absorption, and anomalous dispersion and aberration of light which are in complete accordance with the most accurate experiments.

The rotatory polarisation of quartz, sugar, and other substances points to a more complicated action between the ether and matter than is contemplated above; and, accordingly, other terms have to be introduced into the equations to account for these effects. It will be noted as a defect, and perhaps a fatal one, that the connection between electricity and light is not hinted at, but I hope to return to that point shortly.

Such a medium as I have described is afforded us by the labile ether of Lord Kelvin. It is an elastic solid or quasi-solid incapable of transmitting normal waves. The quantity A is zero, but Lord Kelvin has shown that the medium would still be stable provided its boundaries are fixed, or, which comes to the same thing, provided it extends to infinity. Such a medium would collapse if it were not held fixed at its boundaries; but if it be held fixed, and if then all points on any closed spherical surface in the medium receive a small normal displacement, so that the matter within the surface is compressed into a smaller volume, there will be no tendency either to aid or to prevent this compression, the medium in its new state will still be in equilibrium, the stresses in any portion of it which remains unaltered in shape are independent of its volume, and are functions only of the rigidity and, implicitly, of the forces which hold the boundary of the whole medium fixed.

A soap film affords in two dimensions an illustration of such a medium; the tension at any point of the film does not depend on the dimensions; we may suppose the film altered in area in any way we please—so long as it remains continuous—without changing the tension. Waves of displacement parallel to the surface of the film would not be transmitted. But such a film, in consequence of its tension, has an apparent rigidity for displacements normal to its surface: it can transmit transverse waves with a velocity which depends on the tension. Now the labile ether is a medium which has, in three dimensions, characteristics resembling those of the two-dimensional film. Its fundamental property is that the potential energy per unit volume, in an isotropic body, so far as it arises from a given strain, is proportional to the square of the resultant twist. In an incompressible elastic ether this potential energy depends upon the shearing strain. Given such a medium—and there is nothing impossible in its conception—the main phenomena of light follow as a necessary consequence. We have a mechanical theory by the aid of which we can explain the phenomena; we can go a few steps behind the symbols we use in our mathematical processes. Lord Kelvin, again, has shown us how such a medium might be made up of molecules having rotation in such a way that it could not be distinguished from an ordinary fluid in respect to any irrotational motion; it would, however, resist rotational movements with a force proportionate to the twist, just the force required; the medium has no real rigidity, but only a quasi-rigidity conferred on it by its rotational motion. The actual periodic displacements of such a medium may constitute light. We may claim, then, with some confidence to have a mechanical theory of light.

But nowadays the ether has other functions to perform, and there is another theory to consider, which at present holds the field. Maxwell's equations of the electromagnetic field are practically identical with those of the quasi-labile ether. The symbols which occur can have an electromagnetic meaning; we speak of permeability and inductive capacity instead of rigidity and density, and take as our variables the electric or magnetic displacements instead of the actual displacement or the rotation.

[1] The equations of motion for a medium such as is supposed above can be written—

$\rho \times$ acceleration of ether $+ \rho' \times$ acceleration of matter $= \Sigma B \times$ function of ether displacements, and their differential coefficients with respect to the co-ordinates $+ \Sigma B' \times$ similar function for matter displacements.

The quantity ρ may be spoken of as the effective ether density, the quantities B as the effective elasticity or rigidity.

Still such a theory is not mechanical. Electric force acts on matter charged with electricity, and the ratio of the force to the charge can be measured in mechanical units. A fundamental conception in Maxwell's theory is electric displacement, and this is proportional to the electric force. Moreover, its convergence measures the quantity of electricity present per unit volume; but we have no certain mechanical conception of electric displacement or quantity of electricity, we have no satisfactory mechanical theory of the electromagnetic field. The first edition of the "Electricity and Magnetism" appeared twenty years ago. In it Maxwell says: "It must be carefully borne in mind that we have made only one step in the theory of the action of the medium. We have supposed it to be in a state of stress, but we have not in any way accounted for this stress or explained how it is maintained. This step, however, appears to me to be an important one, as it explains by the action of consecutive parts of the medium phenomena which were formerly supposed to be explicable only by direct action at a distance. I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric." And these words are true still.

But, for all this, I think it may be useful to press the theory of the quasi-labile ether as far as it will go, and endeavour to see what the consequences must be.

The analogy between the equations of the electromagnetic field and those of an elastic solid has been discussed by many writers. In a most interesting paper on the theory of dimensions, read recently before the Physical Society, Mr. Williams has called attention to the fact that two only of these analogies have throughout a simple mechanical interpretation. These two have been developed at some length by Mr. Heaviside in his paper in the *Electrician* for January 23, 1891. To one of them Lord Kelvin had previously called attention ("Collected Papers," vol. iii. p. 450.)

Starting with a quasi-labile ether, then, we may suppose that μ, the magnetic permeability of the medium, is $4\pi\rho$,[1] where ρ is the density, and that K, the inductive capacity, is $1/4\pi B$. B being the rigidity, or the quasi-rigidity conferred by the rotation.

The kinetic energy of such a medium is $\frac{1}{2}\rho(\xi^2 + \eta^2 + \zeta^2)$, where ξ, η, ζ are the components of the displacement. Let us identify this with the electromagnet energy $(\alpha^2 + \beta^2 + \gamma^2)8\pi$, α, β, γ being components of the magnetic force, so that $\alpha = \dot{\xi}, \beta = \dot{\eta}, \gamma = \dot{\zeta}$. Then the components of the electric displacement, assuming them to be zero initially, are given by

$$f = \frac{1}{4\pi}\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right), \text{ \&c.};$$

that is, the electric displacement $\mathfrak{D}$ multiplied by 4π is equal to the rotation in the medium. Denote this by Ω.

The potential energy due to the strain is

$$\tfrac{1}{2}B\Omega^2, \text{ or } \tfrac{1}{2}16\pi^2 B\mathfrak{D}^2,$$

and on substituting for B this becomes

$$\frac{1}{2}\frac{4\pi}{K}\mathfrak{D}^2,$$

which is Maxwell's expression for the electrostatic energy of the field.

Thus so far, but no farther, the analogy is complete; the kinetic energy of the medium measures the magnetic energy, the potential energy measures the electrostatic energy. The stresses in the ether, however, are not those given by Maxwell's theory.

In the other form of the analogy we are to take the inductive capacity as $4\pi\rho$ and the magnetic permeability as $1/4\pi B$. The velocity measures the electric force, and the rotation the magnetic force, so that electrostatic energy is kinetic, and magnetic energy potential. Such an arrangement is not so easy to grasp as the other. Optical experiments, however, show us that in all probability it is ρ, and not B, which varies, while from our electrical measurements we know that K is variable and μ constant; hence this is a reason for adopting the second form.

In either case we look upon the field as the seat of energy distributed per unit of volume according to Maxwell's law. The total energy is obtained by integration throughout the field.

[1] If we adopted Mr. Heaviside's rational system of units the 4π would disappear.

Now we can transform this integral by Green's theorem to a surface integral over the boundary, together with a volume integral through the space; and the form of these integrals shows us that we may look upon the effects, dealing for the present with electrostatics only, as due to the attractions and repulsions of a certain imaginary matter distributed according to a definite law over the boundary and throughout the space. To this imaginary matter, then, in the ordinary theory we give the name of Electricity.

Thus an electrified conducting sphere, according to these analogies, is not a body charged with a quantity of something we call electricity, but a surface at which there is a discontinuing in the rotation impressed upon the medium, or in the flow across the surface; for in the conductor a viscous resistance to the motion takes the place of rigidity. No permanent strain can be set up.

From this standpoint we consider electrical force as one of the manifestations of some action between ether and matter. There are certain means by which we can strain the ether: the friction of two dissimilar materials, the chemical action in a cell are two; and when, adopting the first analogy, this straining is of such a nature as to produce a rotational twist in the ether, the bodies round are said to be electrified; the energy of the system is that which would arise from the presence over their surfaces of attracting and repelling matter, attracting or repelling according to the inverse square law. We falsely assign this energy to such attractions instead of to the strains and stresses in the ether.

Such a theory has many difficulties. It is far from being proved; perhaps I have erred in trespassing on your time with it in this crude form. The words of the French *savant*, quoted by Poincaré, will apply to it: "I can understand all Maxwell except what he means by a charged body." It is not, of course, the only hypothesis which might be formed to explain the facts, perhaps not even the most probable. For many points the vortex sponge theory is its superior. Still I feel confident that in time we shall come to see that the phenomena of the electro-magnetic field may be represented by some such mechanism as has been outlined, and that confidence must be my excuse for having ventured to call your attention to the subject.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. EMERSON REYNOLDS, M.D., SC.D., F.R.S., PRESIDENT OF THE SECTION.

AT the Nottingham Meeting of the British Association in 1866, Dr. H. Bence Jones addressed the Section over which I have now the honour to preside on the place of Chemical Science in Medical Education. Without dwelling on this topic to-day, it is an agreeable duty to acknowledge the foresight of my predecessor as to the direction of medical progress. Twenty-seven years ago the methods of inquiry and instruction in medicine were essentially based on the formal lines of the last generation. Dr. Bence Jones saw that modern methods of research in chemistry—and in the experimental sciences generally—must profoundly influence medicine, and he urged the need of fuller training of medical students in those sciences.

The anticipated influence is now operative as a powerful factor in the general progress of medicine and medical education; but much remains to be desired in regard to the chemical portion of that education. In the later stages of it, undue importance is still attached to the knowledge of substances rather than of principles; of products instead of the broad characters of the chemical changes in which they are formed. Without this higher class of instruction it is unreasonable to expect an intelligent perception of complex physiological and pathological processes which are chemical in character, or much real appreciation of modern pharmacological research. I have little doubt, however, that the need for this fuller chemical education will soon be so strongly felt that the necessary reform will come from within a profession which has given ample proof in recent years of its zeal in the cause of scientific progress.

In our own branch of science the work of the year has been substantial in character, if almost unmarked by discoveries of popular interest. We may probably place in the latter category the measure of success which the skill of Moissan has enabled him to attain in the artificial production of the diamond form of carbon, apparently in minute crystals similar to those recognised by Koenig, Mallard, Daubrée, and by Friedel in the supposed meteorite of Cañon de Diablo in Arizona. Members of the Section will probably have the opportunity of examining some of these artificial diamonds through the courtesy of M. Moissan, who has also, at my request, been so good as to arrange for us a demonstration of the properties of the element fluorine, which he succeeded in isolating in 1887.

Not less interesting or valuable are the studies of Dr. Perkins, on electro-magnetic rotation; of Lord Rayleigh, on the relative densities of gases; of Dewar, on chemical relations at extremely low temperatures; of Clowes, on exact measurements of flame-cap indications afforded by miners' testing lamps; of Horace Brown and Morris, on the chemistry and physiology of foliage leaves, by which they have been led to the startling conclusion that cane-sugar is the first sugar produced during the assimilation of carbon, and that starch is formed at its expense as a more stable reserve material for subsequent use of the plant; or of Cross, Bevan, and Beadle, on the interaction of alkali-cellulose and carbon bisulphide, in the course of which they have proved that a cellulose residue can act like an alcohol radical in the formation of thiocarbonates, and thus have added another to the authors' valuable contributions to our knowledge of members of the complex group of celluloses.

But it is now an idle task for a President of this Section to attempt a slight sketch of the works of chemical philosophers even during the short space of twelve months; they are too numerous and generally too important to be lightly treated, hence we can but apply to them a paraphrase of the ancient formula—Are they not written in the books of the chronicles we term "Jahresberichte," "Annales," or "Transactions and Abstracts," according to our nationality?

I would, however, in this connection ask your consideration for a question relating to the utilisation of the vast stores of facts laid up—some might even say buried—in the records to which reference has just been made. The need exists, and almost daily becomes greater, for facile reference to this accumulated wealth, and of such a kind that an investigator, commencing a line of inquiry with whose previous history he is not familiar, can be certain to learn *all* the facts known on the subject up to a particular date, instead of having only the partial record to be found in even the best edited of the dictionaries now available. The best and most obvious method of attaining this end is the publication of a subject-matter index of an ideally complete character. I am glad to know that the Chemical Society of London will probably provide us in the years to come with a compilation which will doubtless aim at a high standard of value as a work of reference to memoirs, and in some degree to their contents, so far as the existing indexes of the volumes of the Society's Journal supply the information. Whether this subject-matter index is published or not, the time has certainly arrived for adopting the immediately useful course of publishing monographs, analogous to those now usual in Natural Science, which shall contain all the information gained up to a particular date in the branch of chemistry with which the author is specially familiar by reason of his own work in the subject. Such monographs should include much more than any mere compilation, and would form the best material from which a complete subject-matter index might ultimately be evolved.

My attention was forcibly drawn to the need of such special records by noting the comparatively numerous cases of re-discovery and imperfect identification of derivatives of thiourea. In my laboratory, where this substance was isolated, we naturally follow with interest all work connected with it, and therefore readily detect lapses of the kind just mentioned. But when it is remembered that the distinct derivatives of thiourea now known number considerably over six hundred substances, and that their descriptions are scattered through numerous British and foreign journals, considerable excuse can be found for workers overlooking former results. The difficulty which exists in this one small department of the science I hope shortly to remove, and trust that others may be induced to provide similar works of reference to the particular branches of chemistry with which they are personally most familiar.

When we consider the drift of investigation in recent years, it is easy to recognise a distinct reaction from extreme specialisation in the prominence now given to general physico-chemical problems, and to those broad questions concerning the relations of the elements which I would venture to group under the head of "Comparative Chemistry." Together these lines of inquiry afford promise of definite information about the real nature of the seventy or more entities we term "elements," and about the mechanism of that mysterious yet definite change in matter which we call "chemical action." Now and again one or other class of investigation enables us to get some glimpse beyond the known which stimulates the imaginative faculty.

For example, a curious side-light seems to be thrown on the nature of the elements by the chemico-physical discussion of the connection existing between the constitution of certain organic compounds and the colours they exhibit. Without attempting to intervene in the interesting controversy in which Armstrong and Hartly are engaged as to the nature of the connection, we may take it as an established fact that a relation exists between the power which a dissolved chemical compound possesses of producing the colour impression within our comparatively small visual range, and the particular mode of grouping of its constituent radicals in its molecule. Further, the reality of this connection will be most freely admitted in the class of aromatic compounds; that is, in derivatives of benzene, whose constituents are so closely linked together as to exhibit quasi-elemental persistence. If then, the possession of what we call colour by a compound be connected with its constitution, may we not infer that "elements" which exhibit distinct colour, such as gold and copper, in thin layers and in their soluble compounds, are at least complexes analogous to definitely decomposable substances? This inference, while legitimate as it stands, would obviously acquire strength if we could show that anything like isomerism exists among the elements; for identity of atomic weight of any two chemically distinct elements must, by all analogy with compounds, imply dissimilarity in constitution, and, therefore, definite structure, independently of any argument derived from colour. Now, nickel and cobalt are perfectly distinct elements, as we all know, but, so far as existing evidence goes, the observed differences in their atomic weights (nickel 58·6, cobalt 58·7) are so small as to be within the range of the experimental errors to which the determinations were liable. Here, then, we seem to have the required example of something like isomerism among elements, and consequently some evidence that these substances are complexes of different orders; but in the cases of cobalt and nickel we also know that in transparent solutions of their salts, if not in thin layers of the metals themselves, they exhibit strong and distinct colours—compare the beautiful rosy tint of cobalt sulphate with the brilliant green of the corresponding salt of nickel. Therefore, in exhibiting characteristically different colours, these substances afford us some further evidence of structural differences between the matter of which they consist, and support the conclusion to which their apparent identity in atomic weight would lead us. By means of such side-lights we may gradually acquire some idea of the nature of the elements, even if we are unable to get any clue to their origin other than such as may be found in Crookes' interesting speculations.

Again, while our knowledge of the genesis of the chemical elements is as small as astronomers possess of the origin of the heavenly bodies, much suggestive work has recently been accomplished in the attempt to apply the principle of gravitation, which simply explains the relative motions of the planets, to account for the interactions of the molecules of the elements. The first step in this direction was suggested by Mendeleef in his Royal Institution lecture (May 31, 1889) wherein he proposed to apply Newton's third law of motion to chemical molecules, regarded as systems of atoms analagous to double stars. The Rev. Dr. Haughton has followed up this idea with his well-known mathematical skill, and, in a series of papers just published, has shown that the three Newtonian laws are applicable to explain the interactions of chemical molecules, "with this difference, that whereas the specific coefficient of gravity is the same for all bodies, independent of the particular kind of matter of which they are composed, the atoms have specific coefficients of attraction which vary with the nature of the atoms concerned." The laws of gravitation, with this proviso, were found to apply to all the definite cases examined, and it was shown that a chemical change of combination is equivalent to a planetary catastrophe. So far the fundamental hypothesis of "Newtonian Chemistry" has led to conclusions which are not at variance with the facts of the science, while it gives promise of help in obtaining a solution of the great problem of the nature of chemical action.

Passing from considerations of the kind to which I have just referred, permit me to occupy the rest of the time at my disposal with a short account of a line of study in what I have already termed "comparative chemistry," which is not only of inherent interest, but seems to give us the means of filling in some details of a hitherto rather neglected chapter in the early chemical history of this earth.

The most remarkable outcome of "comparative chemistry" is the periodic law of the elements, which asserts that the properties of the elements are connected in the form of a periodic function with the masses of their atoms. Concurrently with the recognition of this principle, other investigations have been in progress, aiming at more exact definitions of the characters of the relations of the elements, and ultimately of their respective offices in nature. Among inquiries of this kind the comparative study of the elements carbon and silicon appears to me to possess the highest interest. Carbon, whether combined with hydrogen, oxygen, or nitrogen, or with all three, is the great element of organic nature, while silicon, in union with oxygen and various metals, not only forms about one-third of the solid crust of the earth, but is unquestionably the most important element of inorganic nature. The chief functions of carbon are those which are performed at comparatively low temperatures; hence carbon is essentially the element of the present epoch. On the other hand, the activities of silicon are most marked at very high temperatures; hence it is the element whose chief work in nature was performed in the distant past, when the temperature of this earth was far beyond that at which the carbon compounds of organic life could exist. Yet between these dominant elements of widely different epochs remarkably close analogies are traceable, and the characteristic differences observed in their relations with other elements are just those which enable each to play its part effectively under the conditions which promote its greatest activity.

The chemical analogies of the two tetrad elements carbon and silicon are most easily recognised in compounds which either do not contain oxygen, or which are oxygen compounds of a very simple order, and the following table will recall a few of the most important of these, as well as some which have resulted from the fine researches of Friedel, Crafts, and Ladenburg:—

Some Silicon Analogues of Carbon Compounds.

SiH_4	Hydrides	CH_4
$SiCl_4$	Chlorides	CCl_4
Si_2Cl_6	Chlorides	C_2Cl_6
SiO_2	Oxides	CO_2
H_2SiO_3	Meta Acids	H_2CO_3
$HSiHO_2$	Formic Acids	$HCHO_2$
$(SiHO)_2O$	Formic Anhydrides	$(CHO)_2O$?
$H_2Si_2O_4$	Oxalic Acids	$H_2C_2O_4$
$HSi(CH_3)O_2$	Acetic Acids	$HC(CH_3)O_2$
$HSi(C_6H_5)O_2$	Benzoic Acids	$HC(C_6H_5)O_2$
$SiC_8H_{19}H$	Nonyl Hydrides	$C_9H_{19}H$
$SiC_8H_{19}OH$	Nonyl Alcohols	$C_9H_{19}OH$

But these silicon analogues of carbon compounds are, generally, very different from the latter in reactive power, especially in presence of oxygen and water. For example, hydride of silicon, even when pure, is very easily decomposed, and, if slightly warmed, is spontaneously inflammable in air; whereas the analogous marsh gas does not take fire in air below a red heat. Again, the chlorides of silicon are rapidly attacked by water affording silicon hydroxides and hydrochloric acid; but the analogous carbon chlorides are little affected by water even at comparatively high temperatures. Similarly, silicon-chloroform and water quickly produce silico-formic acid and anhydride along with hydrochloric acid, while ordinary chloroform can be kept in contact with water for a considerable time without material change.

Until recently no well-defined compounds of silicon were known including nitrogen; but we are now acquainted with a number of significant substances of this class.

Chemists have long been familiar with the fact that a violent reaction takes place when silicon chloride and ammonia are allowed to interact. Persoz, in 1830, assumed that the resulting white powder was an addition compound, and assigned to it the formula $SiCl_4, 6NH_3$, while Besson, as lately as 1892, gave $SiCl_4, 5NH_3$. These formulæ only express the proportions in which ammonia reacts with the chloride under different conditions and give us no information as to the real nature of the product; hence they are almost useless. Other chemists have, however, carefully examined the product of this reaction, but owing to peculiar difficulties in the way have not obtained results of a very conclusive kind. It is known that the product when strongly heated in a current of ammonia gas affords ammonium chloride, which volatilises, and a residue, to which Schutzenberger and Colson have assigned the formula Si_2N_3H. This body they regard as a definite hydride of Si_2N_3, which latter they produced by acting on silicon at a white heat with pure nitrogen. Gattermann suggests that a nearer approach to the silicon analogue of cyanogen, Si_2N_2, should be obtained from the product of the action of ammonia on silicon-chloroform; but it does not appear that this suggestion has yet borne fruit. It was scarcely probable that the above-mentioned rather indefinite compounds of silicon with nitrogen were the only ones of the class obtainable, since bodies including carbon combined with nitrogen are not only numerous but are among the most important carbon compounds known. Further investigation was therefore necessary in the interests of comparative chemistry, and for special reasons which will appear later on; but it was evident that a new point of attack must be found.

A preliminary experimental survey proved the possibility of forming numerous compounds of silicon containing nitrogen, and enabled me to select those which seemed most likely to afford definite information. For much of this kind of work silicon chloride was rather too energetic, hence I had a considerable quantity of the more manageable silicon tetrabromide prepared by Serullas' method, viz. by passing the vapour of crude bromine (containing a little chlorine) over a strongly heated mixture of silica and charcoal. In purifying this product I obtained incidentally the chloro-bromide of silicon, $SiClBr_3$, which was required in order to complete the series of possible chlorobromides of silicon.[1]

Silicon bromide was found to produce addition compounds very readily with many feebly basic substances containing nitrogen. But one group of bromides of this class has yet been investigated in detail, namely, the products afforded by thioureas. The typical member of this group is the perfectly definite but uncrystalline substance

$$SiBr_2\begin{cases}(CSN_2H^4)_4Br\\(CSN_2H_4)_4Br\end{cases}$$

Substituted thioureas afford similar bodies, the most interesting of which is the allyl compound. This is a singularly viscid liquid, which requires several days at ordinary temperatures to regain its level, when a tube containing it is inverted. But these are essentially addition compounds, and are therefore comparatively unimportant.

In most cases, however, the silicon haloids enter into very definite reaction with nitrogen compounds, especially when the latter are distinctly basic, such as aniline or any of its homologues. One of the principal products of this class of change is the beautiful typical substance on the table, which is the first well-defined crystalline compound obtained in which silicon is exclusively combined with nitrogen. Its composition is $Si(NHC_6H_5)_4$.[2] Analogous compounds have been formed with the toluidines, naphthylamines, &c., and have been examined in considerable detail, but it suffices to mention them and proceed to point out the nature of the changes we can effect by the action of heat on the comparatively simple anilide.

When silicon anilide is heated carefully *in vacuo* it loses one molecule of aniline very easily and leaves triphenyl-guanidine, probably the a modification; if the action of heat be continued, but at ordinary pressure and in a current of dry hydrogen, another molecule of aniline can be expelled, and, just before the last trace of the latter is removed, the previously liquid substance solidifies and affords a silicon analogue of the insoluble modification of carbodiphenyldiimide, which may then be heated moderately without undergoing further material change. A comparison of the formulæ will make the relations of the products clear:—

Silicotetraphenylamide—$Si(NHPh)_4$
Silicotriphenylguanidine—$Si : NPh. (NHPh)_2$
Silicodiphenyldiimide—$Si : (NPh)_2$.

Moreover, the diimide has been heated to full redness in a gas combustion furnace while dry hydrogen was still passed over it; even under these conditions little charring occurred, but some

[1] Three years later Besson formed the same compound, and described it as new.
[2] Harden has obtained an uncrystalline intermediate compound $SiCl_2(NHC_6H_5)_2$.

nitrogen and a phenyl radical were eliminated, and the purified residue was found to approximate in composition to SiNPh, which would represent the body as phenylsilicocyanide or a polymer of it. Even careful heating of the diimide in ammonia gas has not enabled me to remove all the phenyl from the compound, but rather to retain nitrogen, as the best residue obtained from such treatment consisted of Si_2N_3Ph, or the phenylic derivative of one of the substances produced by Schutzenberger and Colson from the ammonia reaction. It may be that both these substances are compounds of silicocyanogen with an imide group of the kind below indicated—

$$\begin{matrix} SiN \\ SiN \end{matrix}\!\!>\!NH \quad : \quad \begin{matrix} SiN \\ SiN \end{matrix}\!\!>\!NPh$$

Further investigation must decide whether this is a real relationship; if it be, we should be able to remove the imidic group and obtain silicocyanogen in the free state. One other point only need be noticed, namely, that when the above silicon compounds are heated in oxygen they are slowly converted into SiO_2; but the last traces of nitrogen are removed with great difficulty, unless water-vapour is present, when ammonia and silica are quickly formed.

Much remains to be done in this department of comparative chemistry, but we may fairly claim to have established the fact that silicon, like carbon, can be made to form perfectly well-defined compounds in which it is exclusively united with the triad nitrogen of amidic and imidic groups.

Now, having proved the capacity of silicon for the formation of compounds of this order with a triad element, Nature very distinctively lets us understand that nitrogen is not the particular element which is best adapted to place the triad *rôle* towards silicon in its high-temperature changes, which are ultimately dominated by oxygen. We are not acquainted with any natural compounds which include silicon and nitrogen; but large numbers of the most important minerals contain the pseudo-triad element aluminum combined with silicon, and few include any other triad. Phosphorus follows silicon in the periodic system of the elements as nitrogen does carbon, but silicates containing more than traces of phosphorus are rare; on the other hand, silicates are not uncommon containing boron, the lower homologue of aluminum; for example, axinite, datholite, and tourmaline.

Moreover, it is well known that silicon dissolves freely in molten aluminum, though much more of the former separates on cooling. Winkler has analysed the gangue of aluminum saturated with silicon, and found that its composition is approximately represented by the formula SiAl, or, perhaps, Si_2Al_2, if we are to regard this as analogous to C_2N_2 or cyanogen. Here aluminum at least resembles nitrogen in directly forming a compound with silicon at moderately high temperature. It would appear, then, that while silicon can combine with both the triads nitrogen and aluminum, the marked positive characters of the latter, and its extremely low volatility, suit it best for the production of permanent silicon compounds similar to those which nitrogen can afford.

With these facts in mind we may carry our thoughts back to that period in the earth's history when our planet was at a higher temperature than the dissociation point of oxygen compounds. Under such conditions the least volatile elements were probably liquids, while silicides and carbides of various metals were formed in the fluid globe. We can imagine that the attraction of aluminum for the large excess of silicon would assert itself, and that, as the temperature fell below the point at which oxidation become possible, these silicides and carbides underwent some degree of oxidation, the carbides suffering most owing to the volatility of the oxides of carbon, while the fixity of the products of oxidation of silicides rendered the latter process a more gradual one. The oxidation of silicides of metals which had little attraction for silicon would lead to the formation of simple metallic silicates and to the separation of the large quantities of free silica we meet with in the solid crust of the earth, whereas oxidation of silicides of aluminum would not break up the union of the two elements, but rather cause the ultimate formation of the alumino-silicates which are so abundant in most of our rocks.

Viewed in the light of the facts already cited and the inferences we have drawn from them as to the nitrogen-like relationship of aluminum to silicon, I am disposed to regard the natural alumino-silicates as products of final oxidation of sometime active silico-aluminum analogues of carbo-nitrogen compounds, rather than ordinary double salts. It is generally taken for granted that they are double salts, but recent work on the chromoxalates by E. A. Werner has shown that this view is not necessarily true of all such substances.

Without going into undue detail we can even form some conception of the general course of change from simple aluminum silicide to an alumino-silicate, if we allow the analogies already traced to lead us further.

We recognise the existence of silico-formyl in Friedel and Ladenburg's silico-formic anhydride; hence silico-triformamide is a compound whose probable formation we can admit, and, on the basis of our aluminum-nitrogen analogy, an aluminum representative also. Thus—

$$N\!\begin{cases} COH \\ COH \\ COH \end{cases} : \quad N\!\begin{cases} SiOH \\ SiOH \\ SiOH \end{cases} : \quad Al\!\begin{cases} SiOH \\ SiOH \\ SiOH \end{cases} : \quad Al\!\begin{cases} SiO_2R' \\ SiO_2R' \\ SiO_2R' \end{cases}$$

Triformamide. Silico-triformamide. Silico-alumino-triformamide. Salt of an alumino-silicic acid.

Now, oxidation of triformamide would lead to complete resolution into nitrogen gas, carbondioxide gas and water rendering it an extremely unstable body; under similar conditions silico-triformamide would probably afford nitrogen gas and silicic acid (or silicon dioxide and water); while the third compound, instead of breaking up, would (owing to the fixity of aluminum as compared with nitrogen) be likely at first to afford a salt of an alumino-silicic acid, in presence of much basic material.

The frequent recurrence of the ratios Si_3Al, Si_3Al_2, &c., in the formulæ of natural alumino-silicates, suggests that some at least of these minerals are derived from oxidation products of the above triformic type. Without stopping to trace all the possible stages in the oxidation of the primary compound $Al(SiO_2R)_3$, or variations in basicity of the products, I may cite the four following examples out of many others which might be given of resulting representative mineral groups:—

$$Al\!\begin{cases} SiO_3R' \\ SiO_3R' \\ SiO_3R' \end{cases} : \quad Al\!\begin{cases} SiO_4R'_3 \\ SiO_4R'_3 \\ SiO_4R''' \end{cases} : \quad Al\!\begin{cases} SiO_4R'_3 \\ SiO_4R''' \\ SiO_4R''' \end{cases} : \quad Al\!\begin{cases} SiO_4R'''^{1} \\ SiO_4R''' \\ SiO_4R''' \end{cases}$$

Beryl type (hemi-). Garnet type. Muscovite type. Xenolite type.

Five years ago Prof. F. W. Clarke, of the United States Geological Survey, published a most interesting paper on the structure of the natural silicates. In this he adopts the view that the mineral xenolite, $Si_3Al_4O_{12}$, is the primary from which all other alumino-silicates may be supposed to arise by various substitutions. Nature, however, seems to teach us that such minerals as xenolite, fibrolite, and the related group of "clays" are rather to be regarded as end-products of a series of hydrolytic changes of less aluminous silicates than primary substances themselves; hence the sketch which I have ventured to give above of the probable genesis of alumino-silicates seems to provide a less arbitrary basis for Clarke's interesting work, without materially disturbing the general drift of his subsequent reasoning.

We may now consider for a moment in what direction evidence can be sought for the existence in nature of derivatives of the hypothetical intermediate products of oxidation between a primary silicide and its fully oxidised silicate.

In the absence of a working hypothesis of the kind which I have already suggested it is not probable that direct evidence would yet be obtainable—this must be work for the future—but when we consider that the existence of compounds of the order in question would manifest themselves in ordinary mineral analyses by the analytical products exceeding the original weight of material, we seem to find some evidence on the point in recorded cases of the kind. A deficiency of a single atom of oxygen in compounds having the high molecular weights of those in question, would be indicated by very small excesses (from 2 to 3 per cent.) whose real meaning might be easily overlooked. Now, such results are not at all unusual in analyses of mineral alumino-silicates. For instance, *Amphiboles* containing a mere trace of iron have afforded 102·75 parts from 100, and almost all analyses of *Microsommite* are high, giving as much as 103 parts. In less degree *Vesuvianite* and members of

[1] In these cases where $R''' = Al$ it is, of course, assumed that the latter is acting only as a basic radical.

the *Andalusite* group may be noted. All these cases may be capable of some other explanations, but I cite them to show that such excesses are commonly met with in published analyses. On the other hand, it is scarcely to be doubted that a good analyst, who obtained a really significant excess, would throw such a result aside as erroneous and never publish it. I therefore plead for much greater care in analyses of the kind in question, and closer scrutiny of results in the light of the suggestions I have ventured to offer. It is probable that silicates containing only partially oxidised aluminum are rare; nevertheless the search for them would introduce a new element of interest into mineralogical inquiries.

If the general considerations I have now endeavoured to lay before you are allowed their full weight, some of the alumino-silicates of our primary rocks reveal to us more than we hitherto supposed. Regarded from this newer standpoint, they are teleoxidised representatives of substances which foreshadowed in terms of silicon, aluminum, and oxygen the compounds of carbon, nitrogen, and hydrogen required at a later stage of the earth's history for living organisms. Thus, while the sedimentary strata contain remains which come down to us from the very dawn of life on this globe, the rocks from whose partial disintegration the preserving strata resulted contain mineral records which carry us still further back, even to Nature's earliest efforts in building up compounds similar to those suited for the purposes of organic development.

NOTES.

PROF. MAX MÜLLER has attained the jubilee of his Doctorate, having taken his degree in 1843, and in honour of the occasion the University of Leipzig has conferred a new diploma upon him.

MR. BELL, of Carlton Street, Nottingham, has brought out, at an opportune moment, "A Contribution to the Geology and Natural History of Nottinghamshire." The little volume is edited by Mr. J. W. Carr, who in his preface records his indebtedness to various friends—specialists in certain departments—who wrote for him some portions of the book. The book was compiled at the request of the Local Excursions' Committee of the British Association, for the use of members attending the Nottingham meeting. We have no doubt that many such will avail themselves of the handy little guide-book, which has been prepared for their special benefit.

THERE seems to be no doubt that the latest report of the death of Emin Pasha is to be relied upon. Mr. A. J. Swann, of Ujiji, from whom the report comes, declares that in his opinion it is as conclusive as anything can be in Africa. And now within a week of the tidings of Emin's death the sudden decease is announced of another African traveller—Surgeon-Major Parke—one of the most widely-known of the members of the Emin Relief Expedition. He died suddenly on the night of Sunday last, while on a visit to the seat of the Duke of St. Albans at Alt-na-Craig.

THE death is announced, at the age of sixty-one, of Dr. Alexander Strauch, the Director of the Zoological Museum of St. Petersburg. Dr. Strauch was an authority on reptiles, and the author of several zoological works.

THE death is reported of Mr. T. W. Kennard, C.E., ounder of the Monmouthshire Crumlin Works, designer and constructor of the Crumlin Viaduct, and engineer-in-chief of the Atlantic and Great Western Railway, United States. He died at the age of 68.

WE have to record the death of a well-known inventor and civil engineer of New York, in the person of Mr. Joseph Battin. He was in his 87th year.

ON and after November 1 next, the railway time throughout the kingdom of Italy will, according to a recent Act of the Legislature, be regulated by the mean solar time of the 15th meridian east of Greenwich, this being the so-called Middle European time. The hours will be reckoned from midnight to midnight. The new time will be 11 minutes in advance of the mean solar time of Rome. It is expected that the other services and the Italian public generally will soon follow the example set by the railway stations.

M. D'ARSONVAL, in *Électricité* for August 24, describes some experiments which he has made on the effects of strong, alternating magnetic fields on animals, his results apparently being somewhat contradictory to those recently obtained in the Edison Laboratory. M. D'Arsonval's experiments were performed by means of coils wound on cylinders of cardboard, glass or wood, large enough to accommodate a man inside them when required. The solenoid thus formed constituted the path for the discharge of a condenser of two to twelve Leyden jars, arranged in two batteries with proper precautions for rendering the discharge oscillatory. The jars were charged periodically by a transformer, giving a current at about 15,000 volts, with a frequency of sixty per second. A lamp held with one terminal in each hand of a man standing within the solenoid, may then be raised by the induced currents to bright incandescence, while M. D'Arsonval asserts that considerable physiological effects are also produced. The method used to determination the strength of these alternating magnetic fields is very ingenious; it consists simply in inserting a mercurial thermometer in the field, and noting the rise of temperature produced by the Foucault currents in the mercury. A considerable rise is very quickly produced in the strongest fields, while for weaker fields a petroleum thermometer is employed, or an air thermometer the bulb of which contains a small copper tube.

DR. W. S. HEDLEY, in an article in the *Lancet*, comments on M. D'Arsonval's work, and mentions some experiments of his own which seem to support the hypothesis that the harmlessness of high frequency alternating currents may be explained by the fact that in these cases there is "virtually no current strength"; *e.g.*, a current of two amperes at 200 volts, if transformed up to 100,000 volts, cannot exceed in strength 0·004 ampere. Another factor concerned in the effect is the "concentration" of the current. Passing a current of high frequency and capable of keeping a 5-candle lamp glowing, through the body by means of copper cylinders held in the hands, produced no appreciable effect beyond a slight warming under the electrodes; using a half-crown as electrode on the forearm, the same negative result follows; with a shilling, there is a slight pricking effect, which becomes quite painful with a threepenny-piece substituted for the shilling, thus indicating that other factors have to be considered, as well as more frequency, in the discussion of the "harmlessness" of alternating currents.

THE issue in a compact form of the interesting series of articles on "Sewage Purification in America," by M. N. Baker, which appeared in the *Engineering News* of New York last year, furnishes an important addition to our information on this complicated subject. The treatment of the sewage of thirty municipalities in the United States and Canada is given in detail, and the description further elucidated by no less than seventy-seven illustrations, including elaborate plans showing the various arrangement of purification, plant, &c. The little pamphlet of 196 pages is well printed and is provided with a copious index. That America has recently devoted much attention to the vexed question of the purification of sewage will be remembered by all who have had occasion to consult the admirable experimental work on the chemical and bacteriological aspects of this subject, conducted at the instigation and under the superintendence of the Massachusetts State Board of Health.

"CHEMICAL and Micro-Mineralogical Researches on the Upper Cretaceous Zones of the South of England" is the title of the thesis sent in by Dr. W. F. Hume for the recent D.Sc. examination at London University. This paper gives a large amount of information on the subject; the zones described being those established by Dr. C. Barrois. The notes refer chiefly to the chalk of the Isle of Wight and Dorsetshire, but they include numerous references to the chalk of other areas, especially to that of Folkestone. The author's researches incline him to the belief that most of the chalk was deposited in fairly deep water; thus differing from M. Cayeux, whose work in the north of France led him to infer a shallow water origin for the chalk of that area. The insoluble residue decreases in quantity as we ascend in the series, and is generally greater in the Isle of Wight than at Folkestone; the excess being especially apparent in the Cenomanian zones. All the Upper Cenomanian beds have undergone secondary silicification.

THE Geological Survey of France has now published rather more than one-half of the country on the scale of 1 : 80,000—138 streets out of a total of 259. An excellent general map on the scale of 1 : 1,000,000 was issued in 1889. The first street (No. 13) of the map on the scale of 1 : 320,000 has just been published. This map, which is a reduction of sixteen streets on the larger scale, has Paris nearly in the centre, and includes Honfleur and Liseaux on the west, Chateaudun and Sens on the south, Nogent and Dormans on the east, Rouen and Beauvais on the north. The tertiary and secondary rocks are well represented within this area, the Silurian, Ordovieian, Cambrian and pre-Cambrian occupying a small space on the south-west, near Mamers. The clay with flints is shown by shading over the chalk. The freshwater and estuarine strata are indicated by shading over the colour denoting the geological formations. Numerous notes on economic geology are printed below the map.

AN interesting study of the compounds of phosphorus and sulphur, by Herr Helff, is published in the current number of the *Zeitschrift für physikalische Chemie*. Hitherto seven sulphides of phosphorus have been described. Observations on vapour-density, and on the boiling-point of solutions in carbon bisulphide indicate, however, that four of these only are true chemical compounds, viz. P_4S_3, P_4S_7, P_3S_6, and P_2S_5. On heating two atomic proportions of phosphorus with three of sulphur, instead of P_2S_3 being formed it appears that the main product is P_4S_7, a little P_4S_3 being also obtained. The substances previously taken to be P_4S and P_4S_2 are merely solutions of sulphur in phosphorus. Incidentally the author confirms the results already arrived at by Beckmann, that when in solution in carbon bisulphide, sulphur has the molecular formula S_8 and phosphorus the formula P_4; he also shows that phosphorus and sulphur when dissolved in carbon bisulphide do not unite even on heating to the ordinary boiling-point. It is also noteworthy that the freezing-points of solutions of sulphur in phosphorus favour the view that here the molecular complexity of sulphur is the same as when it is dissolved in carbon bisulphide.

THE eighth meeting of the International Congress of Hygiene and Demography is to be held during the present month at Buda Pesth, and several international committees have, we understand, been formed with a view to carrying out the decisions of the London Congress. A separate section for tropical countries has been organised, and will meet under the presidency of Dr. Theodor Duka.

THE sixteenth annual meeting of the Library Association was held at Aberdeen on September 4 and 5. In his inaugural address, Dr. Garnett, Keeper of the Printed Books in the British Museum, dwelt upon the cataloguing of books. He said that a catalogue should not merely enable the reader to find a book with the least possible delay, but also present an epitome of the life-work of every author, and assist the literary historian in his researches. Of the papers read, one by Prof. Trail, on "The Classification of Books in the Natural Sciences," was of especial scientific interest.

SEVERAL very interesting lectures will shortly be delivered at the evening meetings of the Camera Club. On September 21 Prof. J. Milne, F.R.S., who is on a short visit from Japan, will discourse upon "The Earthquakes of Japan"; Mr. Lamond Howie will give a lecture, entitled "The Scottish Alps," on September 28; and the October and November programmes will include a paper by Prof. Marshall Ward, F.R.S.

THE Journal of the College of Science, Imperial University, Japan, vol. vi. part 2, is devoted to a paper by Mr. Sadahisa Matsuda on "The Anatomy of Magnoliaceæ." The author splits up *Magnoliaceæ* into the four following groups: (1) Those identical with *Magnolieæ*, (2) those identical with *Schizandreæ* (3) *Trochodendron* and the genera of *Illicieæ*, (4) *Euptelæa* and *Cercidiphyllum*.

A REVISED report on the "Copepoda of Liverpool Bay," by Mr. Isaac C. Thompson, has been published in the Transactions of the Liverpool Biological Society, vol. vii. The report deals with 136 species, eighteen of which are new to the district over which the collection was made, and eleven are considered to be entirely new species. Twenty plates are included, containing a number of outline sketches for facilitating identification.

THE Anthropological Institute has issued an index to its publications. The index includes communications published in the journal and transactions of the Ethnological Society from 1843 to 1871; those in the journal and memoirs of the Anthropological Society (1863-71), and also those that have appeared in the *Anthropological Review*. In 1871 the Ethnological and Anthropological Societies united to form the Anthropological Institute, and since then all papers have appeared in the Institute's journal. The first twenty volumes of the journal are included in the index.

Cosmos contains an article by M. C. Maze, from which it appears that droughts such as we have experienced this year follow a cycle of forty-two years. Since, however, the observations discussed have not been obtained from one place, and there is no clear definition as to what constitutes a dry season, the theory can hardly be said to be above suspicion.

MESSRS. TAYLOR AND FRANCIS will shortly publish a work, by Griffith Brewer and Patrick Y. Alexander, on "Aëronautics," being an abridgement of aëronautical specifications filed at the Patent Office between 1851 and 1891.

MESSRS. G. P. PUTMAN'S SONS have published for Dr. Lauro Sodré, the Governor of Pará, Brazil, a work on "The State of Pará." The work is in five parts, by different contributors, dealing respectively with the history of Pará, physical features, public instruction, revenues and commerce, and industries.

THE *Journal of the Franklin Institute* for September contains among other things the continuation of Nikola Tesla's lecture "On Sight and other High Frequency Phenomena," and the conclusion of the lecture, by Dr. Richards, on "The Specific Heats of the Metals."

THE Calendar of the University College, Bristol, for the session 1893–94 has just been issued through Mr. Arrowsmith, of Bristol.

PROF. KINCH, of the Royal Agricultural College, Cirencester, has just issued a report of some of his field experiments for 1892 and 1893.

MR. W. F. STANLEY, of Great Turnstile, Holborn, has sent us the new edition of his catalogue of mathematical instruments made and sold by him. It is excellently printed upon good paper, and is very admirably classified.

THE U.S. Department of Agriculture, division of Entomology, has lately brought out Vol. v., No. 5, of "Insect Life."

WE are informed by a correspondent that the Rev. L. Blomefield, whose death we recorded in last week's NATURE, was in his ninety-fourth year, and not, as stated by us, in his ninety-first.

FURTHER information concerning hydrazine, N_2H_4, and its compounds is contained in an inaugural dissertation by one of Prof. Curtius's assistants at Kiel, Herr Franz Schrader, and a brief account of it will be found in the current *Chemiker Zeitung*. A considerable time has now elapsed since the first preparation of hydrazine, which was announced by Prof. Curtius in June, 1887, and it is almost three years since full details were published concerning the isolation of the pure hydrate, a liquid of the composition $N_2H_4.H_2O$ which boils at 119°. It was then stated (vide NATURE, vol. 43, p. 205) that in closed vessels this hydrate may be preserved unaltered for any length of time. The experience of the last three years, however, necessitates a modification of that statement, and Herr Schrader now informs us that the liquid stored in sealed tubes decomposes sooner or later, the principal product of decomposition being ammonium hydrate. No gas appears to be generated during the process, so that tubes containing it do not become dangerous from accumulation of pressure. The reactions between the hydrate and a large number of metallic oxides are described, in which the strong reducing proclivities of hydrazine are very markedly exhibited, the reaction being frequently of an explosive character. Herr Schrader further describes a series of double sulphates containing hydrazine sulphate and the sulphate of a metal. Their general formula is $R''SO_4.(N_2H_4)_2H_2SO_4$ where R'' may represent copper, nickel, cobalt, iron, manganese, zinc and cadmium. These double sulphates, which contain no water of crystallisation, and are further distinguished from the double sulphates containing ammonium by their difficult solubility, are readily prepared by the admixture of solutions of the constituent simple sulphates. The constitution of the salts is probably expressed by the formula,

$$\begin{array}{l} NH_2\text{—}NH_3\text{—}O \\ \qquad\qquad\quad > SO_2 \\ \qquad\qquad O \\ \quad R''< \\ \qquad\qquad O \\ \qquad\qquad\quad > SO_2 \\ NH_2\text{—}NH_3\text{—}O \end{array}$$

Considerable difficulty was found in preparing double chlorides of fixed composition containing hydrazine-chloride. Good crystals of very soluble double chlorides are easily obtained, but so many appear capable of existence that the conditions for the formation of salts of definite constitution have not yet been ascertained. Two entirely new compounds of hydrazine are described by Herr Schrader. By saturating hydrazine hydrate with sulphocyanic acid, or by decomposing hydrazine sulphate with barium sulphocyanate, hydrazine sulphocyanate, $N_2H_4.HSCN$, is obtained as a deliquescent solid substance which melts at 80°. When this hydrazine sulpho cyanate is heated, or when molecular proportions of hydrazine dichloride $N_2H_4.2HCl$ and ammoniumsulpho cyanate in aqueous solution are heated for four or five hours in a sealed tube to 100°, a second new substance of urea-like constitution

$$\begin{array}{l} CS\langle^{NH_2}_{NH} \\ \qquad\ | \\ CS\langle^{NH}_{NH_2} \end{array}$$

is formed. This second substance, hydrazine sulphocarbamide crystallises well from solution in hot water and melts at 214–215°. Its properties are somewhat remarkable, inasmuch as it appears to possess fairly strong acid properties, its solution yielding very characteristic precipitates with solutions of most metallic salts. The dissertation of Herr Schrader likewise reviews most of the work which has been carried out in the Kiel laboratories in connection with hydrazine, and adds many little details of importance in the practical manipulation of the substance and its compounds.

NOTES from the Marine Biological Station, Plymouth.—Last week's captures include the Actinian *Zoanthus Couchii* and the Archiannelid *Histriobdella Homari*. In the floating fauna *Noctiluca*, *Obelia* medusæ, Cirrhipede *Nauplii* and Caridid larvæ were plentiful; *Sagitta*, *Oikopleura* and the tubicolous larvæ of *Terebella* were fairly numerous. Specimens were also taken of the Leptomedusæ *Willia stellata* and *Eucopium quadratum*, of young stages of *Geryonia*, of *Doliolum Tritonis* and Ascidian larvæ, and of the larvæ of the Gephyrean *Thalassema*. The Nemertine *Amphiporus pulcher* and the Mollusc *Goniodoris castanea* are now breeding.

THE additions to the Zoological Society's Gardens during the past week include a Sooty Mangabey (*Cercocebus fuliginosus*, ♀) from West Africa, presented by Miss Grimston; two Brazilian Cariamas (*Cariama cristata*) from Brazil, presented by Mr. Lindsay C. Scott; a Melodious Jay Thrush (*Leucodioptron canorum*) from China, presented by Mr. B. H. Jones, F.Z.S.; a Wall Lizard (*Lacerta muralis* var. *taliguerta*) from Triest, presented by Mr. A. W. Arrowsmith; a Chilian Teal (*Querquedula creccoides*) from Antarctic America, a Little Tern (*Sterna hirundo*) from Brit. Isles, an Axis Deer (*Cervus axis*, ♂) from India, and a St. Thomas's Conure (*Conurus pertinax*) from West Indies, purchased.

OUR ASTRONOMICAL COLUMN.

MR. TEBBUTT'S OBSERVATORY.—We have received from Mr. Tebbutt the report of the work that has been done during the year 1892 at his observatory. In addition to meridian observations a great many extra-meridian observations were made, among which we may name the following:—Forty-five phases of occultations of stars by the moon, several observations of the phenomena of Jupiter's satellites, twenty good comparisons of the planet Mars, when in conjunction with Iota Aquarii, with that star, by means of the filar micrometer on the 8-inch equatorial, numerous observations of the comets visible in that year, about fifteen double-star measures and the variable star observations, including a few comparisons of η Argus and R. Carinæ with neighbouring stars. The meteorological observations have been regularly taken. Under the heading "Personal Establishment," an idea of the energy and zeal which Mr. Tebbutt shows for this science can be gathered from the fact that all the astronomical, and nearly all the meteorological observations are made by himself. Occasionally his son takes the meteorological readings during his absence from home, while the assistance of a computer is sometimes made use of. Although Mr. Tebbutt has received from several astronomers many suggestions as to work desirable to be done, he, nevertheless, wishes to fulfil the main work of the observatory, which consists in observations of lunar occultations of stars, southern comets, and the meteorological observations. That Mr. Tebbutt is thinking about seeking some relaxation, is only natural when one considers how his powers must have been taxed during the last few years; and we sincerely hope that after a good holiday and rest he may come back to his work again a new man, and continue the work he has so ably begun.

UNIVERSAL TIME IN AUSTRALIA.—With three meridians differing by one hour from one another passing through the continent of Australia, the question has been raised as to whether only central time should be used, or all three times. (*The Observatory* for September). Adopting the latter, it will be necessary, of course, for frequent changes of time to be made; but with the former, although places on the extreme east and west would have their time about 1½ hours away from local time, greater convenience for railways, telegraph work, &c., will be gained. Sir Charles Todd, who supports this latter view, and who is backed by the Hon. J. G. Ward (New Zealand), the Hon. J. Kidd (N.S.W.), and the Hon. A. Wynne (Victoria), came to the following conclusion at the Postal and Telegraph Conference held in Brisbane this year, when the subject of the Hour Zone Time was being considered:—"That it is desirable in the public interests that the Hour Zone system should be adopted in a modified form, so that there should be one time throughout Australia, viz. that of the 135th meridian, or nine hours east of Greenwich."

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 4.—M. Lœwy in the chair.—Report upon a memoir by M. Defforges, entitled, on the distribution of the intensity of gravity at the surface of the globe, by MM. Fizeau, Daubrée, Cornu, Bassot, Tisserand. This memoir, submitted to the judgment of the Academy by the Minister of War, summarises the theoretical and experimental researches made during eight years in the geographical service of the army, with the object of determining the absolute intensity of gravitation for a small number of primary stations, and the relative intensity for a large number of secondary stations with simplified apparatus. The latter were determined by means of the "reversible invertible pendulum" invented by M. Defforges, which exceeds all used previously in lightness and convenience, and easily gives an approximation to within 1 part in 100,000. The anomalies extending along a line from Spitzbergen through the Shetlands, Scotland, England, France, and Algiers considerably exceed any possible experimental errors, and the excess of gravitation on the islands and defect on the continents is well established. The report, which was adopted by the Academy, advises the Government to supply M. Defforges with the means to extend his work to the islands of the southern hemisphere and especially the Pacific.—The hypothesis of sub-continental bells, by M. Rateau. The phenomena of the earth's crust are well explained and connected by assuming that the crust underneath the continents does not touch the fluid globe, but is separated from it by a space filled with gaseous matter under pressure. The continents would thus form a sort of bells, very much flattened, and supported by gas, whereas the ocean beds would lie direct upon the igneous globe. The continental projections tend generally to rise, blown up as it were by the accumulating gas below, whilst the sea-beds sink. But the gases, imprisoned under high pressure, escape gradually through the fissures of the crust, when the production of new quantities from the nucleus will become insufficient, the pressure under the continents will decrease, and these will be projected upon the new crust underneath, giving rise to more or less extended crateriform configurations. This is the state in which we see the moon at the present time. If the earth's crust is assumed to be 30 km. thick, the pressure of the gases should be 650 atmospheres and their temperature 900°. The gases would be of a density nearly equal to that of water, and superposed in the order: hydrogen, methane, nitrogen, ethane, oxygen, carbonic anhydride. Hydrochloric acid and siliciuretted hydrogen would also probably be stable under these conditions. The presence of gas underneath the continents, elevated as they are above the sea and of greater density than water, is necessitated by conditions of hydrostatic equilibrium. It is easily seen why volcanoes in the interior of continents never give off larva, but only gases; also why lines of coast volcanoes have successively receded inland where the sea encroached.—On the elimination of foreign bodies in the *Acephala* and especially in *Pholas*, by M. Henri Coupin. If the mantle and the ventral siphon of a Pholas are cut along their entire length, and a collection of foreign particles are thrown upon the tentacles, the particles falling upon the dorsal tentacles are carried away with great rapidity, not towards the mouth, but upon that part of the mantle which lies between the anterior luminous organ and the palp. Thence they pass quickly towards the siphon region, and are stuck together by mucus and rolled up into balls, which are then extruded at the siphon. It is thus that the animal gets rid of the particles of rock disintegrated during its boring operations, and protects its delicate internal canals.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Index to the Publications of the Anthropological Institute (1843 to 1891): G. W. Bloxam (Anthropological Institute).—The Amphioxus and its Development: Dr. B. Hatschek, translated and edited by J. Tuckey (Sonnenschein).—The Pharmacopœia of the United States of America, 7th Decennial Revision, 1890 (Philadelphia)—London Inter. Science and Prelim. Sci. Directory, No. iv. July 1893 (London).—Accidents de Chaudières: F. Sinigaglia (Paris, Gauthier-Villars).—Théorie des Jeux de Hasard: H. Laurent (Paris, Gauthier-Villars).—Smithsonian Institution, 'Report of National Museum for Year ending June 30, 1891 (Washington)—An Elementary Text-book of Biology: J. R. A. Davis, 2nd edition, 2 parts (Griffin).—Publicazione della Specola Vaticana, fasc. iii. (Rome).—Bulletin of the U.S. Fish Commission, Vol. x. for 1890 (Washington).—Index Kewensis: Sir J. D. Hooker and B. D. Jackson, Part 1 (Oxford, Clarendon Press).—A Contribution to the Geology and Natural History of Nottinghamshire: edited by J. W. Carr (Nottingham, Bell)—Illustrated Hand-book of the Cape and South Africa: edited by J. Noble (Stanford).—Terra: A. A. Anderson, 2nd edition (Reeves and Turner).

PAMPHLETS.—Abstract of Returns furnished to the Department of Science and Art (Eyre and Spottiswoode).—Report of Mr. Tebbutt's Observatory, the Peninsula, Windsor, N.S.W. 1892: J. Tebbutt (Sydney).

SERIALS.—Journal of the Anthropological Institute, August (K. Paul).—Natural Science, September (Macmillan).—Geological Magazine, September (K. Paul).—American Journal of Mathematics, Vol. xv. No. 3 (Baltimore).—Journal of the Asiatic Society of Bengal, Vol. 62, Part 2, No. 1 (Calcutta).—Journal of the Chemical Society, September (Gurney and Jackson).—Proceedings of the American Philosophical Society, Vol. 31, No. 141 (Philadelphia)—Proceedings of the Rochester Academy of Science, Vol. 2, Brochure 2 (Rochester, New York).—Geological and Natural History Survey of Minnesota, Bulletin No. 8 (Minneapolis).—Medical Magazine, September (Southwood).—Proceedings of the Royal Society of Edinburgh, Session 1892-93, Vol. xx. pp. 1 to 96.—Journal of the College of Science, Imperial University, Japan, Vol. 6, Part 2 (Tokyo).

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