THURSDAY, MARCH 6, 1890.

## *THE SCIENCE COLLECTIONS AT SOUTH KENSINGTON.*

IT is satisfactory to learn that the Government has taken the first step towards carrying out the recommendations of the recent Commission on the South Kensington Museum. The Report of the Commissioners was to the effect that the Science Museums contained valuable apparatus which ought to be exhibited; that the buildings in which it is displayed are inadequate; and that the area of the exhibition space ought immediately to be increased by 50 per cent. Between the Natural History Museum in Cromwell Road and the Imperial Institute Road lies the strip of ground on which the new buildings must be erected. It belonged to the Commissioners of the 1851 Exhibition, and they were willing to sell at a price somewhat less than the valuation of the Office of Works, or at ten shillings for every pound of their own estimate.

The question to be decided was, whether the country could afford £100,000 to purchase the land necessary to carry out the Report of one of the strongest Commissions which has ever investigated such a subject, or whether the great group of Museums for which South Kensington is famous was to be cut into two by rows of mansions.

The Government, which certainly did not err through undue haste, felt that a case had been made out, that further delay was useless, that the land ought to be secured before time and labour were spent in discussing the details of the buildings to be erected upon it, and therefore they brought in a supplementary estimate for the sum required.

Then followed a debate of the kind by which the *prestige* of ordinary members of the House of Commons has been reduced to its present level. One member "affirmed that there were empty rooms in South Kensington Museum which might well be used for the display of exhibits," though a body of Commissioners appointed to investigate the state of the collections had reported in a directly opposite sense. Another "could not understand why all these educational collections should be established close to one another at South Kensington." In other words, he could not see that if there is to be at South Kensington a great training school for teachers of science and art, it is desirable that the students should have ready access to the national science and art collections, and that the collections themselves should benefit from the advice of the Professors who are familiar with them. These objections were not, however, raised by men who knew the facts. Approval was expressed from both sides of the House by those who have the interests of education at heart. Sir Lyon Playfair, Sir Henry Roscoe, Mr. Mundella, and Mr. Chamberlain, all spoke in favour of the vote, and Mr. Mundella put clearly what those who are acquainted with the Museum know to be the truth, when he said "this question had been pressing for the last ten years, because for the whole of that period the most valuable national science collections, such as no other country in the world possessed, had been housed in the most disgraceful manner."

The vote was finally carried by 144 to 67, and it is to be hoped, now that the Government have entered upon the path of progress, they will pursue it with determination.

No one would urge precipitancy. Due care ought to be taken that money's value is obtained for money spent; but as the question of principle has been decided after ten years' debate, we have a right to demand that progress shall not be delayed by mere blind obstruction to every proposal which involves outlay, but that those in whose hands the fate of the science collections rests shall make up their minds as to what ought to be done, and shall forthwith do it.

## *THREE RECENT POPULAR WORKS UPON NATURAL HISTORY.*

*Glimpses of Animal Life.* By W. Jones, F.S.A. (London: Elliot Stock, 1889.)

*Toilers in the Sea.* By M. C. Cooke, M.A., LL.D. (London: S.P.C.K., 1889.)

*Les Industries des Animaux.* Par F. Houssay. (Paris: J. B. Baillière et Fils, 1890.)

MR. JONES'S book is a charming little volume of 229 pages, with one illustration forming a frontispiece. There are, in all, seven chapters; dealing, in succession, with "Playfulness of Animals," "Animal Training," "Musical Fishes" (title ill chosen), "Nest-Building and Walking Fishes," "Luminous Animals," "Birds' Nests in Curious Places," and "The Mole." The author has been at immense pains to sift the voluminous literature of his subject (a task which he admits has involved a "somewhat unprofitable course of romance reading"). We find, as might be expected, citations of the old old stories of our youth; the climbing perch, Cowper's hares, and other time-honoured (if perhaps too highly coloured) narratives appear; the luminous centipede is not overlooked; and authorities are appealed to, from Aristotle and the ancient classical writers of the past, down to Lubbock and Romanes ("the Rev. Dr. Romanes" [*sic*], p. 25) of to-day. The work is essentially a compilation; it consists mainly of a collection of lengthy extracts, and the author has left himself little room for originality. There results from this an occasional heaviness of style, which is especially noteworthy in the earlier portions of the volume. Paragraphs too frequently lead off with "Broderip mentions," "Evelyn records," "Humboldt saw," and the like; and not even stories of the gambols between a rhinoceros and an elephant, or of those of a 60-foot whale, serve to relieve the monotony. It is doubtful whether the author has not occasionally erred in the placing of his anecdotes. To take a leading instance; on p. 32 there is recorded the story of a parrot, "which, when a person said to it, 'Laugh, Poll; laugh!' laughed accordingly, and the instant after screamed out, 'What a fool to make me laugh!'" This narrative cannot be said to betray any sense of playfulness on the part of the bird, as would be inferred from its position in the text; it surely should have found a place under "Animal Training." The most serious defect in the book is the absence of an index. The author has brought together a very remarkable series of anecdotes; and if he would give us an exhaustive index, together with a complete bibliography, his book would befit the more special and advanced student of animal life. Without these it can only appeal to the *dilettanti;* and we shall look for them in a future edition. We would point out, at the same time, that the climbing perch is referred to on p. 151 as *Perca*, and on 157 as *Anabas* (the latter being correct); that "Willmoes" (p. 185) should read Willemoes Suhm; and that Mr. Romanes does not lay claim to the distinction accorded him on p. 25 (*cf. supra*). The author, as he enters into details not usually met with in books of this kind, might advantageously incorporate with his account of the stickleback's nest, the discovery of Möbius and Prince that the thread employed in weaving it is secreted by the animal's kidney. So unique a fact in natural history should not be allowed to pass unnoticed; and that portion of the work which deals with the luminous fishes might well be brought more completely up to date.

Dr. Cooke's treatise is one of 369 pages, with 4 lithographic plates, 70 woodcuts, and an index. It deals with marine invertebrata, in their especial relations to skeleton formation; and the volume is especially designed to make good the shortcomings of the Rev. J. G. Wood's work, entitled "Homes without Hands." The book has its good points; the chapter on "Coral Reefs and Islands," and the "Introduction," are fairly well done. The last-named deals with generalities as affecting life and the conditions of life in the ocean depths; it gives a record of important explorations, from that of Ross in Baffin's Bay, to the *Challenger;* the Bathybius controversy is abstracted, and alternative theories of reef-formation are summarized, both being presented in concise and impartial language. On perusal, however, of the main portion of the book, we meet with a preponderance of antiquated, and often erroneous information. Lengthy citations from the writings of authorities of the last two or three decades are flaunted as if expressive of current knowledge and opinion. The question of sponge affinities is discussed as though settled by Clark and Kent; that of the significance of the yellow bodies of the Radiolarians as though set at rest by the misconceptions of Wallich. We are told that there is no proof that the Millepore is a Hydroid, and so on. Upon the ill-effects which must result from this method of procedure it is needless to enlarge; but in justice to the author it must be admitted that he has made some use of recent literature. He appeals to the *Challenger* volumes. His quotations from these are, however, very capricious, and in some instances inaccurate. It cannot be said that the spines of the Radiolaria are "never tubular," for Haeckel (whose Report the author quotes) has given their tubular character as a diagnosis of his *Phæodaria*. Writing of "sensation in the Radiolaria," the author indulges (p. 103) in a remarkable paragraph, which concludes as follows:—

> "Prof. Haeckel considers that the central capsule contains the common central vital principle, which he terms the 'cell-soul,' and that it may be regarded as a simple ganglion cell, comparable to the nervous centre of the higher animals, whilst the pseudopodia are analogous to a peripheral nervous system."

These are not the words of the author cited, and, even if they were, the introduction of such silly stuff into the pages of a book intended for "the large and increasing section of the nature-loving public who indulge in the use of the microscope as a source of instruction and amusement" (p. 3) is intolerable. It is a remarkable fact that, while the author has reproduced the more commonplace statements of the earlier writers in their original form, he should have chosen to give us the above, his own, rendering of the lucubrations of a Haeckel. In doing this he betrays a sad want of sound judgment. The public have a right to expect that a work of this type, intended to serve (p. 3) "as a preliminary to more specific knowledge, the direction of which they will thereafter be better able to choose," shall be up to date; but, to fulfil the useful purpose aimed at, such a work should rest upon a more authoritative foundation than the book now under review. That is amusing as an example of editorial piece-work among a somewhat antiquated literature, and to those familiar with the subjects approached it suggests reflections.

The volume by M. Houssay is one of 312 pages, with 47 woodcuts intercalated in the text (38 only are acknowledged on the title-page). The bulk of the work is divided into six chapters, dealing respectively with modes of capture of prey, of defence, of transport and storage of food, of provision for the young; of constructing or acquiring nests and habitations, and of preservation and protection of the same. The illustrations are, for the most part, admirable; some, which we take to be original, are fit to rank with the famous woodcuts in Brehm's "Thier-Leben," while others are already familiar to us from the pages of that work. In the introduction the author justly asserts that the naturalist of to-day lives more in the laboratory than in the field, that the scalpel and microtome have replaced the pins of the collector, and that the magnifier pales beside the microscope. This is, alas! too true. It cannot be denied that our present systems for the most part take insufficient heed of field-work, and we fully endorse the author's further remarks upon the changed aspect of affairs. The introduction as a whole deals with generalities in direct bearing upon those facts which follow; and by no means its least satisfactory feature is that it clearly sets forth what the author would have his readers understand by the title of his work. The main portion of the book is confined to bare records of observed fact, systematically arranged, and, where necessary, brought into special relationship by cross-references. That "talkee-talkee" so often forced into books of this kind is here withheld. Such comments as are indulged in are either confined to the introduction, or to a few concise paragraphs which make up the author's "conclusion"; and the latter is, as might be expected, devoted to a brief consideration of animal intelligence. In place of an index there is furnished a zoological table, in which the generic names of the animals written about are arranged in classificatory order, each being accompanied by a paged reference and a mention of that particular habit or industry dwelt upon. It is a pity that the author takes no cognizance of animals lower in the scale than the Arthropods; but we nevertheless heartily recommend his book to our readers. It is throughout popular, and written in that peculiarly pleasing, yet didactic, style, so characteristic of the works of the more successful of

French popularizers of science, which has made them masters of their art.

The above-named volumes are three of a number of similar treatises which have lately appeared. The appreciation of the beautiful and generally interesting in Nature must always precede the study of the more useful and special, and it is the highest function of works like the present to awaken this preparatory appreciation. Of such works those are the most valuable whose authors can claim a sound elementary knowledge of the facts with which they deal, and a familiarity with current research. Only on these terms can a popular natural history rise above the level of the too well-known type, in which the scissors supply the knowledge and the paste usurps the place of the co-ordinating intellect. G. B. H.

## *A GENERAL FORMULA FOR THE FLOW OF WATER.*

*A General Formula for the Uniform Flow of Water in Rivers and other Channels.* By E. Ganguillet and W. R. Kutter. Translated from the German by Rudolph Hering and John C. Trautwine, Jun. (London: Macmillan and Co., 1889.)

THE general formula devised by Messrs. Ganguillet and Kutter for caculating the flow of water in both large and small channels, under varied conditions, was brought under the notice of English-speaking engineers by the publication, in 1876, of a translation by Mr. Jackson of some articles on the subject written by Mr. Kutter, which appeared in the *Journal der Cultur-Ingenieur* in 1870. This translation, however, was not authorized by Mr. Kutter, and contained some incomplete tables inserted by Mr. Kutter in his articles at the request of a friend. The present volume is a translation of the second edition of the treatise on the formula, written by Messrs. Ganguillet and Kutter, engineers in Berne, who have added a preface to the translation. Mr. Kutter died whilst this translation was in progress; and a short memoir of him, with a list of his works, is appended to the translators' preface.

The book commences with an historical sketch of the attempts to arrive at a formula for the flow of water in open channels; and the insufficiency of the earlier formulæ is pointed out. The investigations of Messrs. Darcy and Bazin, and the gaugings of the Mississippi by Messrs. Humphreys and Abbot, are then concisely described, and the formulæ which they deduced from the results of their experiments are given, the history of the subject, in a brief form, being thus brought down to the period at which Messrs. Ganguillet and Kutter commenced their investigations. This forms a sort of introduction to the account of the conception and development of the general formula, of which the various steps are described in detail. The modifications for various amounts of roughness are classified; and, finally, the formula is tested by the comparison of its results with a number of gaugings under very different conditions; and these results indicate, in considerably the greater number of cases, a closer approximation to the actual measurements than those obtained with the formulæ of either Humphreys and Abbot, or Bazin. A supplement gives a more direct derivation of the formula for mathematical readers; and the appendices contain numerous tables giving the flow of water in pipes under pressure, as well as in open channels, for practical use in English measures, derived from the formula, and also a diagram for the graphical determination of the values of the factors in the formula, adapted to English measures by the translators.

Most of the hydraulicians who had investigated the question before Darcy and Bazin, such as De Prony, Dubnat, Eytelwein, D'Aubuisson, Downing, and others, agreed in adopting a formula of the form $V = c\sqrt{RS}$, of which Brahms and Chezy are said to have been the authors in the latter half of the last century, in which V is the velocity, R the hydraulic radius, and S the slope. Different values were assigned to the factor $c$ by the various investigators; but it was always regarded as a constant, applicable to any sized stream in most cases, to any slope, and to any state of the bed. Mr. Darcy was the first who directed attention to the influence the condition of the sides of channels and pipes exercised on the discharge; and he instituted a series of experiments, carried out after his death by Mr. Bazin, by which the flow of water in regular uniform channels, under different conditions of slope, form, and roughness of bed, was measured by careful gaugings and gauge-tubes. A few years previously, Messrs. Humphreys and Abbot had carried out their well-known gaugings of the flow of the Mississippi by means of double floats, and deduced a formula for the results obtained. Messrs. Ganguillet and Kutter found that the formula derived from the Mississippi experiments, relating to a large river with a very slight slope, was not applicable to the small streams with steep slopes of which they measured the flow in Switzerland, and also that Mr. Bazin's formula was not suitable, in its original form, for large rivers with irregular beds. This led Messrs. Ganguillet and Kutter to search for a formula applicable to very different slopes and sizes of channel, and adaptable to various conditions of bed. They took as the basis of their formula the various experimental results obtained in France and America, together with their own independent observations on channels with steep slopes, so as to include the extreme varieties of flow within the range of a single formula. Starting from Mr. Bazin's formula, $V = \sqrt{\dfrac{RS}{\alpha + \dfrac{\beta}{R}}}$, where $c = \sqrt{\dfrac{1}{\alpha + \dfrac{\beta}{R}}}$, they eventually found it expedient to express the value of $c$ in the form $\dfrac{y}{1 + \dfrac{x}{\sqrt{R}}}$, in which, though they at first assumed $y$ and $x$ to be constant for any given state of bed, they finally modified them to expressions varying with the slope. The alterations in the formula were effected by aid of graphical representations of the various sets of gaugings. It was found, in investigating the various experimental results, that the factor $c$ varied generally with the slope; but a somewhat anomalous result was also noted—namely, that whereas in the Mississippi observations $c$ increased with a decrease in the slope, it on the contrary decreased with a decrease of slope in the gaugings of small channels, unless the wetted perimeter was very rough. This change in the variation of $c$ with relation to the slope was found to depend upon the hydraulic radius being greater or less than 3·281 feet; so that $c$ becomes independent of the change in slope when R approximates to this value, though the actual value of R at which the modification occurs varies with the degree of roughness of the channel. This result is attributed to the conflicting currents and eddies in large rivers having irregular beds, or in small channels with very rough beds, which are intensified by an increase in the slope; whereas, in small streams flowing in confined channels with smooth beds, an increased velocity tends to dissipate retarding lateral movements. A preliminary form adopted for the value of $c$ was $\frac{a + \frac{l}{n}}{1 + \frac{an}{\sqrt{R}}}$, where $a + \frac{l}{n}$ replaces $y$ in the original formula, and $an = x$, or $x = ny - l$, in which $a$ is a constant with value 41·66 in English measures; $l$ is another constant, equal to $\sqrt{R}$ when R has the special value 3·281 referred to above, and therefore 1·811; and $n$ is the coefficient of roughness, varying, according to the state of the channel, from 0·009 to 0·040. The above value of $c$ suffices for the flow in pipes and other small channels with steep slopes, owing to the small influence of a variation of slope on the coefficient $c$ in such cases; but for ordinary channels allowance has to be made for variations in slope, necessitating the introduction of another variable factor into the expression for $c$. The final shape given to the value of $c$ by Messrs. Ganguillet and Kutter, in their general formula, was $\frac{a + \frac{l}{n} + \frac{m}{S}}{1 + \left(a + \frac{m}{S}\right)\frac{n}{\sqrt{R}}}$, where $m = 0·0028075$, for English measures, is a constant of a hyperbola employed in constructing the formula. The general formula, accordingly, became, for English measures—

$$V = \frac{\frac{1·811}{n} + 41·6 + \frac{0·00281}{S}}{1 + \left(41·6 + \frac{0·00281}{S}\right)\frac{n}{\sqrt{R}}}\sqrt{RS},$$

where V is the mean velocity in feet per second, which multiplied by the cross-section would give the discharge in cubic feet per second, and S is the actual slope.

The main interest of the book consists in the clear exposition of the several steps by which the formula was reached; and even if at some future time, by the aid of fresh observations and more accurate experiments, the formula should be superseded by a more comprehensive and exact one, the merit of this work as an elaborate scientific investigation for a general empirical formula must always remain; and the book would deserve to be consulted on this ground alone. The formula depends entirely upon the exactness of the observations upon which it has been based. Mr. Révy has questioned the accuracy of the Mississippi experiments, owing to the use of double floats; and if fresh investigations should establish the inaccuracy of any of the observations made use of, or if further experiments should extend the scope of the inquiry, or bring new facts to light, a modified formula will be required. The authors, however, of the formula do not regard it as final or complete, nor do they claim for it any mathematical precision; they only consider that it agrees more closely than any previous formula with the results of recorded observations. The formula has naturally been objected to on account of its complicated appearance; but the variation due to change of slope renders this inevitable; and it has been seen that a simpler formula may be adopted for pipes, and small channels with steep slopes; and, moreover, graphical methods and tables might simplify the calculations. At the close of last year, Mr. Robert Manning, Engineer to the Board of Works in Dublin, presented a new formula to the Institution of Civil Engineers of Ireland, which, in its general form, is hardly less complicated than that of Messrs. Ganguillet and Kutter. This formula is

$$V = n\sqrt{Sg}\left\{R^{\frac{1}{2}} + \frac{0·22}{m^{\frac{1}{2}}}(R - 0·15m)\right\},$$

where $n$ is the coefficient of roughness, $g$ the force of gravity, and $m$ the height of the barometric column of mercury. Mr. Manning puts it forward as simpler and better than the other, and claims for it, in a simplified form, a closer approximation to the mean of the results of seven of the best known formulæ than any other. Actual observations, however, form a surer basis upon which to establish a general formula than the results of previous formulæ; and it is upon a close concordance with very varied and accurate observations that any general formula must claim acceptance. Whatever position may in the future be assigned to the formula of Messrs. Ganguillet and Kutter, their work marks a notable step in advance, and must rank with the researches of Messrs. Darcy and Bazin, and Messrs. Humphreys and Abbot, as a record of important hydraulic investigations; and the translators have performed a valuable service in placing clearly before English readers the successive steps by which this general formula has been established.

## THE COMPASS ON BOARD.

*Der Kompass an Bord: Ein Handbuch für Führer von eisernen Schiffen.* Herausgegeben von der Direktion der Deutschen Seewarte. (Hamburg: L. Friederichsen and Co., 1889.)

THE important subject of the magnetism of iron ships and the resulting deviations of their compasses, has, during the last fifty years, received marked attention in England from eminent men of science, attended with most valuable results for the safe navigation of our Royal and mercantile navies.

During the last thirteen years this same subject has been one of continuous inquiry at the German Naval Observatory in Hamburg, and papers have been published from time to time in the annual report of that institution, showing what had been accomplished. Combining the results of this work with those obtained from the extensive literature chiefly produced in England, Dr. Neumayer, the Director of the Observatory, has compiled the present work for the use of officers commanding the iron ships of the German mercantile navy.

Of the six chapters into which the work is divided, the first is devoted to information on the magnetism of iron and steel, terrestrial magnetism, and the means of obtaining the three magnetic elements.

In the second chapter, the various modern forms of the mariner's compass, and instruments for adjusting compasses without sights, are described with illustrations. There is much here which should be of value to commanders of ships anxious to know as much as possible of their best friend in navigation.

It is, however, to be regretted that in some particulars both text and illustrations belong to the past, for in Fig. 38 an imperfect idea is given of Sir W. Thomson's compass. The drawing was correct for 1877, but important improvements were made ten years ago in the substitution of the wire grummet suspension for india-rubber, a change attended with marked success in vessels propelled and severely shaken by powerful engines; also, in 1881, the adoption of a total reflection prism in the azimuth mirror instead of an ordinary piece of looking-glass.

Prominence is given to the Hechelmann compass card, which is intended to combine the principles of the Thomson card (which consist chiefly of a long period of oscillation and great lightness), with a much greater magnetic moment in the Thomson-Hechelmann card, as it may be termed. The chief difference in these cards lies in the arrangement of the needles, Hechelmann's idea being to suspend more powerful needles than Thomson's near the circumference, thus bringing the weight as far as possible from the centre of the card to produce a slow period.

In bringing powerful needles so near the circumference, it is easy to see that something has been lost by Hechelmann when the quadrantal deviation is to be corrected as it should be—a correction so perfectly accomplished by Thomson. The greater weight of the card, too, tends to increase friction at the cap and pivot. Under these considerations the Thomson-Hechelmann card can hardly be considered equal to the modern Thomson.

In the next chapter, which treats of the magnetism of ships and the resulting deviation, it is satisfactory to find that the different kinds of magnetism which careful investigation has shown to exist in modern vessels are specially mentioned. These are—

(1) Permanent magnetism.
(2) Sub-permanent (termed also retentive) magnetism.
(3) Transient magnetism.

These definitions are accompanied by a footnote stating that in the English text-books on deviation no difference is made between permanent and sub-permanent magnetism, but that the two are combined under the expression sub-permanent. This is perhaps rather hard upon some English books, where, by careful reading, it will be found that the distinction is really made, but, it must be confessed, with a want of that clearness of division which is important to sound knowledge. Readers of the papers published by the Royal Society, and more recently by the Royal United Service Institution, will find that the division of a ship's magnetism into the three kinds mentioned above is strongly insisted upon.

A complete analysis of the deviations of any given compass in a ship, and of the changes which take place on a change of latitude, is necessary before a satisfactory compensation of the deviation by magnets and soft iron can be made. In the "Compass on Board," this analysis has a chapter devoted to it, containing information which should be of value both to the captains of ships and compass adjusters. It is illustrated by many examples.

Values of the coefficients $v$ and $v'$, representing the temporary deviation caused by running on a given course for some days, are given for a number of vessels of different types, steam and sailing. They clearly show the navigator of a new ship the need of caution when altering course, and some idea of the amount of change of deviation he may expect; whilst it should be understood that no careful seaman would fail to learn and note the peculiarities of the iron affecting his ship's compasses from personal observation under the varied circumstances experienced during each voyage.

A corrector for the deviation caused by sub-permanent magnetism has yet to be discovered.

Taking a general view of this book, it may be described as calculated to provide good practical information for the officers of the German mercantile navy, as well as a certain amount of a theoretical nature for those inclined to learn something of a ship's magnetism from a higher standpoint.

The maps of the three magnetic elements provided at the end of the book are given for the epoch 1885, and on a larger scale than those usually provided in hand-books. The accompanying map of values of the secular change is somewhat open to criticism as regards the figures recorded in the Red Sea, Bombay, East Indies, and Australia. This, however, will not prove of any detriment to safety in practical navigation.

The difficulties connected with the compass in warships, with their armoured deck, thickly-plated sides, and conning-towers, are not treated of, and their officers must look elsewhere for the special information they require; still, there is much to be found in this book that will serve their purpose.

## *OUR BOOK SHELF.*

*Library Reference Atlas of the World.* By John Bartholomew, F.R.G.S. (London: Macmillan and Co., 1890.)

THE recognition of the intimate connection that exists between physiography and geography is made very manifest, in all the atlases published during the last few years, by the insertion of maps indicating the physical features of the earth's surface.

We are in an eminently utilitarian age, and a collection of maps, to meet the requirements of the day, must serve more purposes than that of a mere index to the positions of places; it must represent the most permanent features of importance in commercial geography, and the distribution of commodities as explained by the sciences of physics, geology, meteorology, biology, &c., or collectively by physiography. The elegant work before us satisfies all these requirements, it is as complete as it is a trustworthy atlas of modern geography, and will be equally appreciated by the student, the business man, and the general reader.

The atlas contains 84 maps, and amongst them we find plates delineating drainage areas, ocean currents, prevailing winds, rainfall, temperature, climate, and commercial features. A characteristic of the collection is the large number of maps that have been devoted to the British Empire, eighteen plates being given of the United Kingdom alone. India is completed in eight plates, the Dominion of Canada is very completely represented in seven plates, and the mapping of all the British possessions has been carried out on the same elaborate scale. After the British Empire, special prominence has been given to the United States, whilst all the other countries of the world have been treated in a very comprehensive manner. The general reference index comprises the names of 100,000 places contained in the maps, and for British names it is the most complete ever published. One matter of regret, however, is that the places on some of the maps are not obviously visible because of the bright and superabundant colouring used to indicate the divisions of a country, for, generally speaking, these divisions are better represented by coloured lines. The less masking there is, the more distinct must places appear, and therefore the purpose of an atlas will be the better served. This is, however, but a minor point. The atlas is an excellent one, it is complete and accurate, contains all the results of recent exploration and geographical research, and is issued at a moderate price; its addition to every library therefore is a thing to be desired.

*The Bala Volcanic Series of Caernarvonshire and Associated Rocks; being the Sedgwick Prize Essay for* 1888. By Alfred Harker, M.A., F.G.S., Fellow of St. John's College, and Demonstrator in Geology (Petrology) in the University of Cambridge. (Cambridge: University Press, 1889.)

IN this useful little work, Mr. Harker has given an admirable *résumé* of the results which have, up to the present time, been arrived at by the study of the ancient igneous rocks of North Wales. Besides summarizing the work of the late John Arthur Phillips and E. B. Tawney, of Prof. Bonney, Mr. Rutley, Mr. Cole, Mr. Teall, Mr. Waller, Miss Raisin, and others who have written on the petrography of the district, he has added many new and often judicious notes on the rocks in question. A number of fresh analyses, and the description of hitherto unrecognized varieties of rocks and minerals, raise the work out of the category of mere compilations; and the excellent classification and arrangement of his materials make the book one eminently useful for purposes of reference. It is unfortunate that it has no index, though the "table of contents," which is very full and carefully paged, causes the want to be less felt than it otherwise would be. Mr. Harker classifies the districts of Caernarvonshire in which volcanic rocks are found as the *Eastern*, *North-Western*, and *Western*, the latter consisting of the Lleyn peninsula. He groups the types of rocks represented under the headings of "rhyolitic lavas," "nodular rhyolites," "acid intrusives," "intermediate rocks," "diabase sills and basalts," and "other basic intrusions." The work concludes with a "review of vulcanicity in Caernarvonshire," in which we find discussions of the relation of the volcanic eruptions to the earth-movements that took place at the period of their occurrence, the succession of lavas in the district, and the evidence in favour of their submarine origin. The book is admirably printed, and is illustrated by six very clearly-drawn sketch-maps. The essay is worthy of the memorial in connection with which it appears, and is creditable to the University under whose auspices it is issued; and higher praise than this it would be difficult to give to any work of the kind.

## LETTERS TO THE EDITOR.

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

### The Inheritance of Acquired Characters.

WITHOUT expressing any opinion upon the question recently discussed in your columns under the above title, I think it may be as well to recall the belief of one whose judgment was not without weight, and to give some of the evidence on which that belief was founded.

In the first chapter of the "Origin of Species" (p. 8 of the sixth edition), Mr. Darwin says, respecting the inherited effects of habit, that "with animals the increased use or disuse of parts has had a more marked influence"; and he gives as instances the changed relative weights of the wing-bones and leg-bones of the wild duck and the domestic duck, and, again, the drooping ears of various domestic animals. Here are other passages taken from subsequent parts of the work:—

"I think there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited" (p. 108). And on the following pages he gives five further examples of such effects. "Habit in producing constitutional peculiarities, and use in strengthening and disuse in weakening and diminishing organs, appear in many cases to have been potent in their effects" (p 131). "When discussing special cases, Mr. Mivart passes over the effects of the increased use and disuse of parts, which I have always maintained to be highly important, and have treated in my 'Variation under Domestication' at greater length than, as I believe, any other writer" (p. 176). "Disuse, on the other hand, will account for the less developed condition of the whole inferior half of the body, including the lateral fins" (p. 188). "I may give another instance of a structure which apparently owes its origin exclusively to use or habit" (p. 188). "It appears probable that disuse has been the main agent in rendering organs rudimentary" (pp. 400-401). "On the whole, we may conclude that habit, or use and disuse, have, in some cases, played a considerable part in the modification of the constitution and structure; but that the effects have often been largely combined with, and sometimes overmastered by, the natural selection of innate variations" (p. 114).

In his subsequent work, "The Variation of Animals and Plants under Domestication," he writes:—

"The want of exercise has apparently modified the proportional length of the limbs in comparison with the body" [in rabbits] (p 116). "We thus see that the most important and complicated organ [the brain] in the whole organization is subject to the law of decrease in size from disuse" (p. 129). He remarks that in birds of the oceanic islands "not persecuted by any enemies, the reduction of their wings has probably been caused by gradual disuse." After comparing one of these, the water-hen of Tristan D'Acunha, with the European water-hen, and showing that all the bones concerned in flight are smaller, he adds:—"Hence in the skeleton of this natural species nearly the same changes have occurred, only carried a little further, as with our domestic ducks, and in this latter case I presume no one will dispute that they have resulted from the lessened use of the wings and the increased use of the legs" (pp. 286-87). "As with other long-domesticated animals, the instincts of the silk-moth have suffered. The caterpillars, when placed on a mulberry tree, often commit the strange mistake of devouring the base of the leaf on which they are feeding, and consequently fall down; but they are capable, according to M. Robinet, of again crawling up the trunk. Even this capacity sometimes fails, for M. Martins placed some caterpillars on a tree, and those which fell were not able to remount and perished of hunger; they were even incapable of passing from leaf to leaf" (p. 304).

Here are some instances of like meaning from vol. ii.:—

"In many cases there is reason to believe that the lessened use of various organs has affected the corresponding parts in the offspring. But there is no good evidence that this ever follows in the course of a single generation. . . . Our domestic fowls, ducks, and geese have almost lost, not only in the individual but in the race, their power of flight; for we do not see a chicken, when frightened, take flight like a young pheasant. . . . With domestic pigeons, the length of sternum, the prominence of its crest, the length of the scapulæ and furcula, the length of the wings as measured from tip to tip of the radius, are all reduced relatively to the same parts in the wild pigeon." After detailing kindred diminutions in fowls and ducks, Mr. Darwin adds, "The decreased weight and size of the bones, in the foregoing cases, is probably the indirect result of the reaction of the weakened muscles on the bones" (pp. 297-98). "Nathusius has shown that, with the improved races of the pig, the shortened legs and snout, the form of the articular condyles of the occiput, and the position of the jaws with the upper canine teeth projecting in a most anomalous manner in front of the lower canines, may be attributed to these parts not having been fully exercised.

. . . These modifications of structure, which are all strictly inherited, characterize several improved breeds, so that they cannot have been derived from any single domestic or wild stock. With respect to cattle, Prof. Tanner has remarked that the lungs and liver in the improved breeds 'are found to be considerably reduced in size when compared with those possessed by animals having perfect liberty.' . . . The cause of the reduced lungs in highly-bred animals which take little exercise is obvious" (pp. 299–300). And on pp. 301, 302, and 303, he gives facts showing the effects of use and disuse in changing, among domestic animals, the characters of the ears, the lengths of the intestines, and, in various ways, the natures of the instincts.

Clearly the first thing to be done by those who deny the inheritance of acquired characters is to show that the evidence Mr. Darwin has furnished by these numerous instances is all worthless. HERBERT SPENCER.

———

LET me remind the readers of NATURE that the discussion which has been going on in these columns, between the Duke of Argyll and Mr. Thiselton Dyer, arose out of a reference in Mr. Wallace's book on "Darwinism" to the dislocation of the eyes of flat-fishes. Two views have been expressed as to the origin of this arrangement—the one endeavouring to explain it as a case in which a "sport" or congenital variation, had been selected and intensified; the other attributing it to the direct action of the muscles of ancestral flat-fishes which had pulled the eye out of its normal position, the dislocation thus established being transmitted to offspring, and its amount increased by like action in each succeeding generation. In common with Mr. Wallace and other naturalists, I spoke of this latter hypothesis as one of transmission of an "acquired character." The term "acquired character" was clearly enough defined by this example; it has been used in England for some years, and its equivalent in German (*erworbene Eigenschaften*) has been defined and used for the purpose of indicating the changes in a parent referred to by Lamarck in the following words ("Philosophie Zoologique," tome i. p. 235, édition Savy, 1873):—

"*Première Loi.*—Dans tout animal qui n'a point depassé le terme de ses developpements, l'emploi plus fréquent et soutenu d'un organe quelconque, fortifie peu à peu cet organe, le developpe, l'agrandit, et lui donne une puissance proportionnée à la durée de cet emploi; tandis que le défaut constant d'usage de tel organe, l'affaiblit insensiblement, le détériore, diminue progressivement ses facultés, et finit par le faire disparaître.

"*Deuxième Loi.*—Tout ce que la nature a fait *acquérir* ou perdre aux individus par l'influence des circonstances où leur race se trouve depuis longtemps exposée, et par conséquent par l'influence de l'emploi prédominant de tel organe, ou par celle d'un défaut constant d'usage de telle partie, elle le conserve par la génération aux nouveaux individus qui en proviennent, pourvu que les *changements acquis* soient communs aux deux sexes ou à ceux qui ont produit ces nouveaux individus."

The meaning of the term "acquired characters" is accordingly perfectly familiar to all those who have any qualification for discussing the subject at all. It is used by Lamarck, and has been used since as Lamarck used it. Naturalists are at present interested in the attempt to decide whether Lamarck was justified in his statement that acquired changes are transmitted from the parents so changed to their offspring. Many of us hold that he was not; since, however plausible his laws above quoted may appear, it has not been possible to bring forward a single case in which the acquisition of a character as described by Lamarck and its subsequent transmission to offspring have been conclusively observed. We consider that, until such cases can be produced, it is not legitimate to assume the truth of Lamarck's second law. We admit, of course, the operation of the environment and of use and disuse as productive of "acquired characters"; but we do not find any evidence that these particular characters so acquired are transmitted to offspring. Accordingly it has been held by several naturalists recently (whom I will call the anti-Lamarckians, and among whom I include myself) that it is necessary to eliminate from Mr. Darwin's teachings that small amount of doctrine which is based on the admission of the validity of Lamarck's second law. As everyone knows, Mr. Darwin's own theory of the natural selection of congenital variations in the struggle for existence is entirely distinct from Lamarck's theory, and the latter was only admitted by Darwin as being possibly or probably true in regard to some cases, and of minor importance. Although Darwin expressly states that he was more inclined to attach importance to Lamarck's theory in the later editions of the "Origin of Species," the anti-Lamarckians are convinced that it is conducive to the progress of knowledge to reject that theory altogether until (if ever) it is placed on a solid basis of observed fact; and in the meantime to try if it is possible to explain the cases which seem most favourable to Lamarck's view by the application of Darwin's own theory.

It is essential for those who are not thoroughly familiar with Darwin's writings to note that this does not involve a rejection of the conclusion that the action of external conditions upon a parent may be such as to modify the offspring. That is an important part of Mr. Darwin's own theory, and, as I recently pointed out in NATURE, it is to such action of the environment upon the parent that Mr. Darwin attributed the origin of those congenital variations upon which natural selection acts. This disturbance of the parental body (I compared it to the shaking up of a kaleidoscope), and with it of the germs which it carries, resulting in "sporting" or "variation" in the offspring, is, it should hardly be needful to state, a totally different thing to the definite acquirement of a structural character by a parent as the result of the action upon it of the environment, and the transmission to offspring of that particular acquired structural character. I am not concerned to inquire here whether, or how far, Prof. Weismann's theory of the continuity of the germ-plasm admits of the action of external forces on a parental body in such a way as to disturb the germ-plasm and induce variation. Prof. Weismann can very well defend his own views. All that I am concerned with—and that quite independently of the conclusions of Prof. Weismann—is whether it is or is not reasonable, useful, or indeed legitimate, to assume the truth of Lamarck's second law, in the absence of any direct proof that any such transmission as it postulates takes place. Those who think Lamarck's second law to be true have been urged to state (1) cases in which the transmission of acquired characters is directly demonstrated, or (2) cases in which it seems impossible to explain a given structure except on the assumption of the truth of that law. If they fail to do this, they are asked to admit that Lamarck's second law is unproven and unnecessary.

The response which has been made to this attempt to arrive at facts is beside the mark. Mr. Cope writes to NATURE merely *asserting*, "If whatever is acquired by one generation were not transmitted to the next, no progress in the evolution of a character could possibly occur,"—an opinion peculiar to himself, and certainly one which cannot be taken in place of fact. The Duke of Argyll then "interpolates" (to use his own word) a general statement of his beliefs, and in the last of his letters a statement of "what his position is." We really are not concerned in this matter with beliefs or positions. We want well-ascertained facts and straightforward reasoning from facts. The Duke of Argyll has not assisted us. When on a recent occasion he was asked to cite an instance of what he called "a prophetic germ" in the adult structure of a plant or animal having, in his opinion, such claims to this title as he had ascribed to the electric organ of skates, the Duke was unable to reply. He wrote as a substitute something about embryological phenomena, which had nothing to do with the case. He has not yet ventured to stake his oft-asserted right to offer an opinion upon zoological topics, on the reception which his attempt to deal with the details of a particular case of organic structure would obtain: in this, I think, he is wise.

The Duke similarly tries to evade the appeal to facts when he is pressed by Mr. Dyer to state cases of the transmission of acquired characters. In doing so, however, he has, it must be admitted, revealed an astonishing levity. He answers (par. 9 of his letter) that in all domesticated animals, and especially in dogs, we have constant proof that many acquired characters may become congenital. This is mere assertion; we require details. It is maintained, on the contrary, by anti-Lamarckians that the whole history of artificial selection, and of our domesticated animals, furnishes a mass of evidence against the theory of the transmission of acquired characters, since if such cases occurred they would be on record, and moreover would have been utilized by breeders.

The subsequent proceeding of the Duke is almost incredible. In the following paragraphs of his letter he gives up his contention that acquired characters are transmitted, coupling his retreat with unwarrantable charges against those who have lately raised the question as to whether this is the case or not. He correctly states what is meant by the term "acquired characters," and declares that this meaning has been expressly invented for the purposes of the present discussion by "fortuitists," and is "irrational." A more baseless charge was never yet made in controversy, nor a more obvious attempt to alter the terms of discussion so as to give some appearance of plausibility to a lost cause. The Duke, in fact, now at length tells us that *he* does not mean by "acquired characters" what *we* mean. Why then did he "interpolate" his remarks on the subject and make use of the term ?

If the meaning which the phrase has for the scientific world generally be insisted upon, we are now, it appears, to understand that the Duke of Argyll agrees with us : what *we* mean by "acquired characters" are not, he admits, shown to be transmitted.

"Fortuitists," the Duke says, "have invented a new verbal definition of what they mean by 'acquired.'" I have shown at the commencement of this letter that the term "acquired" is used to-day as it was by Lamarck. To the Duke this meaning is "new"—because he has either never read or has forgotten his Lamarck. If this be so, the Duke has been writing very freely about a subject with which his acquaintance is very small. The alternatives are as clear as possible : either the Duke of Argyll knew the significance of the term "acquired characters" as employed by Lamarck, in which case it would have been impossible that he should charge those whom he calls "fortuitists" with having invented a new verbal definition of what they mean by "acquired"; *or* he did not know Lamarck's use of the phrase, and was therefore not qualified to offer an opinion in the discussion, nor to press his "beliefs" and "position" upon public attention.

I have no time and you have no space to devote to a full exposure of the character of other assertions made in the Duke of Argyll's "statement of his position" which are as reckless and demonstrably erroneous as that concerning the meaning of the term "acquired."

Perhaps the most flagrant of these is the assertion that "the theory of Darwin is essentially unphilosophical in so far as it ascribes the phenomena of variation to pure accident or fortuity" (paragraph 4). Of course the Duke cannot be acquainted with the following passage from the "Origin of Species," sixth edition, p. 106 ; but if he has to plead ignorance of the writings not only of Lamarck, but also of Darwin, what is the value of his opinions and beliefs on Lamarckism and Darwinism ? The words of Mr. Darwin referred to are these :—"I have hitherto sometimes spoken as if the variations, so common and multiform with organic beings under domestication, and in a lesser degree with those under nature, were due to chance. This, of course, is a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation."

Whatever meaning the Duke may attach to the word "fortuity," it is mere empty abuse on his part to call the later Darwinians "fortuitists," and still less justifiable to insinuate that their investigations and conclusions are not guided by a simple desire to arrive at truth, but by the intention of propping up a worship of Fortuity. It is natural for the Duke to suppose it impossible to write on Darwinism without some kind of theological bias.

In conclusion, I venture to point out that the Duke of Argyll has (1) failed to cite facts in support of his assertions of belief in "prophetic germs," and "transmission of acquired characters" when challenged to do so ; (2) that he displays ignorance of two of the most important passages in the works of Lamarck and of Darwin, whom he nevertheless criticizes, and in consequence of his ignorance completely, though unintentionally, misrepresents ; and (3) that he has introduced into these columns a method of treating the opinions of scientific men, viz. by insinuation of motive and by rhetorical abuse, which, though possibly congenial to a politician, are highly objectionable in the arena of scientific discussion.

February 22. E. RAY LANKESTER.

## Physical Properties of Water.

As you inform me that my anonymous critic (*ante*, p. 361) does not intend to avail himself of the opportunity I gave him (through you) of correcting his misstatements about my *Challenger* Report, I must ask to be permitted to correct them myself.

(1) There is nothing whatever in my Report to justify the critic's statement that I "*had never heard of* Van der Waals' work . . . till the end of the year 1888." Yet this is made the basis of an elaborate attack on me !

What I did say was to the effect that I was not aware, till Dr. Du Bois told me, that Van der Waals had given numerical estimates of the value of Laplace's K. I had long known, from the papers of Clerk-Maxwell and Clausius, the main features of Van der Waals' investigation. But I also knew that Maxwell had shown it to be theoretically unsound ; and that Clausius had taken the liberty of treating its chief formula as a mere empirical expression, by modifying its terms so as to make it better fit Andrews' data. This paper of Clausius is apparently unknown to my critic, as is also my own attempt to establish (on defensible grounds) a formula somewhat similar to that of Van der Waals.

(2) I said nothing whatever about the "Volume of Matter in unit volume of Water." Hence the critic's statement, "Prof. Tait's value is 0·717," is simply without foundation.

I merely said that the empirical formula

$$p(v - \alpha) = \text{constant},$$

if assumed to hold for all pressures, shows that $\alpha$ is the volume when the pressure is infinite. I still believe that to be the case. If not, Algebra must have changed considerably since I learned it.

My critic speaks of a totally different thing (with which I was not concerned), which may be $\alpha/4$ or $\alpha/4\sqrt{2}$, or (as I think is more plausible) $\alpha/8$. But he says that liquids can be *compressed* to 0·2 or 0·3 of their bulk at ordinary temperatures and pressures. I was, and remain, under the impression that this could be done *only at absolute zero*, and then no compression is required.

There are other misrepresentations of my statements, quite as grave as those cited. But it would be tedious to examine them all. I have no objection to a savage review, anonymous or not; on the essential condition, however, that it be *fair*. It is clear from what I have shown that this essential condition is absent.

But my critic, when his statements are accurate, finds fault with the form of my work. I will take two examples of this kind, and examine them.

(3) He blames me for not using C.G.S. units. The *Challenger* Reports are, as a rule, written in terms "understanded of" nautical men. I wonder what such men would have said of me, in their simple but emphatic vernacular, if I had spoken of a pressure of 154,432,200 C.G.S. units, when I meant what they call a "ton"; or, say, of 185,230 C.G.S. units, when I meant a "naut."

(4) I am next blamed for "mixing units."

I should think that if we could find a formula expressing, in terms of a man's age, the average rate at which he can run, say for instance

$$v = \frac{Ax(B - x)}{x^2 + C},$$

even my critic would express A in feet per *second*, and take $x$ as the mere number denoting the age in *years*. Would he, alone in all the world, insist on expressing $x$ as denoting the age in *seconds* in order to prevent what he calls the mixing of units? This is a case precisely parallel to the one in question.

Generally, I would remark that my critic seems to have written much more for the purpose of displaying his own knowledge than of telling the reader what my Report contains. For at least three of the most important things in my Report are not even alluded to :—the compressibility of mercury, the *nature* of Amagat's grand improvement of the *Manomètre Desgoffes*, and (most particularly) the discussion of the wonderful formula for the compressibility of water given in the splendid publications of the *Bureau International*.

P. G. TAIT.

THE last volume of the *Challenger* Reports contains papers on various branches of science. The review which appeared in NATURE was not the work of one writer, and was therefore not signed, but I have no desire to avoid taking full responsibility for the part of which I am the author.

It will be convenient to reply to Prof. Tait in paragraphs numbered to correspond with his own.

(1) Of course I fully accept Prof. Tait's account of his knowledge of Van der Waals' theory at the time when his *Challenger* Report was written, but I entirely dissent from his statement that what he said about it in the Addendum referred to in the review was "to the effect" described above.

It is hardly possible to do justice to my own case without quoting freely, but I will compress as much as possible. He says (p. 60) that he "was informed" (which implies that he did not previously know) that "one of Van der Waals' papers . . . contains an elaborate study of the molecular pressure in fluids."

Again he says, "I have left the passages . . . which refer to this subject in the form in which they stood before I became acquainted with Van der Waals' work. I have not sufficiently studied his memoir to be able as yet to form a definite opinion whether the difficulty... which is raised in Appendix E. can, or cannot, be satisfactorily met by Van der Waals' methods."

Further, he states that he "had been under the impression . . . that Laplace's views had gone entirely out of fashion—having made, perhaps, their final appearance . . . about 1850."

As a matter of fact, Van der Waals adopted Laplace's views in 1873, and his formula differs from the expression $pv = \text{RT}$, only by the introduction of two terms, one of which is obviously an additional pressure such as is deduced from Laplace's theory.

I do not think that any reader could be expected to conclude from these passages in Prof. Tait's Addendum that when writing the paper he had long known the "main features of Van der Waals' investigation." To me they seemed to mean that he had not previously been acquainted with Van der Waals' work, nor with his methods, nor with the facts that he studied molecular pressure and adopted Laplace's ideas.

While, therefore, I willingly submit to Prof. Tait's correction of the phrase that he had "never heard of Van der Waals," I cannot admit that, on the evidence then before me, I did him any substantial injustice.

(2) I very much doubt whether the distinction between the ultimate volume and the molecular volume can be maintained if the equations are treated as empirical; and even if they are not, I doubt whether the ultimate volume, as defined by Prof. Tait, has any real physical meaning. The value of $v$ when $p = \infty$ is independent of the temperature, whether deduced from the theoretical formula to which Prof. Tait refers (p. 48), or from those of Van der Waals or Clausius: hence it must (from this point of view) be the molecular volume. In the case of Prof. Tait's new equation, which was published after his Report was completed, and which is the only one I had not seen when I wrote the review, the results when we put $p = \infty$ or $\text{T} = 0$, are such as to show that its application to these extreme cases is not legitimate. My own view is that such algebraical solutions are worth very little, and I only discuss them because I wish to show that if we admit them at all they justify my treating Prof. Tait's number as an estimate of the molecular volume.

(3) I cannot say that I think that Prof. Tait's reason is adequate. The Royal Naval College at Greenwich has done more for our naval officers than he would have us believe, and, if it were not so, the *Challenger* Reports are not addressed to members of any one profession, nor intended for English-speaking scientific men alone. Their cosmopolitan character is shown by the fact that bound up in the same volume with Prof. Tait's Report is another by a distinguished Belgian geologist.

Foreigners have helped to describe the specimens which our Expedition collected; they will read the Reports which our experts have written. It would have required but a few minutes' work, and a few additional lines of print, to have given the final results in terms which they would have understood at a glance.

(4) The analogy is fallacious. Prof. Tait has devised a formula into which he introduces two quantities (age and speed), which are commonly expressed with reference to different units of time.

I pointed out that he had expressed in the same formula (contrary to common usage) the same quantity (pressure) in terms of two different units, of which one is not ordinarily used by many of those who will make use of his work.

As to the last paragraph, I have only two remarks to make, First, that I think Prof. Tait does himself injustice in regarding a description of apparatus devised by another, and the discovery of a blunder of the Bureau International, as two of the most important things in his Report. Secondly, that I think the imputation of motives should be banished from scientific discussions.

In conclusion, I wish to add that probably I should have left Prof. Tait's defence unanswered if he had not accused me of unfairness. I have no desire for any controversy, and no wish to impugn his knowledge of the theory of gases. But he will forgive my reminding him of the old saying, "*Noblesse oblige.*" A classical research should not be published in a state which leads the reader to the conclusion that the author was only just becoming acquainted with facts which bear upon his work and have been long before the world. As a reviewer, I formed the opinion that the Report under discussion was open to this criticism. As a reviewer, it was my duty to express my opinion in all honesty, and, as I hope, in all courtesy.

ARTHUR W. RÜCKER.

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## Visualized Images produced by Music.

IN the annexed paper, and in her own words, are related the very curious effects produced on a lady friend by certain musical tones and orchestral combinations. They are so very singular, so entirely outside my experience, and, withal, so inexplicable, that I shall be glad if you will give them a place in your columns, in the hope that some of your readers—physiological or psychological—may be able to throw some light on them.

I should state that the lady is in perfect health, is very intelligent, an accomplished musician, and not at all, in this or any sense, the victim of a disordered imagination. She is quite conscious that these spectral images have only a subjective existence, though visually they have all the vividness of presentment which belongs to realities.

At the first blush it would seem as though these apparitions were in some way a response to stimuli sent through the auditory nerve; but this, if any, is an imperfect explanation, since it will be noticed that occasionally these visualized pictures *slightly precede the instrument they belong to.*

This fact suggests that a state of unconscious expectancy may be a factor in their reproduction, but it fails entirely, I think, to account for their initial appearance. GEO. E. NEWTON.

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"The sound of an oboe brings before me a white pyramid or obelisk, running into a sharp point; the point becoming more acute if the note is acute, blunter if it is grave. The obelisk appears to be sharply defined and solid if the note is loud, and vague and vaporous if it is faint. All the notes of the 'cello, the high notes of the bassoon, trumpet, and trombone, and the low notes of the clarionet and viola, make me see a flat undulating ribbon of strong white fibres.

"The tone of the horn brings before me a succession of white circles of regularly gradated sizes, overlapping one another. These circles and the ribbon float past me horizontally, but the point of the obelisk seems to come at me.

"In an orchestra, when the violins strike up, after the wind band has been prominent for a time, I see often, but not always, a shower of bright white dust or sand, very crisp and glittering. I am taking note of the recurrence of this impression, and think it is becoming more frequent, but it is not invariable like the others.

"I have heard a great deal of orchestral music all my life, but I have only noticed these effects for four or five years. They gained gradually in frequency and clearness, and now the first three are invariable.

"If I know the scoring of a piece well, the various effects *slightly precede* the instrument they belong to; only the objects are vague and faint till the sound begins.

"Sometimes, if an oboe passage has an intense and yearning character, the white point comes so near me, and moves so rapidly, that I think it *must wound me.*

"I am very anxious to make it clear that I am not trying to describe a mental state by symbols, but that *I actually see* the point, the fibres, and the circles. Generally they seem to float half-way between me and the orchestra.

"If only one class of instruments is used, the effect does not extend beyond the opening bars: for instance, in a string quartette I only see the white sand for a moment at the beginning; if, however, wind and stringed instruments are combined, I see the various effects again and again in one piece."

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## Foreign Substances attached to Crabs.

IN your issue of December 26, 1889 (p. 176), Mr. Pascoe drew attention to the cases of certain crabs which are frequently found covered with sponges, algæ, shells, &c., and brought forward also the well-known case of the Gastropod *Phorus*. He at the same time confessed that he could not see "where protection came in" in any of the cases which he cited. Mr. A. O. Walker, on the other hand (NATURE, January 30, p. 296), regards it as obvious that the attachment of these foreign substances is a useful adaptation for purposes of concealment. Prof. Herdman also (NATURE, February 13, p. 344) bears witness to the "scarcely recognizable" appearance of the crab *Hyas* when covered with algæ, &c. Indeed, no one who has seen one of these crabs brought up with the dredge, or has found a well-covered *Stenorhynchus* on our own shores, can seriously doubt the usefulness of the habit in rendering the animal inconspicuous. In *Stenorhynchus* and *Inachus* the process of "dressing" with weeds and zoophytes has been described by Bateson (Journ. Mar. Biol. Association, vol. i. 1889, p. 213), and it is seen from his description that, as also in the cases of *Dorippe*, *Pagurus*, *Dromia vulgaris*, &c., the foreign substances or animals become attached to the body not by accident but by the act of the crabs themselves.

Now Mr. Walker, in regarding *all* these cases as instances of adaptation for concealment, has overlooked the fact that in two of our British species of hermit crab (*Pagurus bernhardus* and *P. prideauxii*) it is the habit of the animals to prefer, and often to fight for, shells which are rendered conspicuous by the attachment to them of species of Anemone, in the one case *Adamsia rondeletii* (*Sagartia parasitica*), in the other *Adamsia palliata*. Another British species (*Pagurus cuanensis*) is almost invariably found inhabiting a shell enveloped in the sponge *Suberites domuncula*, which is frequently of a conspicuous orange-red colour Only in the smallest species of *Pagurus* (*e.g.*, *P. lævis*) does the animal depend invariably upon an inconspicuous appearance for its safety.

The value to the crabs of a preference for shells to which Actinians are attached is found in the fact that these gaily-coloured animals are carefully shunned by fishes on account of their stinging powers; and although hermit crabs themselves are very palatable to fishes, their association with Actinians, while rendering them conspicuous as they move about, is at the same time an efficient protection from the persecution of their enemies.

This also explains the habits of the two Mauritian crabs, which, according to Möbius, carry about a sea-anemone in each claw.

The sponge with which *Pagurus cuanensis* is associated is (like all other sponges with which I have experimented) exceedingly obnoxious to fishes on account of its bad smell and taste. I have never succeeded in inducing a fish of any species to swallow a fragment of the sponge; but on the contrary the smell is in most cases quite sufficient to drive the fish away. The association with the sponge is therefore here also an efficient protection, for I know of no fish capable of extracting the crab from its retreat. It is seen from this that the case of *Dromia vulgaris* should probably be removed from the category of adaptations for concealment, and, like the cases of *P. bernhardus*, &c., be included in a special group of *warning* adaptations.

There yet remains the interesting case, adduced by Dr. R. von Lendenfeld, of *Dromia excavata* associated with a Compound Ascidian of the genus *Atopogaster* (Herdman). This, I believe, will be found to belong to the same category of warning adaptations, for after repeated experiments with Compound and other *Tunicata* at the Plymouth Laboratory I can state that these animals are essentially inedible to fishes. The inedibility is in large part due, as in the case of sponges, to the characteristic odour which *Tunicata*, and more especially Compound *Tunicata*, give out, and in no family (excepting perhaps the *Botryllidæ*) is this better marked than in the *Polyclinidæ*, the group to which *Atopogaster* belongs. Bearing in mind also the fact that Composite Ascidians frequently vie with sponges and Actinians in the possession of varied and conspicuous colours, it is rendered practically certain that the case of *Dromia excavata* is another instance of this same type of adventitious warning contrivances.

Thus the edible (the edibility is not yet proved for foreign species) *Crus acea* which attach foreign substances to their bodies may be divided into two groups:—

(α) Those which are rendered inconspicuous in relation to their natural surroundings by the habit; *e.g.*, *Stenorhynchus*, *Hyas*, *Dorippe*, *Pagurus lævis*, and young forms of *Pagurus bernhardus*, &c.

(β) Those which associate themselves with animals, easily recognizable by, and possessing qualities offensive to, their chief enemies; *e.g.*, *Dromia vulgaris* and *excavata*, *Pagurus bernhardus*, *prideauxii*, and *cuanensis*. WALTER GARSTANG.

Laboratory of the Marine Biological Association,
Plymouth, February 21.

P.S.—From facts which Mr. Weldon and Mr. Harmer have communicated to me, it would appear that *Dromia vulgaris* frequently attaches Compound Ascidians (*Leptoclinum maculosum*, *Botrylloides Gasconiæ*) to its back instead of sponges, a variation of habit which is very interesting in connection with the apparently fixed habit of the Australian species.—W. G.

### A Key to the Royal Society Catalogue.

"A CATALOGUER" appears to have misunderstood me in two points. In the index that I propose, the heads would not be numbered. Again, in forming an estimate of the size of the work, I made the supposition that the 8 papers of an author could be grouped, not under 8, but under 3 heads.

JAMES C. MCCONNEL.

### A Meteor.

LAST night (Monday, the 3rd), as I was crossing the Old Deer Park to Richmond, I witnessed the flight of an exceptionally fine meteor, which shone out with great brilliancy notwithstanding the presence of a bright moon, which was almost at the full.

It appeared to start from the constellation of Leo, and travelled across the sky to the westward, vanishing some 10° or 15° above the horizon.

The night was very quiet at the time, and I heard no report.

T. W. BAKER.

Kew Observatory, Richmond, Surrey, March 4.

## THE DISCOVERY OF COAL NEAR DOVER.

THE question of the existence of coal under the newer rocks of Southern England, which has engaged the attention of some of our leading geologists since the year 1855, has found its final answer in the discovery announced last week in the daily press. The story of the discovery is a striking example of the progress of a scientific idea, passing through various phases, and growing more clearly defined through opposition and failure, until ultimately it has been proved to be true, and likely to lead to industrial changes of national importance.

The question was originally started 35 years ago by Mr. Godwin-Austen in a memorable paper brought before the Geological Society of London, in which it was argued, from the character and arrangement of the coal-fields and associated rocks of Somersetshire and South Wales on the west, and of the Belgian and North French coal-fields on the east, that similar coal-fields lie buried beneath the newer strata of the intervening regions. Mr. Godwin-Austen pointed out that the general direction of the exposed coal-fields was ruled by a series of great east and west folds, running parallel to the great line of disturbance—"the axis of Artois,"—from the south of Ireland, through South Wales and Northern Somerset on the west, eastwards through Belgium and Northern France, into the valley of the Rhine, near Düsseldorf. Throughout this area the exposed coal-fields lie in long east and west troughs. This series of folded Carboniferous and older rocks formed also an east and west ridge along the line of the axis of Artois, which gradually sank beneath the waves of the Triassic, Liassic, Oolitic, and Cretaceous seas. Against this the strata of the three first of these rocks gradually thin off, while the coal-measures and other rocks of the ridge have repeatedly been struck in France and Belgium, and are now being worked immediately underneath the Cretaceous strata over a wide area.

The axis of Artois also, where it is concealed by the newer rocks in the south of England, is marked from Somerset eastwards by the anticlinal of the chalk of North Wiltshire, and the line of the North Downs, the general law seeming to be "that when any great folding and dislocation of the earth's crust has taken place, each subsequent disturbance follows the very same lines, and that simply because they are lines of least resistance."

Mr. Godwin-Austen, by combining all these observations, finally concluded that there were coal-fields beneath the Oolitic and Cretaceous rocks of the south of England,

and that they were sufficiently near the surface to allow of their being of great economic value. He further specified the line of the Thames Valley, and the region of the Weald, as possible places where they might be discovered.

These important conclusions were during the next 11 years generally received by geologists, with the exception of Sir Roderick Murchison. The next important step in the direction of their verification was that taken by the Coal Commission of 1866–67, by whom Mr. Godwin-Austen and Sir R. Murchison were examined at length, and the results of the inquiry embodied in the Report by Mr. Prestwich. In the Report, Mr. Godwin-Austen's views are accepted, and fortified by a vast number of details relating both to the coal-fields of Somersetshire and of France and Belgium. Mr. Prestwich also calls special attention to the physical identity of the coals of these two regions, and to the fact that the Carboniferous and older rocks in both are similarly disturbed. He concludes, further, that the coal-fields which now lie buried beneath the newer rocks are probably equal in value and in extent to those which are exposed in Somerset and South Wales on the west, and in Belgium and France on the east.

In 1872 the Coal Commission Report was published, and in the same year the Sub-Wealden Exploration Committee was organized[1] by Mr. Henry Willett to test the question of the existence of coal in the Wealden area by an experimental boring. The site chosen was Netherfield, near Battle, in Sussex, where the lowest rocks of the Wealden formation form the bottom of the valley. It was resolved to go down to the older Palæozoic strata, which were thought to occur at about 1000 feet from the surface, or to carry the bore-hole to 2000 feet if they were not struck before. The work was carried on under considerable difficulties for the next three years, until in 1875 it had to be abandoned at a depth of 1905 feet, because of the breakage of many hundred feet of lining-pipes, coupled with the loss of the boring-tool at the bottom. The section of the strata passed through is as follows :—

*Netherfield Section.*

| | Feet. |
|---|---|
| Purbeck strata | 200 |
| Portland strata | 57 |
| Kimmeridge Clay[2] | 1073 |
| Corallian rocks[2] | 515 |
| Oxford Clay | 60 |
| | 1905 |

This section, although it yielded no information as to the Palæozoic rocks, showed that in this particular district they are more than 1900 feet beneath the surface, and revealed the great thickness of the Kimmeridge Clay and Corallian rocks, sufficiently distant from the ridge of coal-measures and older rocks, against which the Oolitic strata thin away to the north, to allow of an accumulation of Oolitic sediments to a thickness of more than 1700 feet. In this respect, therefore, it afforded unmistakable evidence that the search for the ridge in question might be carried on with much greater chance of success further to the north, in the direction of the North Downs. The great and increasing thickness of the successive newer rocks of the Wealden formation, which form the surface of the ground between Netherfield and the North Downs, rendered it undesirable to repeat the experiment within the Wealden area proper. Close to Battle, the Secondary strata were of great thickness, and where the whole series of Wealden rocks were present, they were more than 1000 feet thick.

For the next eleven years the problem remained where it was left by the results of the Netherfield boring; while in the district of London, evidence was being collected in various sinkings for water, which proved the existence of the Palæozoic ridge of rocks, Silurian and old red sandstones, older than the Carboniferous, at about 1000 feet from the surface. Here, too, the Oolitic strata were not more than 87 feet in thickness, at their thickest point in the well at Richmond. The older rocks, moreover, were inclined at a very high angle, as in the case of the similar rocks underlying the coal-fields of Somerset, and of Northern France and Belgium, and this implied the existence of troughs of coal-measures in the synclinal folds, in neighbouring areas.

I come now to the last experiment, which has been so fortunately crowned with success. In 1886, I reported to Sir Edward Watkin that it was desirable, both on scientific and commercial grounds, for a boring to be put down in South-East Kent, in the neighbourhood of Dover, and that the Channel Tunnel works under the Shakespear Cliff would be the best site for the experiment. It was almost within sight of Calais, where the coal-measures had been proved at a depth of 1092 feet. It was also not many miles away from the spot where a large mass of bituminous material—which, according to Mr. Godwin-Austen, was the result of the distillation of coal from the measures beneath—had been discovered in the chalk. Sir Edward Watkin acted with his usual energy on my report, and the work was begun in 1886, and carried on, under my advice, down to the present time. The boring operations have been under the direction of Mr. F. Brady, the chief engineer of the South-Eastern Railway, to whose ability we owe the completion of the work to its present point, under circumstances of great difficulty. The strata passed through may be generalized as follows :—

*Section at Shakespear Cliff, Dover.*

| | Feet. |
|---|---|
| Lower Grey Chalk, and Chalk-Marl | 500. |
| Glauconitic Marl | |
| Gault | |
| Neocomian | |
| Portlandian | 660. |
| Kimmeridgean | |
| Corallian | |
| Oxfordian | |
| Callovian | |
| Bathonian | |
| Coal-measures, sandstones, and shales and clays, with one seam of good blazing coal, struck at 1180 feet from the top of the bore-hole | 20. |

The coal-measures were struck at a depth of 1160 feet, or 68 feet below the point where the coal-measures were met with in the boring at Calais. It may also be noted as a remarkable confirmation of Mr. Godwin-Austen's views as to the abrupt thinning off of the Wealden strata, that, although along the line of the North Downs the Weald clay strikes towards the French coast, and is seen at low water between Hythe and Folkestone, it and the underlying Wealden strata are not represented in the section at the Shakespear Cliff.

It is too soon as yet to measure the full value of this discovery near Dover, while our work is as yet unfinished. We may, however, remark that the coal-fields of the Continent, which have been proved beneath the newer rocks in Northern France and Belgium, some 60 miles to the west of their eastern outcrops, have now been traced across the Channel, that they are at a workable depth, and that we have now a well-defined base for further researches in Southern England.

W. BOYD DAWKINS.

[1] The Committee consisted of Profs. Ramsay and Phillips, Sir John Lubbock, Sir Philip Egerton, and Messrs. Thomas Hawksley, Warington Smyth, Prestwich, Bristow, Etheridge, Boyd Dawkins, and Topley.

[2] The precise boundary between these two groups is uncertain. If the Kimmeridge Clay series be taken down to the Coralline Oolite, its thickness will be 1512 feet.

## *THE RELATION BETWEEN THE ATOMIC VOLUMES OF ELEMENTS PRESENT IN IRON AND THEIR INFLUENCE ON ITS MOLECULAR STRUCTURE.*

IN a lecture on the Hardening and Tempering of Steel, published in November last (NATURE, vol. xli. pp. 11, 32), an attempt was made to set forth the prominent facts developed in recent researches, more especially those of M. Osmond, which tend to prove that iron, like many other elements, can pass from the normal state to an allotropic one. It was shown that as a mass of iron or steel cools down, there are at least two distinct evolutions of heat, one occurring at a variable temperature not higher than 855° C., the other at a more constant temperature, near 650° C. From a long series of most patient investigations, Osmond argues that there are two kinds of iron, one [hard] $\beta$ iron, and the other [soft] $\alpha$ iron. The molecular change from $\beta$ to $\alpha$ iron is indicated by the first evolution of heat in the cooling mass of iron or steel, and at this point the cooling mass of iron regains the magnetic properties which it loses at higher temperatures. The second evolution of heat only occurs in carburized iron or steel, and marks the point at which carbon itself changes from the dissolved or 'hardening-carbon,' to the state of combined or 'carbide-carbon.' In highly carburized steel, the two points at which heat is evolved coincide, and experimental evidence has been given (*loc. cit.* p. 34) as to the abnormal molecular weakness which is exhibited when a very hot bar of such steel cools down to about 660° C. In a recent communication to NATURE (February 20, p. 369), Prof. Carl Barus, of Washington, has pointed out, with reference to this molecular weakness, "that when iron passes through the temperature of recalescence its molecular condition is almost chaotic"; whilst with regard to Osmond's view that $\alpha$ iron passes to $\beta$ iron when submitted to any stress which produces permanent deformation of the mass, Prof. Barus says that "there is reason to be urged even in favour of the extreme view" that such molecular change may be produced in most metals. In the lecture at Newcastle, I expressed the belief (NATURE, *loc. cit.*) that it would be shown that the influence of small quantities of other elements on masses of iron would be found not to be at variance with the periodic law. I had already given experimental evidence to show that the action of small quantities of impurity on the tenacity of gold was closely in accordance with that law, but in the case of iron it was difficult to say what property of the metal would be most affected by the added matter. It appeared safe, however, to point to the possibility that the direct connection with the periodic law would "be traced by the effect of a given element in retarding or promoting the passage of ordinary iron to the allotropic state," a point of much importance, as the mechanical properties of the metal must depend on the atomic arrangement in the molecules.

I am glad that so eminent an authority and admirable experimenter as M. Osmond has satisfied himself as to the probable accuracy of this view. In two recent papers communicated to the Académie des Sciences, the results of his experiments are given, and the following is a translation of the later of these (*Comptes rendus*, vol. cx. p. 346):—

"Within the last few years and quite recently (*Comptes rendus*, Séances des 26 octobre et 6 décembre 1886, 4 avril 1887, et 3 février 1890), I have had the honour to submit to the Academy facts relating to the allotropic modifications of iron, and to the part played in such changes by foreign bodies alloyed with the mass. Prof. Roberts-Austen, by studying the effect produced on the mechanical properties of gold by the addition of the same weight (about 0·2 per cent.) of seventeen foreign metals, has discovered a curious relation between the results obtained and the position occupied by the added metals in the periodic classification (Phil. Trans. Roy. Soc., vol. clxxix. p. 339, 1888). Prof Roberts-Austen has deduced from this that an analogous relation should exist for iron, but the irons and steels of commerce are such complex products, and the same metal may assume such different aspects, that the relation in question is not readily apparent from a study of their mechanical properties.

"In reviewing my former experiments with these new ideas as guides, it appeared to me that the law of Roberts-Austen was well based, and new experiments undertaken to verify it have only confirmed my first view.

"The foreign elements whose action on the critical points of iron I have studied experimentally with more or less completeness, are ranged as follows in two columns in the order of their atomic volumes:—

| I. | Atomic volume. | II. | Atomic volume. |
|---|---|---|---|
| Carbon ... ... | 3·6 | Chromium ... | 7·7 |
| Boron ... ... | 4·1 | Tungsten ... ... | 9·6 |
| Nickel ... ... | 6·7 | Silicon ... ... | 11·2 |
| Manganese ... | 6·9 | Arsenic ... ... | 13·2 |
| Copper ... ... | 7·1 | Phosphorus ... | 13·5 |
| | | Sulphur ... ... | 15·7 |

"The elements in column I., whose atomic volumes are smaller than that of iron (7·2), delay during cooling, *cæteris paribus*, the change of $\beta$ [hard] iron to $\alpha$ [soft] iron, as well as that of 'hardening-carbon' (*carbone de trempe*) into 'carbide-carbon' (*carbone de recuit*). For these two reasons they tend to increase, with equal rates of cooling, the proportion of $\beta$ iron that is present in the cooled iron or steel, and consequently the hardness of the metal. Indeed, their presence is equivalent to a more or less energetic hardening.[1]

"On the other hand, the elements of column II., whose atomic volumes are greater than that of iron, tend to raise or at least to maintain near its normal position, during cooling, the temperature at which the change of $\beta$ to $\alpha$ iron takes place; further, they render the inverse change during heating more or less incomplete, and usually hasten the change of 'hardening-carbon' to 'carbide-carbon.'[2]

"Thus they maintain the iron in the $\alpha$ [soft] state at high temperatures, and must therefore have the same effect in the cooled metal. In this way they would act on iron as annealing does, rendering it soft and malleable, did not their individual properties, or those of their compounds, often intervene and partially mask this natural consequence of their presence.

"The essential part, therefore, played by foreign elements alloyed with iron, is either to hasten or delay the passage of iron, during cooling, to an allotropic state, and to render the change more or less incomplete in one sense or the other, according to whether the atomic volume of the added impurity is greater or less than that of iron. In other words, foreign elements of low atomic volume tend to make iron itself assume or retain the particular molecular form that possesses the lowest atomic volume, whilst elements with large atomic volume produce the inverse effect.

"It should be noted that carbon, whilst obeying the general law, possesses on its own account the property of undergoing, at a certain critical temperature, a change the nature of which is still disputable, although its existence is acknowledged. It is this property which gives carbon a place by itself in the metallurgy of iron."

M. Osmond has shown me the curves which represent the results of his experiments, and these will doubtless

[1] To the elements of column I. hydrogen may be added. As is well known, this element renders electro-deposited iron hard and brittle; perhaps it would be better to say with Graham *hydrogenium*, for hydrogen gas does not appear to have a marked influence on the critical temperature.

[2] Tungsten alone presents certain anomalies.

soon be published. Whatever may ultimately prove to be the true nature of the molecular change which accompanies the thermal treatment of iron and determines its mechanical properties, there is little doubt but that there is a close relation between the action of foreign elements and their atomic volume. Few metallurgial questions are of greater interest at the present time than those which relate to the molecular structure of metals, and the admirable work of M. Osmond has shown it to be very probable that the presence of a small quantity of a foreign metal may cause a mass of another metal to pass into an allotropic state. In relation to iron and steel the problems are of great industrial importance, and it is fortunate that we appear to be nearing the discovery of a law in accordance with which all metallic masses are influenced by "traces." W. C. ROBERTS-AUSTEN.

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## *SEDGWICK AND MURCHISON: CAMBRIAN AND SILURIAN.*[1]

ERRONEOUS impressions have long existed among American geologists with regard to the relations to one another, and to Cambrian and Silurian geology, of Sedgwick and Murchison. The Taconic controversy in this country served, most unreasonably, to intensify feelings respecting these British fellow-workers in geology, and draw out harsh judgments. Now that right views on the American question have been reached, it is desirable that the facts connected with the British question should be understood and justly appreciated.

Sedgwick and Murchison were literally fellow-workers in their earlier investigations. Prof. John Phillips, in a biographical sketch of Sedgwick (NATURE, vol. vii. p. 257), whose intimate friendship through fifty years "he had the happiness of enjoying," speaks thus, in 1873, of their joint work :—

"Communications on Arran and the north of Scotland, including Caithness (1828) and the Moray Firth; others on Gosau and the Eastern Alps (1829–31); and still later, in 1837, a great memoir on the Palæozoic strata of Devonshire and Cornwall, and another on the coeval rocks of Belgium and North Germany, show the labours of these intimate friends in the happiest way—the broad generalizations in which the Cambridge professor delighted, well supported by the indefatigable industry of his zealous companion."

Prof. Phillips then speaks of the Cambrian and Silurian labours "of two of the most truly attached and mutually helpful cultivators of geological science in England."

Of these Cambrian and Silurian labours it is my purpose to give here a brief history derived from the papers they published. They were begun in 1831, without concert—Sedgwick in Wales, Murchison along the Welsh and English borders.

In September of 1831, the summer's excursions ended, Murchison made his first report at the first meeting of the British Association. It was illustrated by a coloured geological map representing the distribution of the "Transition Rocks," the outlying Old Red Sandstone, and the Carboniferous limestone (Murchison, Report of the British Association, i. 91, 1831).

These "Transition Rocks" (of Werner's system), upturned semi-crystalline schists, slates, and other rocks, passing down into uncrystalline, and regarded as mostly non-fossiliferous, the "*agnotozoic*" of the first quarter of the century, were the subject of Sedgwick's and Murchison's investigations—the older of the series, as it turned out, being included in Sedgwick's part.[2] They were early resolved into their constituent formations by Murchison, and later as completely by Sedgwick in his more difficult field.[1]

Already in March and April of 1833, Murchison showed, by his communications to the Geological Society of London, that he had made great progress; for the report says :[2]—He "separated into distinct formations, by the evidence of fossils and the order of superposition, the upper portion of those vast sedimentary accumulations which had hitherto been known only under the common terms of Transition Rocks and Grauwacke." And these "distinct formations" were : (1) the Upper Ludlow rocks; (2) the Wenlock limestone; (3) the Lower Ludlow rocks; (4) Shelley sandstones, "which in Shropshire occupy separate ridges on the south-eastern flanks of the Wrekin and the Caer Caradoc"; (5) the Black Trilobite flagstone whose "prevailing Trilobite is the large *Asaphus Buchii*, which with the associated species," he observed, "is never seen in any of the overlying groups"; and below these, (6) Red Conglomerate sandstone and slaty schist several thousand feet in thickness.

By the following January, 1834, Murchison was ready with a further report,[3] in which he described the "four fossiliferous formations" in detail, and displayed, on a folded table arranged in columns, their stratigraphical order, thickness, subdivisions, localities, and "characteristic organic remains." The subdivisions of the rock-series in the memoir are as follows, commencing above : (I.) Ludlow rocks, 2000 feet; (II.) Wenlock and Dudley rocks, 1800 feet; (III.) Horderley and May Hill rocks (afterward named Caradoc), 2500 feet; (IV.) Builth and Llandeilo flags, characterized by *Asaphus Buchii*, 1200 feet; and, below these, (V.) the Longmynd and Gwastaden rocks, many thousand feet thick, set down as unfossiliferous.

Thus far had Murchison advanced in the development of the Silurian system by the end of his third year. Upper and Lower Silurian strata were comprised in it, but these subdivisions were not yet announced.

During the interval from 1831 to 1834, Sedgwick presented to the British Association in 1832 a verbal communication on the geology of Caernarvonshire, and another brief report of progress in 1833. A few lines for each are all that was published. The difficulties of the region were a reason for slow and cautious work.

In 1834, as first stated in the Journal of the Geological Society for the year 1852, the two geologists took an excursion together over their respective fields. Sedgwick says (Quarterly Journal of the Geological Society, viii. 152, 1852): "I then studied for the first time the Silurian types under the guidance of my fellow-labourer and friend; and I was so struck by the clearness of the natural sections and the perfection of his workmanship, that I received, I might say, with implicit faith everything which he then taught me." And further, "the whole 'Silurian system' was by its author placed *above* the great undulating slate-rocks of South Wales." The geologists next went together over Sedgwick's region, and

[1] Printed from advance sheets kindly supplied by Prof. Dana. The article appears in the current number of the *American Journal of Science*.

[2] Murchison says, in the introductory chapter of his "Silurian System," p. 4, "No one [in Great Britain, before his investigations began] was aware of the existence below the Old Red Sandstone of a regular series of deposits containing peculiar organic remains." "From the days of De Saussure and Werner, to our own, the belief was impressed on the minds of geologists that the great dislocations to which these ancient rocks had been subjected had entirely dissevered them from the fossiliferous strata with which we were acquainted."

[1] The term "Transition" early appeared in American geological writings. Sixty to seventy-five years ago it was applied by Maclure, Dewey, and Eaton, to the rocks of the Taconic region and their continuation; for these were upturned, apparently unfossiliferous, semi-crystalline to uncrystalline, and extended eastward to a region of gneisses. The study of the rocks was commenced; but in 1842, before careful work for the resolution of them had been done—like that in which Murchison and Sedgwick were engaged—they were, unfortunately, put, as a whole, into a "Taconic system" of assumed pre-Potsdam age; at the same time "Transition" was shoved west of the Hudson, over rocks that were horizontal, and already resolved. Owing to this forestalling of investigation, and partly also to inherent difficulties, the right determination of the several formations comprised in this Taconic or "Transition" region was very long delayed.

[2] Murchison, Proceedings of the Geol. Soc. London, i. 470, 474, 1833, in a paper on the sedimentary deposits of Shropshire and Herefordshire.

[3] Murchison, Proc. Geol. Soc., ii. 13, 1834. The subject was also before the British Association; Report for 1834, p. 652.

the sections from the top of the Berwyns to Bala. Murchison concluded, after his brief examination, and told Sedgwick, that the Bala group could not be brought within the limits of his system. He says: "I believed it it to plunge under the true Llandeilo flags with *Asaphus Buchii*, which I had recognized on the east flank of that chain." "Not seeing, on that hurried visit, any of the characteristic Llandeilo Trilobites in the Bala limestone, I did not then identify that rock with the Llandeilo flags, as has since been done by the Government surveyors" (Q. J. G. Soc., viii. 175).

In 1835, the terms "Silurian" and "Cambrian" first appear in geological literature. Murchison named his system the "Silurian" in an article in the *Philosophical Magazine* for July of that year, and at the same time defined the two grand subdivisions of the system: (I.) the Upper Silurian, or the Ludlow and Wenlock beds; and (II.) the Lower Silurian, or the Caradoc and Llandeilo beds (*Phil. Mag.*, vii. 46, July 1835).

During the next month, August, the fourth meeting of the British Association was held at Edinburgh, and in the Report of the meeting (Brit. Assoc., v., August 1835), the two terms, "Silurian" and "Cambrian," are united in the title of a communication "by Prof. Sedgwick and R. I. Murchison," the title reading, "On the Silurian and Cambrian Systems, exhibiting the order in which the older sedimentry strata succeed each other in England and Wales." Murchison, after explaining his several subdivisions, said that "in South Wales" he had "traced many distinct passages from the lowest member of the "Silurian system" into the underlying slaty rocks now named by Prof. Sedgwick the Upper Cambrian." Sedgwick spoke of his "Upper Cambrian group" as including the greater part of the chain of the Berwyns, where, he said, "it is connected with the Llandeilo flags of the Silurian and expanded through a considerable part of South Wales"; the "Middle Cambrian group" as "comprising the higher mountains of Caernarvonshire and Merionethshire"; the "Lower Cambrian group" as occupying the south-west coast of Caernarvonshire, and consisting of chlorite and mica schists, and some serpentine and granular limestone; and finally, he "explained the mode of connecting Mr. Murchison's researches with his own so as to form one general system."

Thus, in four years Murchison had developed the true system in the rocks he was studying; and Sedgwick likewise had reached what appeared to be a natural grouping of the rocks of his complicated area. Further, in a united paper, or papers presented together, they had announced the names Silurian and Cambrian, and expressed their mutual satisfaction with the defined limits. Neither was yet aware of the unfortunate mischief-involving fact that the two were overlapping series.

It is well here to note that *the term "Cambrian" antedates "Taconic" of Emmons by seven years;* and also that Emmons did not know—any more than Sedgwick with regard to the Cambrian—that his system of rocks was in part Lower Silurian, and of Llandeilo and Caradoc age.

In May of 1838, nearly three years later, Sedgwick presented his first detailed memoir on North Wales and the Cambrian rocks to the Geological Society.[1] Without referring to the characteristic fossils, he divides the rocks below the Old Red Sandstone, beginning below, into (I.) the Primary Stratified Groups, including gneiss, mica-schist, and the Skiddaw slates, giving the provisional name of "Protozoic" for the series should it prove to be fossiliferous, and (II.) the Palæozoic Series; the latter including (1) the Lower Cambrian (answering to Middle Cambrian of the paper of 1835), (2) the Upper Cambrian, and (3) the "Silurian," or the series so called by Murchison. Without a report on the fossils, no comparison was possible at that time with Murchison's Silurian series. Yet Sedgwick goes so far as to say that the "Upper Cambrian," which "commences with the fossiliferous beds of Bala, and includes all the higher portions of the Berwyns and all the slate-rocks of South Wales which are below the Silurian System," "appears to pass by insensible gradation into the lower division of the Upper System (the Caradoc Sandstone);" and that "many of the fossils are identical in species with those of the Silurian System."[1] Respecting the Silurian System he refers to the abstracts of Mr. Murchison's papers and "his forthcoming work."

The Protozoic division included the "Highlands of Scotland, the crystalline schists of Anglesea, and the south-west coast of Caernarvonshire." It is added: "The series is generally without organic remains; but should organic remains appear unequivocally in any part of this class they may be described as the Protozoic System."

In the later part of the same year, 1838, Murchison's "Silurian System" was published[2]—a quarto volume of 800 pages, with twenty-seven plates of fossils, and nine folded plates of stratigraphical sections, besides many plates in the text—the outcome of his eight years of work. Five hundred pages are devoted to the Silurian System.

The dedication is as follows:—

"To you, my dear Sedgwick, a large portion of whose life has been devoted to the arduous study of the older British rocks, I dedicate this work.

"Having explored with you many a tract, both at home and abroad, I beg you to accept this offering as a memorial of friendship, and of the high sense I entertain of the value of your labours."

Through Murchison's investigations here recorded, as he remarks in his introduction with reasonable satisfaction, "a complete succession of fossiliferous strata is interpolated between the Old Red Sandstone and the oldest slaty rocks." He observes as follows of Sedgwick:—"In speaking of the labours of my friend, I may truly say, that he not only shed an entirely new light on the crystalline arrangement or slaty cleavage of the North Welsh mountains, but also overcame what to most men would have proved insurmountable difficulties in determining the order and relations of these very ancient strata amid scenes of vast dislocation. He further made several traverses across the region in which I was employed; and, sanctioning the arrangement I had adopted, he not only gave me confidence in its accuracy, but enhanced the value of my work by enabling me to unite it with his own; and thus have our joint exertions led to a general view of the sequence of the older fossiliferous deposits." In accordance with these statements many of the descriptions and the very numerous sections represent the Cambrian rocks lying beneath the Silurian—though necessarily with incorrect details, since neither Murchison nor Sedgwick had then any appreciation of the actual connection between the so-called Cambrian and Silurian.

The Silurian System, as here set forth, is essentially that of Murchison's earlier paper of 1835; and through the work, as each region is taken up, the rocks of the Upper and Lower divisions, and their several subdivisions, are described in order, with a mention of the characteristic fossils. As to the relations of the two grand divisions, he says that, "although two or three species of

[1] An abstract appeared in the Proc. Geol. Soc., ii. 675, 1838. A continuation of the paper appeared in 1841, *ibid.*, iii. 541. See also Q. J. Geol. Soc., viii., 1852.

[1] Of these fossils, he had mentioned "*Bellerophon bilobatus*, *Producta sericea*, and several species of *Orthis*" as occurring in the Bala limestone, "all of which are common to the Lower Silurian System," in a syllabus of his Cambridge lectures, published in 1837.

[2] Murchison's "Silurian System" bears on its title-page the date 1839. He states in the Q. J. Geol. Soc., viii. 177, 1852, that the work was really issued in 1838. The fossil fishes of the volume were described by Agassiz, the Trilobites by Murchison, and the rest of the species by Sowerby.

shells of the Upper Silurian rocks may be detected in the Lower Silurian, *the mass of organic remains in each group is very distinct.*" Later he makes the number of identical species larger; but even the newest results do not increase it so far as to set aside Murchison's general statement of 1838.

Sedgwick, with all the light which the fossils of the "Silurian System" were calculated to throw on his Upper Cambrian series, found in the work no encroachments on his field or on his views. They were still side by side in their labours among the hitherto unfathomed British Palæozoic rocks.

In 1840 and 1841, Murchison was in Russia with M. de Verneuil and Count Keyserling, and also in Scandinavia and Bohemia, seeking to extend his knowledge of the older fossiliferous rocks and verify his conclusions; and in 1845 the great work on the "Geology of Russia and the Urals" came out, with a further display of Upper and Lower Silurian life. In his Presidential addresses of 1842 and 1843, reviewing the facts in the light of his new observations, he went so far as to say that the Lower Silurian rocks were the oldest of fossiliferous rocks, and that the fossiliferous series of North Wales seemed to exhibit no vestiges of animal life different from those of the Lower Silurian group.

Still Sedgwick made no protest. He states definitely on this point in his paper of 1852 (Q. J. Geol. Soc., viii. 153, 1852), that from 1834, the time of the excursion with Murchison, until 1842, he had accepted Murchison's conclusions, including the reference of the Meifod beds to the Caradoc or Silurian, without questioning; but that from that time, 1842, he began to lose his confidence in the stability of the *base-line* of the "Silurian System." He adds that in 1842, Mr. Salter, the palæontologist, informed him that the Meifod beds were on the same horizon nearly with the Bala beds; and he accepted this conclusion to its full extent, using the words, "if the Meifod beds were Caradoc, the Bala beds must also be Caradoc or very nearly on its parallel." Thus the inference of Murchison was adopted, and discrepancy between them deferred. And on the following page he acknowledges that all his papers of which there is any notice in the Proceedings or Journal of the Geological Society between 1843 and 1846 admit this view as to the Bala beds and certain consequences of it—"mistakes," as he pronounced them six years later, in 1852 (Q.J. Geol. Soc., viii. 154, 1852).

In 1843, Sedgwick read before the Geological Society in June, a paper entitled "An Outline of the Geological Structure of North Wales," which was published in abstract in the Proceedings (iv. 251); and in November of the same year, one "On the Older Palæozoic (Protozoic) Rocks of North Wales" (from observations by himself in company with Mr. Salter), which appeared, with a map, in the Journal of the Geological Society (i. 1). The abstract in the Proceedings was prepared by Mr. Warburton, the President of the Geological Society, and the paper of the following November makes no allusion to this fact, or any objection to the abstract.

A remarkable feature of the November paper is that it nowhere contains the term "Upper Cambrian" or even "Cambrian," although the rocks are Sedgwick's Upper Cambrian, together with Murchison's Upper Silurian.

A second fact of historical interest is the use of the term "Protozoic," not in the sense in which it was introduced by him in 1838, but in that in which introduced in 1838 by Murchison, on p. 11 of his "Silurian System," where he says:—

"But the Silurian, though ancient, are not, as before stated, *the most* ancient fossiliferous strata. They are, in truth, but the upper portion of a succession of early deposits which it may hereafter be found necessary to describe under one comprehensive name. For this purpose I venture to suggest the term 'Protozoic Rocks thereby to imply the first or lowest formations in which animals or vegetables appear."

These facts are in accordance with Sedgwick's acknowledgment, already mentioned.

The map accompanying the paper as originally prepared, had colours corresponding to five sets of areas, those of the "Carboniferous Limestone," "Upper Silurian," "Protozoic Rocks," "Mica and Chlorite Slate," "Porphyritic Rocks"; and here again Cambrian, Upper or Lower, does not appear, the term Protozoic being substituted. The map, as it stands in the Journal of the Geological Society, has, in place of simply "Protozoic," the words "Lower Silurian (Protozoic)." Sedgwick complains, in his paper of 1852, pp. 154, 155, of this change from his manuscript, and attributes it to Mr. Warburton, saying that "the map with its explanations of the colours plainly shows that Mr. Warburton did not comprehend the very drift and object of my paper." "I gave one colour to this whole Protozoic series only because I did not know how to draw a clear continuous line on the map between the Upper Protozoic (or Lower Silurian) rocks and the Lower Protozoic (or Lower Cambrian) rocks." "Nor did I ever dream of an incorporation of all the Lower Cambrian rocks in the system of Siluria." Sedgwick also says on the same point: "I used the word 'Protozoic' to prevent wrangling about the words Cambrian and Silurian." But this is language he had no disposition to use in 1843, as the paper of 1843 shows.

Page 155 has a footnote. In it the aspect of the facts is greatly changed. He takes back his charges, saying, "I suspect that, in the explanation of the blank portion of the rough map exhibited in illustration of my paper I had written 'Lower Silurian and Protozoic,' and that Mr. Warburton, erroneously conceiving the two terms identical, changed the words into Lower Silurian (Protozoic)." "I do not by any means accuse Mr. Warburton of any *intentional* injustice—quite the contrary; for I know that he gave his best efforts to the abstract. But he had undertaken a task for which he was not prepared, inasmuch as he had never well studied any series of rocks like those described in my papers." Sedgwick here uses Protozoic in the Sedgwick sense, not, as above, in the Murchison sense. Sedgwick again, in 1854, speaks of "the tampering with the names of my reduced map." But these explanations of his should take the harshness out of the sentence, as it was in 1843 to 1846 out of all his words.

The paper has further interest in its long lists of fossils in two tables: (I.) "Fossils of the Older Palæozoic (Protozoic) Rocks in North Wales, by J. W. Salter and J. de C. Sowerby," showing their distribution; and (2) "Fossils of the Denbigh Flagstone and Sandstone Series."

Thus, until 1846, no serious divergence of views had been noted by Sedgwick. This is manifested in his paper on the "Slate-rocks of Cumberland," read before the Geological Society on January 7 and 21, 1846 (Q. J. Geol. Soc., ii. 106, 122, 1846), which says, on the last page but one: "Taking the whole view of the case, therefore, as I know it, I would divide the older Palæozoic rocks of our island into three great groups—(3) the upper group, *exclusively Upper Silurian;* (2) the middle group, or *Lower Silurian*, including Llandeilo, Caradoc, and perhaps Wenlock; (1) the first group, or *Cambrian;*" differing in this arrangement from Murchison only in the suggestion about the Wenlock. The italics are his own. He adds:—

"This arrangement does no violence to the Silurian system of Sir R. Murchison, but takes it up in its true place; and I think it enables us to classify the old rocks in such a way as to satisfy the conditions both of the fossil and physical as well as mineralogical development."

But before the year 1846 closed, not only the overlapping of their work was recognized, but also the consequences ahead, and divergence of opinion began.

In December a paper was presented by Sedgwick to the Geological Society, on "The Fossiliferous Slates of North Wales, Cumberland, Westmoreland, and Lancashire" (Q. J. Geol. Soc., iii. 133, December 1846), which contains a protest against the downward extension of the Silurian so as to include the Cambrian. It is excellent in spirit and fair in argument. Many new facts are given respecting sections of the rocks in South Wales and North Wales, in some of which occur the Lingula flags, and characteristic fossils are mentioned. In describing some South Wales sections, Sedgwick uses the term "Cambro-Silurian" to include, beginning below: (1) "conglomerates and slates, (2) Lower Llandeilo flags, (3) slates and grits (Caradoc sandstone of Noeth Grug, &c.), (4) Upper Llandeilo flag, passing by insensible gradations into Wenlock shale." The Cambrian series is made to include: (1) the Festiniog or Tremadoc group; (2) roofing-slates, &c., the "Snowdonian group," fossiliferous in Snowdon, &c.; (3) the Bala group; and then (4) "the Cambro-Silurian group," comprising "the lower fossiliferous rocks east of the Berwyns between the Dee and the Severn—the Caradoc sandstone of the typical country of Siluria—and the Llandeilo flags of South Wales, along with certain associated slates, flags, and grits." The extension of the term Silurian down to the Lingula flags, or beyond, is opposed, because the beds below the Llandeilo are not part of the Silurian system; the term Silurian [derived from the Silures of South-East Wales and the adjoining part of England] is not geographically applicable to the Cambrian rocks; and because the only beds in North Wales closely comparable "with the Llandeilo flags are at the top of the whole Cambrian series." This last reason later lost its value when it was proved, as Sedgwick recognized years afterward, that Murchison's Llandeilo flags were really older than Sedgwick's Bala rocks.

Sedgwick's paper was followed, on January 6, with one by Murchison (Q. J. Geol. Soc., iii. 165, January 1847) objecting to this absorption of the Lower Silurian, and reiterating his remark of 1843 that the fossiliferous Cambrian beds were Lower Silurian in their fossils, and arguing, thence, for the absorption of the Cambrian, to this extent, by the Silurian. Having, eight years before, in his great work on the "Silurian System," described the Lower Silurian groups with so much detail, and with limits well defined by sections and by long lists of fossils, over a hundred species in all, many of them figured as well as described, and having thus added a long systematized range of rocks to the lower part of the Palæozoic series, he was naturally unwilling to give up the name of Lower Silurian for that of Upper Cambrian or Cambro-Silurian. Moreover, the term "Silurian," with the two subdivisions of the system, the Upper and Lower, had gone the world over, having been accepted by geologists of all lands as soon as proposed, become affixed to the rocks to which they belonged, and put into use in memoirs, maps, and geological treatises.

In 1852, the controversy, begun by encroachments not intended on either part, reached its height. Sedgwick's earnest presentation of the case (Q. J. Geol. Soc., viii. 152), and appeal before the Geological Society in February of that year—making the latter part of a memoir by him on the "Classification and Nomenclature of the Lower Palæozoic Rocks of England and Wales"—argues, like that of 1846, for the extension of the Cambrian from below upward to include the Bala beds, and thereby also the Llandeilo flags and Caradoc sandstone, although he says, "my friend has published a magnificent series of fossils from the Llandeilo flagstone." Sedgwick also expresses dissatisfaction with Mr. Warburton's abstract of his paper of June 1843, and with the change made in his map of November 1843, but, as shown above, he has no blame for Murchison and little for Mr. Warburton. He also points out some errors in the stratigraphical sections of the "Silurian System"—since the publication of which fourteen years had passed. He closes with the words (p. 168):—

"I affirm that the name 'Silurian,' given to the great Cambrian series below the Caradoc group, is historically unjust. I claim this great series as my own by the undoubted right of conquest; and I continue to give it the name 'Cambrian' on the right of priority, and, moreover, as the only name yet given to the series that does not involve a geographical contradiction. The name 'Silurian' not merely involves a principle of nomenclature that is at war with the rational logic through which every other Palæozoic group of England has gained a permanent name, but it also confers the presumed honour of a conquest over the older rocks of Wales on the part of one who barely touched their outskirts, and mistook his way as soon as he had passed within them.

"I claim the right of naming the Cambrian rocks because I flinched not from their difficulties, made out their general structure, collected their fossils, and first comprehended their respective relations to the groups above them and below them, in the great and complicated Palæozoic sections of North Wales. Nor is this all,—I claim the name Cambrian, in the sense in which I have used it, as a means of establishing a congruous nomenclature between the Welsh and the Cumbrian mountains, and bringing their respective groups into a rigid geological comparison; for the system on which I have for many years been labouring is not partial and one-sided, but general and for all England."

Sedgwick does not seem to have recognized the fact that Murchison had the same right to extend the Silurian system to the base of the Llandeilo beds, whatever its horizon, that he had to continue the Cambrian to the top of the Bala beds.[1]

Murchison's reply was made at the meeting of the Geological Society in June (Q. J. Geol. Soc., viii. 173, 1852). He remarked, with regard to Sedgwick's allusion to the excursion of 1834, that, "if I lost my way in going downward into the region of my friend, it was under his own guidance; I am answerable only for Silurian and Cambrian rocks described and drawn as such within my own region."

In his closing remarks Murchison says:—

"I am now well pleased to find that, with the exception of my old friend, all my geological contemporaries in my own country adhere to the unity of the Silurian System, and thus sustain its general adoption.

"No one more regrets than myself that Cambrian should not have proved, what it was formerly supposed to be, more ancient than the Silurian region, and thus have afforded distinct fossils and a separate system; but as things which are synonymous cannot have separate names, there is no doubt that, according to the laws of scientific literature, the term 'Silurian' must be sustained as applied to all the *fossiliferous* rocks of North Wales.

"Lastly, let me say to those who do not understand the nature of the social union of the members of the Geological Society, that the controversy which has prevailed between the eloquent Woodwardian Professor and myself has not for a moment interrupted our strong personal friendship. I am indeed confident we shall slide down the hill of life with the same mutual regard which animated us formerly when climbing together many a mountain both at home and abroad."

Murchison was right in saying that all British geologists were then with him, even in the extension of the name Silurian to the lower fossiliferous Cambrian rocks; and this was a chief source of irritation to Sedgwick. It was also, with scarcely an exception, true of geologists else-

[1] One important fact is pointed out in this paper in a letter from M'Coy, on p. 143—that the May Hill group, which Murchison had referred to the Caradoc series, really belonged by its fossils to the Upper Silurian. This point was the subject of a paper by Sedgwick in the next volume (vol. ix.) of the Journal of the Geological Society.

where. This state of opinion was partly a consequence of Murchison's early and wonderfully full description of the Silurian rocks and their fossils, which made his work a key to the Lower Palæozoic of all lands. Sedgwick's Cambrian researches and the palæontology of the region were not published in full before the years 1852–55, when appeared his "Synopsis of the Classification of the British Palæozoic Rocks," along with M'Coy's "Descriptions of British Palæozoic Fossils."

But this general acceptance was further due to the fact that the discovered fossils of the Cambrian, from the Lingula flags downward, or the "Primordial," were few, and differed not more from Silurian forms than the Silurian differed among themselves; and also, because the beds were continuous with the Silurian, without a break. Geologists under the weight of the evidence, American as well as European, naturally gravitated in the Murchisonian direction, while applauding the work of Sedgwick.

In 1853, Mr. Salter showed, by a study of the fossils (Q. J. Geol. Soc., x. 62), that the Bala beds from Bala in Merioneth, the original Bala, were included within the period of the Caradoc. Sedgwick subsequently (in the preface to the Catalogue of the Woodwardian Museum by J. W. Salter) divided his Upper Cambrian into (1) the Lower Bala, to include the Llandeilo flags (Upper Llandeilo of the Geological Survey, the Arenig being the Lower); (2) the Middle Bala, corresponding to the Caradoc sandstone, the Bala rocks, and the Coniston limestone (Geological Survey); and the Upper Bala or the Caradoc shales, Hirnant limestone, and the Lower Llandovery (cited from Etheridge, in Phillips's "Geology," ii. 77, 1885).

In 1854, the Cambrian system not having secured the place claimed for it, Sedgwick brought the subject again before the Geological Society. Besides urging his former arguments, he condemned Murchison's work so far as to imply that none of his sections "give a true notion of the geological place of the groups of Caer Caradoc and Llandeilo"; and to speak of the Llandeilo beds, in a note, as "a remarkable fossiliferous group (about the age of the Bala limestone) of which the geological place was entirely mistaken in the published sections of the Silurian System." There were errors in the sections, and that with regard to the May Hill group was a prominent one; but this was sweeping depreciation without new argument; and, in consequence of it, part of the paper was refused publication by the Geological Society.

The paper appeared in the *Philosophical Magazine* for 1854 (fourth series, vol. viii. pp. 301, 359, 481). It contains no bitter word, or personal remark against Murchison. Sedgwick was profoundly disappointed on finding, when closing up his long labours, that the Cambrian system had no place in the geology of the day. He did not see this to be the logical consequence of the facts so far as then understood. It was to him the disparagement and rejection of his faithful work; and this deeply moved him, even to estrangement from the author of the successful Silurian system.

*Conclusion.*

The ground about which there was reasonably a disputed claim was that of the Bala of Sedgwick's region and the Llandeilo and Caradoc of Murchison's. Respecting this common field, long priority in the describing and defining of the Llandeilo and Caradoc beds, both geologically and palæontologically, leaves no question as to Murchison's title. Below this level lie the rocks studied chiefly by Sedgwick; and if a dividing horizon of sufficient geological value had been found to exist, it should have been made the limit between a Cambrian and a Silurian system.

The claim of a worker to affix a name to a series of rocks first studied and defined by him cannot be disputed. But science may accept, or not, according as the name is, or is not, needed. In the progress of geology, the time finally was reached, when the name Cambrian was believed to be a necessity, and "Cambrian" and "Silurian" derived thence a right to follow one another in the geological record.

"To follow one another;" that is, directly, without a suppression of "Silurian" from the name of the lower subdivision by intruding the term "Ordovician," or any other term. For this is virtually appropriating what is claimed (though not so intended), and does marked injustice to one of the greatest of British geologists. Moreover, such an intruded term commemorates, with harsh emphasis, misjudgments and their consequences, which are better forgotten. Rather let the two names, standing together as in 1835, recall the fifteen years of friendly labours in Cambria and Siluria and the other earlier years of united research. JAMES D. DANA.

## *THE WEATHER IN JANUARY.*

THE month of January, which is generally the coldest month of the year, was so exceptionally warm this year, and in other ways the whole period was so unusual, that a few of the leading features in connection with the weather may not be without interest. The month opened with a short spell of frost, but, after the first few days, mild weather set in, and continued until the close of the month.

The stations used by the Meteorological Office in the compilation of the Daily Weather Report scarcely represent sufficiently the weather at inland stations, but yet they will give an approximate idea of the prevailing conditions. These reports show that the warmest weather was experienced in the south-western parts of the Kingdom, the stations in the north-east of Scotland being about 5° colder than in the south-west of England. On the east coast the mean temperatures of Wick, Aberdeen, Spurn Head, and Yarmouth were each about 41°·0.

The following table gives the mean temperature results for a number of stations in all parts of the British Islands:—

| Station. | Mean of max. and min. | Difference from average 15 years, 1871–1885. | Mean maximum. | Difference from average 15 years, 1871–1885. | Mean minimum. | Difference from average 15 years, 1871–1885. | Number of days with 50° and above. | Number of nights with 32° and below. |
|---|---|---|---|---|---|---|---|---|
| Wick ... ... ... ... | 40·5 | +2·8 | 45·2 | +3·0 | 35·7 | +2·7 | 4 | 8 |
| Nairn ... ... ... ... | 41·6 | +4·3 | 47·1 | +5·2 | 36·1 | +3·4 | 13 | 4 |
| Aberdeen ... ... ... | 41·1 | +3·2 | 45·6 | +3·2 | 36·5 | +3·2 | 7 | 4 |
| Leith ... ... ... ... | 42·2 | +3·0 | 48·2 | +3·6 | 36·2 | +2·5 | 15 | 9 |
| Shields ... ... ... | 42·3 | +3·4 | 47·8 | +4·7 | 36·8 | +2·1 | 14 | 5 |
| York ... ... ... ... | 41·8 | +3·6 | 47·9 | +4·7 | 35·6 | +2·5 | 15 | 8 |
| Loughborough ... ... | 42·2 | +4·0 | 48·4 | +4·9 | 36·0 | +3·1 | 17 | 6 |
| Ardrossan ... ... ... | 43·6 | +3·2 | 47·3 | +2·9 | 39·8 | +3·4 | 6 | 3 |
| Donaghadee ... ... | 42·6 | +2·2 | 47·7 | +3·3 | 37·5 | +1·2 | 15 | 2 |
| Holyhead ... ... ... | 44·7 | +2·2 | 48·7 | +2·8 | 40·7 | +1·7 | 18 | 0 |
| Liverpool ... ... ... | 43·2 | +3·4 | 48·5 | +4·6 | 37·8 | +2·2 | 16 | 4 |
| Parsonstown ... ... | 42·2 | +1·9 | 48·8 | +2·8 | 35·5 | +0·9 | 16 | 7 |
| Valencia ... ... ... | 45·6 | +0·4 | 51·1 | +1·3 | 40·0 | −0·5 | 21 | 3 |
| Roche's Point ... ... | 45·7 | +1·9 | 50·2 | +2·3 | 41·2 | +1·5 | 23 | 1 |
| Pembroke ... ... ... | 46·0 | +3·1 | 49·2 | +3·4 | 42·8 | +2·9 | 17 | 0 |
| Scilly ... ... ... ... | 48·3 | +2·1 | 51·5 | +2·4 | 45·0 | +1·7 | 25 | 0 |
| Jersey ... ... ... ... | 46·6 | +4·2 | 50·5 | +4·5 | 42·6 | +3·9 | 24 | 1 |
| Hurst Castle ... ... | 45·4 | +4·2 | 49·8 | +4·5 | 40·9 | +3·9 | 23 | 2 |
| London ... ... ... | 43·7 | +4·1 | 49·5 | +4·7 | 37·8 | +3·4 | 20 | 5 |
| Oxford ... ... ... | 42·5 | +3·4 | 48·1 | +4·3 | 36·8 | +2·4 | 15 | 4 |
| Cambridge... ... ... | 41·9 | +3·6 | 48·9 | +5·0 | 34·9 | +2·3 | 19 | 10 |
| Yarmouth ... ... ... | 40·8 | +2·6 | 45·6 | +3·7 | 36·0 | +1·5 | 6 | 7 |

From this it is seen that the excess of temperature was least at the extreme western stations, the mean at Valencia only exceeding the average for 15 years by 0°·4, whilst the night temperature was even below the average. In nearly every case it is seen that the excess of the day temperatures over the average was larger than that of the night temperatures. A feature of especial interest in the table is the large number of days on which the temperature reached 50° or above.

It is interesting to notice the very great difference between the temperature in January this year, in comparison with that which occurred in January 1881, when the weather was exceptionally cold. At Loughborough, the mean temperature this year exceeded that in 1881 by 17°, which is 4° in excess of the difference between the average temperature for January and May; there were also several stations in nearly all parts of the Kingdom with an excess of 12° and 13°.

At Greenwich Observatory the mean temperature obtained from the mean of the maximum and minimum readings was 43°·4; and with the exception of 43°·5 in 1884 and 43°·6 in 1846, this has not been exceeded in January during the last half-century. The mean of the highest day temperatures was 48°·5, which is higher than any January during the last fifty years, and the only other instances of 48°, or above, were 48°·1 in 1877 and 1851, and 48°·0 in 1846. There were six years with the mean maximum between 47° and 48°, but only eighteen in all above 45°, whilst in January 1879 the mean of the maxima was only 35°·1, or 13°·4 colder than this year, and in 1881 it was only 36°·2. There have been three Januaries during the last half-century with a higher mean night temperature, but in no year was the excess more than 1.° In January this year the mean minimum was 38°·2, and in 1884 it was 39°·2. The Greenwich observations also show that there were in January 17 days with a temperature of 50° or above, whereas in the corresponding period during the last 50 years there has been no similarly high number of days with this temperature. It was reached 14 times in 1877, 1853, and 1846; 13 times in 1873 and 1849; 12 times in 1884; 11 times in 1874, 1869, 1852, and 1851; and in 28 Januaries 50° or above was only attained 5 times or less.

The warm weather was very intimately connected with the heavy wind storms which occurred throughout the month, the storm systems which so frequently arrived on our coasts from off the Atlantic being the natural carriers of warm moist air. Scarcely a day passed during the month without the arrival of some fresh disturbance from the westward, but with one or two exceptions the central areas of the storm systems skirted the western and northern coasts and did not pass directly over our islands. The disturbances, however, passed sufficiently near to us to cause winds of gale force, and there was scarcely a day throughout the month that a gale was not blowing in some part of the United Kingdom. In the North Atlantic the month was exceptionally stormy, and vessels trading between Europe and America experienced unusually heavy weather.

The month was also marked by the prevalence of influenza, and, in addition to this, a general unhealthiness pervaded all classes of the community. The death-rate, from all causes, in London, for the four weeks ending January 25, corresponded to an annual rate of 29·7 per 1000 of the total population, which is excessively high. The rates for the corresponding period in the last four years were 21·7 in 1889, 23·2 in 1888, 22·7 in 1887, and 22·6 in 1886. CHAS. HARDING.

## NOTES.

THE subject of the Bakerian Lecture, which, as we announced last week, is to be delivered by Prof. Schuster on March 20, will be "The Discharge of Electricity through Gases."

THE Academy of Sciences of Berlin has presented the following sums of money: £90 to Dr. Rohde, of Breslau, for a journey to Naples to continue his observations on the central nervous system of sharks and echinoderms at Prof. Dohrn's zoological station; £80 to Prof. Matthiessen, of Rostock, to further his researches on the eyes of whales at the stations of the North Sea fisheries; £25 to Prof. Dr. Winkler, of Breslau, for a journey to St. Petersburg to make researches on the Turkish, Samoyed, and Tungusian languages; £30 to Dr. Schellong, the New Guinea traveller, to publish the results of his anthropological studies.

IT is proposed that the following address shall be presented to Prof. Stuart on the occasion of his resignation of his Professorship at Cambridge:—"We, the undersigned resident members of the Senate, having learned from your letter to the Vice-Chancellor your intention of resigning your Professorship in the University, desire to express our sense of the great public service which you have rendered in connection with the University Extension movement. By yourself first delivering specimen courses of lectures, and afterwards strenuously advocating and ably organizing their wide-spread establishment, you did for the country at large, and for our own and other Universities, work which we regard with sincere respect and admiration. The degree in which Cambridge has, during the last twenty years, come into useful relations with sections of the community which were previously regarded as beyond the sphere of its influence is, we hold, largely attributable to your inspiring initiative, and to the wise principles of administration which, mainly under your guidance, the University laid down."

AMONG the lectures to be delivered at the Royal Institution of Great Britain after Easter we note the following:—On Tuesdays, April 15, 22, 29, three lectures on the place of Oxford University in English history, by the Hon. George C. Brodrick; on Tuesdays, May 27, June 3, 10, three lectures on the natural history of society, by Mr. Andrew Lang; on Thursdays, April 17, 24, May 1, three lectures on the heat of the moon and stars (the Tyndall Lectures), by Mr. C. V. Boys, F.R.S.; on Thursdays, May 8, 15, 22, 29, June 5, 12, six lectures on flame and explosives, by Prof. Dewar, F.R.S.; on Saturdays, April 19, 26, May 3, three lectures on colour and its chemical action, by Captain W. de W. Abney, F.R.S.

THE De Candolle Prize has been awarded to Prof. F. Buchenau, of Bremen, for his monograph of the Juncagineæ.

A CONGRESS for Viticulture will be held in Rome from the 23rd to the 27th of the present month. The principal object of the Congress will be the discussion of remedies for the *Peronospora viticola* and other diseases of the vine caused by vegetable parasites. There will be an International Exhibition of apparatus for the cure of these diseases, and numerous prizes will be awarded.

THE annual general meeting of the members of the German Botanical Society is to be held this year in Bremen late in September.

APPENDIX I. of the *Kew Bulletin*, just issued, contains a list of such hardy herbaceous annual and perennial plants and of such trees and shrubs as matured seeds under cultivation in the Royal Gardens, Kew, during the year 1889. It is explained that these seeds are available for exchange with Colonial, Indian, and Foreign Botanic Gardens, as well as with regular correspondents of Kew. The seeds are for the most part only available in moderate quantity, and are not sold to the general public.

THE Nachtigal Gesellschaft of Berlin, for German research in Africa, has just completed its second year of business. It was announced at the last general meeting that the list of members had been doubled during the last year. The Society's library contains 200 books on Africa. Herr Schiller-Tietz was elected President of the Society in place of Councillor Engelke.

A CURIOUS phenomenon is reported from Batoum. On January 23, at 4 p.m., during a complete calm, the sea is said to have suddenly receded from the shore, leaving it bare to a depth of ten fathoms. The water of the port rushed out to sea, tearing many of the ships from their anchorage, and causing a great amount of damage. After a short time the sea assumed its usual level.

AN important addition to our knowledge of the meteorology of Central America has been made by the publication of Parts 1-4 of the *Boletin trimestral* of the National Meteorological Institute of San José, Costa Rica, for the year 1888, under the direction of Prof. E. Pittier. The Observatory is situated in latitude 9° 56′ N., longitude 84° 8′ W., and its importance may be judged from the fact that no other station of the first order possessing self-recording instruments is to be found between Mexico, in latitude 19° N., and Rio de Janeiro, in latitude 23° S. The bulletin contains observations made several times daily, and hourly observations of rainfall for five months, also a summary of the observations formerly made in Costa Rica. The older series of observations show that the mean yearly extremes of temperature at San José were 78°·8 and 56°·7, while the mean difference of the monthly means amounted only to about 4°. The daily period of rainfall is very marked. From sunrise to noon scarcely any rain falls, while between noon and 6h. p.m. about 75 per cent. of the whole amount falls. The mean duration of rain on a wet day is 2h. 9m. Only two months of anemometrical observations are given; these show that the maximum velocity at noon is twice as great as the mean velocity during the night. An interesting summary of the observations has been published by Dr. Hann in the *Meteorologische Zeitschrift* for February.

AT a recent meeting of the Paris Geographical Society an interesting lecture was delivered by Dr. Hamy, on the history of scientific missions in France under the old monarchy. He commenced practically with the reign of Francis I., and described many missions abroad, with purely scientific aims, which are now either forgotten, or the results of which have never been published. Thus, the apothecary to Henri IV. went all over the globe in search of the peculiar products of each country, especially medicinal and food plants; still earlier, another explorer went to Brazil to study dyeing woods; and, in the last century, Condamine, Dombey, Bougainville, and La Pérouse went on their well-known expeditions. The President, Comte de Bisemont, mentioned that there were still in the archives of the Ministry of Marine copies of the instructions given to travellers and navigators in past centuries, and that these were "positively models of their kind, which could not be followed too closely now." Prof. Bureau, of the Museum of Natural History in Paris, observed that a botanical collection made by Paul Lucas in the reign of Louis XIV. still existed in the Museum, and he referred especially to Tournefort, of the same period, whom he described as the scientific traveller of former times who perhaps most nearly approached moderns in his methods of observation. He was sent by the King on a botanical expedition to the Levant, with very precise instructions, amongst others, to collect and observe the plants mentioned by the ancients. He did not confine himself to this, but formed a complete herbarium, which is still preserved at the Museum, and is one of its treasures. He was accompanied by an artist named Aubriet, who brought back a large collection of coloured sketches, which forms an important part of the unrivalled collection in the library of the Museum.

A NEW and very simple method of measuring small elongations of a bar under any influence has been devised by Signor Cardani (*Cosmos*). To one end of the bar is attached a metallic wire stretched so as to give a determinate number of vibrations. When the bar expands, the wire becomes less tense, and gives fewer vibrations, and there is a simple relation between the number of vibrations and the elongation of the bar. The author cites a case in which a variation of one hundredth of a millimetre in a bar lessens the double vibrations from 99 to 96·5. Now, a practised ear will appreciate a difference of one vibration per cent.; hence it suffices to ascertain variations of length less than 0·01 millimetre. With other methods of measuring change of vibration, elongations of thousandths of a millimetre may be ascertained.

THE first careful determination of latitude in Tokio (according to the *Japan Weekly Mail*) was made in 1876 by Captain Kimotsuki, at that time Director of the Naval Observatory. In 1888, soon after the transfer of the Naval Observatory to the Imperial University, and its reorganization as the Astronomical Observatory of Tokio, the new Director, Prof. Terao, resolved upon a redetermination of the latitude. The work was entrusted to Mr. Watanabe, a skilled observer, and the result has been published as the first of the "Annales de l'Observatoire Astronomique de Tokio (Université Impériale du Japon, Collège des Sciences)." The determination was made in two distinct ways: first, by observations of the upper and lower transits of the Pole star across the meridian; second, by observations of the zenith distances of 38 different stars, arranged in couples according to Talcott's method. This latter method only was used by Captain Kimotsuki in this earlier determination. The earlier mean value for the latitude was 35° 39′ 17″·492; while the recently obtained mean values were 35° 39′ 15″·05 by the first method, and 35° 39′ 15″·41 by the second method. This discrepancy of fully 2″ is, in the circumstances, too large to be regarded as an accidental error, and must be due to some systematic error in either the earlier or the later determination. More weight will be attached to the new determination, since Mr. Watanabe had much superior instruments at his disposal.

THE stay of some 306 natives from various French colonies, &c., for about six months, in Paris last year, in connection with the Exhibition, was an interesting experiment in acclimatization. Owing to wise hygienic measures (such as vaccination, good water-supply, isolation of closets, and surveillance of food), these Annamites, Tonquinese, Senegalese, &c., seem to have escaped most of the common endemic disease. According to the *Semaine Médicale*, they had no typhoid fever, scarlatina, or measles, though these were in Paris at the time. Some 68 natives were attacked by mumps. The fatigues of a voyage and the change of climate led to a recurrence of intermittent fever, with grave symptoms, in twenty cases. It was thought at first to be typhoid fever of a severe type; but the rapid and durable efficacy of sulphate of quinine, given in doses of 2 to 3 grammes a day, proved the paludine nature of the disorder. It is noteworthy that most illnesses of this population, especially that just noticed, and those from cold, appeared during the first part of the time, when the weather was mild; while in the second period, with unfavourable atmospheric conditions, the illness diminished, whether owing to precautions in the matter of dress and food, or to more complete acclimatization. The negroes of Senegal and the Gaboon seem to have been the greatest sufferers, while the Indo-Chinese race acclimatized the best.

THE first *Bulletin* issued this year by the Académie Royale de Belgique contains a note by M. Van Beneden, on a Ziphius which was stranded in the Mediterranean, and a list of the prize subjects for 1891. The subjects dealt with are architecture, engraving, painting, and music. Four gold medals are given, having values 1000, two 800, and 600 francs respectively. The dissertations may be written in French, Flemish, or Latin, and must be sent before June 1, 1891, to M. J. Liagre, Secretary of the Academy.

A SHORT note on diethylene diamine, $C_2H_4\left<\begin{matrix}NH\\NH\end{matrix}\right>C_2H_4$, is contributed to the new number of the *Berichte* of the German Chemical Society by Dr. J. Sieber, of Breslau. It was obtained by the action of ethylene dibromide, $C_2H_4Br_2$, upon ethylene diamine, $C_2H_4\left<\begin{matrix}NH_2\\NH_2\end{matrix}\right.$, a liquid boiling at 123° C. Upon treating the product of this reaction with caustic potash, an oily liquid separated, consisting of a mixture of bases. The separated liquid was next dehydrated as completely as possible, and then submitted to fractional distillation, when the portion boiling between 168°–175° was found to consist of diethylene diamine admixed with a little water. The affinity of the base for water is, in fact, so great that it was found impossible to remove the last traces of moisture. Diethylene diamine, however, readily forms salts which can be isolated in a state of purity, and the analyses of which prove the composition of the base itself. The hydrochloride, $C_2H_4\left<\begin{matrix}NH.HCl\\NH.HCl\end{matrix}\right>C_2H_4$, crystallizes in beautiful white needles, very soluble in water, but insoluble in alcohol. The platinochloride, $C_4H_{10}N_2(HCl)_2PtCl_4$, forms fine yellow needle-shaped crystals, readily soluble in hot water, but difficultly soluble in boiling alcohol. A very beautiful salt is also formed with mercuric chloride, $C_4H_{10}N_2(HCl)_2HgCl_2$, consisting of star-like aggregates of acicular crystals, also soluble in hot water, but reprecipitated by the addition of alcohol.

DRS. WILL AND PINNOW communicate to the same journal their report upon the analysis of the remarkable meteorite of Carcote, Western Cordilleras, Chili. The great mass of this meteorite, 80 per cent., is found to consist of two silicates. One of them is readily decomposed by hydrochloric acid, and possesses the composition and optical characters of olivine, $(MgFe)_2SiO_4$. The other is unattacked by hydrochloric acid, and exhibits the chemical and crystallographical characters of a member of the diopside group. Interspersed among the silicates are smaller quantities of chrome ironstone, bronze-like sulphide of iron, probably troilite, and light steel-grey nickeliferous iron. The latter is not only found in minute particles, but also frequently in small plates which show the Widmannstadt figures in the form of an extremely fine rectangular network. Here and there are found silver-white crystals of rhabdite, one of the forms of nickeliferous iron. By far, however, the most interesting substance contained in the meteorite, is a form of crystalline elementary carbon, dull black in appearance and of extreme hardness, at least 9. It is, in fact, a variety of black diamond, and its presence in the meteorite affords considerable ground for speculation. Carbon is further present in the form of organic substances soluble in ether, and these substances carbonize upon heating, evolving the usual odour of burning organic matter. Hence this meteorite is an extremely interesting one, and forms another addition to the fast-accumulating list of those in which carbon forms a not insignificant ingredient.

## *OUR ASTRONOMICAL COLUMN.*

### OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on March 6 = 8h. 58m. 19s.

| Name. | Mag. | Colour. | R.A. 1890. | Decl. 1890. |
|---|---|---|---|---|
| | | | h. m. s. | ° ′ |
| (1) G.C. 1713 ... ... | — | — | 8 45 49 | +33 50 |
| (2) 120 Schj. ... ... | 6·5 | Reddish-yellow. | 9 4 6 | +31 26 |
| (3) α Hydræ ... ... | 3 | Yellow. | 9 22 12 | − 8 11 |
| (4) α Cancri ... ... | 4 | Yellowish-white. | 8 52 30 | +12 17 |
| (5) 124 Schj. ... ... | 5·4 | Reddish-yellow. | 9 45 59 | +22 36 |
| (6) T Monocerotis ... | Var. | Yellow. | 6 19 20 | + 7 8·7 |

*Remarks.*

(1) This bright oval nebula is now in a very convenient position for observation. I am not aware that the spectrum has been recorded. It is about 8′ long, and 3′ broad, and is thus described in the General Catalogue: "Very bright, very large, very much elongated 40°·9, gradually much brighter in the middle." The description is very suggestive of the Great Nebula in Andromeda, and if, as in that case, the spectrum at first appears continuous, closer scrutiny may reveal irregularities. The brighter parts, assuming that they exist, should be compared with the spectrum of carbon.

(2) According to the observations of D'Arrest, Secchi, and Vogel, this is a fine example of the stars of Group II. Dunér states that all the bands 1 to 10 inclusive are excessively wide and dark, and that the spectrum is totally discontinuous. The star, therefore, affords a good opportunity for further observations of the bright carbon flutings with the object of establishing the cometary character of the stars of this group. It may be remarked that the citron band of carbon need not enter into this comparison, as it will be masked by the dark fluting of manganese (band 4).

(3) A star of the solar type (Konkoly). The usual differential observations are required.

(4) A star of Group IV. (Vogel). The usual observations are required.

(5) This star has a "very fine" spectrum of the Group VI. type, notwithstanding its low altitude in our latitude (Dunér). The principal bands, 6, 9, and 10, are very dark, and the secondary bands, 4 and 5, are also well seen. Further observations, with special reference to line or other absorptions, are suggested.

(6) A maximum of this short-period variable will occur on March 8. Gore gives the period as 26·76 days, and the magnitudes at maximum and minimum as 6·2 and 7·6 respectively. There is still a little doubt with regard to its spectrum. In his spectroscopic catalogue, Vogel writes it II.*a*? III.*a*, giving the magnitude at the time of observation as 7·3. In all probability the spectrum is intermediate between Group II. and Group III., perhaps something like Aldebaran.

A. FOWLER.

THE TOTAL SOLAR ECLIPSE OF DECEMBER 22, 1889.—M. A. De La Baume Pluvinel, who was located in Royal Island, about 30 miles north of Cayenne, during this eclipse, communicated his results to the Paris Academy on the 17th ult. (*Comptes rendus*, No. 7, 1890). An examination of the photographs of the corona which were obtained leads to the conclusions that—

(1) The corona presented the same general aspect as on January 1, 1889.

(2) The extension of the corona was small, being about 18′ at the solar equator, and about 6′ at the poles, and in this respect resembled the coronæ of 1867 and 1878, thus confirming the intimate relation that exists between the intensity of extra-solar phenomena and the frequency of sun-spots.

(3) The aspect of the luminous aigrettes which constitute the corona, and notably the curved form of the aigrettes in the neighbourhood of the poles, seem to prove the existence of streams of matter submitted to two forces—a force of projection normal to the solar sphere, and a centrifugal force developed by the sun's rotation.

COMETS AND ASTEROIDS DISCOVERED IN 1889.—

*Comet a* 1889.—Discovered on January 15, a little before dawn, by Mr. W. Brooks at Geneva, N.Y., U.S.A. The comet was moving rapidly from east to west, and was not afterwards observed.

*Comet b* 1889.—Discovered by Mr. Barnard, of the Lick Observatory, on March 31; it was then very feeble and difficult to see. After perihelion passage, the comet was observed at Ann Arbor on July 22, near the position assigned to it by M. E. Millosevich.

*Comet c* 1889.—Also discovered by Mr. Barnard, on June 23, as a faint nebulosity without condensation or tail. Not observed after August 6. Dr. Berberich determined the elements of this comet on the hypothesis of an elliptic orbit, and found that its period was 128 years.

*Comet d* 1889.—This comet, the most interesting of those observed last year, was discovered by Mr. Brooks, of Geneva, U.S., on July 6. It is periodic, the time of revolution being 7·04 years. On August 1, Mr. Barnard found that the principal comet was accompanied by four companions. Mr. Chandler has found that in 1886 this comet must have approached near to Jupiter, and his investigations seem to show that it is identical with the lost comet of Lexell.

*Comet e* 1889.—Discovered by Mr. Davidson at Branscombe, Mackay (Queensland), on July 22, and visible to the naked eye at first as a star of the fourth magnitude. It moved rapidly towards the north, and at the same time diminished in brightness, remaining visible, however, up to November.

*Comet f* 1889.—Discovered by Mr. Lewis Swift at Rochester, U.S., on November 17. From observations extending over twenty days, Dr. Zelbr was led to conclude that the comet was periodic, the time of revolution being 6·91 years.

*Comet g* 1889.—Discovered by M. Borrelly at Marseille, on December 12. It was then feeble, but rapidly increased in brightness. Although the declination of this comet on discovery was + 48° 55′, it moved so quickly towards the south, that it was lost to our latitudes about January 10, 1890. The first observations fixed the perihelion passage at January 26, 1890.

Six asteroids were discovered in 1889, viz. :—

(282) Discovered by M. Charlois at Nice on January 28.
(283) ,, ,, ,, ,, February 8.
(284) ,, ,, ,, ,, May 29.
(285) ,, ,, ,, ,, August 3.
(286) ,, ,, M. J. Palisa at Vienna on August 3.
(287) ,, ,, Dr. Peters at Clinton, U.S., October 13.

MASS OF SATURN.—The Transactions of the Astronomical Observatory of Yale University, vol. i. part ii., contains some researches with the heliometer by Mr. Asaph Hall, for the determination of the orbit of Titan and the mass of Saturn.

From observations made at the oppositions of 1885–86, 1886–87, the mean value of the semi-major axis of Titan's orbit was determined as—

$$176''\cdot570 \pm 0''\cdot0243 ;$$

and the mass of Saturn—

$$\frac{1}{3500\cdot5 \pm 1\cdot44},$$

the sun being unity.

Struve showed that the value found by Bessel from Titan should be 3502·5, while the values found by Struve himself from Iapetus and Titan are respectively 3500·2 ± 0·82 and 3495·7 ±1·43. Prof. Hall, with the great Washington refractor, found from Iapetus by means of differences of right ascension and declination, the mass 3481·2 ± 0·65, and by distances and position-angles 3481·4 ± 0·97; from Titan the values corresponding to the same methods are 3496·3 ± 1·84, and 3469·9 ± 1·49, but there seem to be grounds for questioning these results, so discordant with those found by Struve, and at Yale College.

## *THE OPENING OF THE FORTH BRIDGE.*

MUCH interest was excited all over the country by the opening of the Forth Bridge on Tuesday. The ceremony was simple, and all the arrangements were carried out successfully. There was no rain, and although the wind blew stiffly, it was "comparatively mild." The special train conveying the directors and invited guests left the Waverley Station, Edinburgh, in two portions, the first at 10.45, the second, to which the Royal carriages were attached, ten minutes later. At the Forth Bridge Station Sir John Fowler, Mr. Benjamin Baker, Mr. William Arrol, Mr. Phillips, and other gentlemen connected with the building of the bridge, awaited the arrival of the Royal party from Dalmeny. By the special desire of the Prince of Wales, who wished to have an opportunity of examining some details of the structure, the Royal train steamed very slowly across the bridge. As seen from the shore, the long train of large saloon carriages is said to have looked like "a mere toy as it passed through the stupendous framework of tubes and girders at Inverkeithing." From the North Queensferry Pier the steam launch *Dolphin* conveyed the Royal party and the directors over the Firth, so that the bridge might be seen from the sea; and another vessel followed, containing the rest of the company. Both vessels steamed out to the middle of the Firth; and, according to the *Times*, the view was much enjoyed "as each cantilever was passed in succession, the junction of the girder bridges with the cantilever arms being specially noted." Afterwards, the bridge was recrossed, and in the middle of the north connecting girder the train stopped to allow the Prince of Wales to perform the ceremony of driving the last rivet. "A temporary wooden staging," says the *Times*, "had been erected there, and upon it His Royal Highness stepped, along with Lord Tweeddale, Lord Rosebery, and Mr. Arrol. The hydraulic rivetter was swung from one of the booms, the pressure being supplied from an accumulator at Inchgarvie. Two men were placed on the boom below to manipulate the machine. The gilded rivet having been placed in the bolt-hole, and the silver key having been handed to His Royal Highness by Lord Tweeddale, the Prince, with Mr. Arrol's assistance, finished the work in a few seconds, amid cheers. The rivet is in the outside of the boom, and holds together three plates. Around its gilded top there is an inscription stating that it is the 'last rivet, driven in by His Royal Highness the Prince of Wales, 4th March, 1890.' The train stopped a second time at the south great cantilever pier, where another platform had been erected, upon which several ladies were standing. Here the Prince again left the train, at half-past 1 o'clock, to make the formal declaration of the opening of the bridge. As the wind was blowing a perfect gale, so that His Royal Highness had difficulty in retaining a steady foothold, it was impossible to make a speech. He therefore simply said: 'Ladies and Gentlemen, I now declare the Forth Bridge open.' Hearty cheers greeted the announcement, and, the Prince having returned to his carriage, the train moved slowly along to the Forth Bridge Station."

At 2 o'clock a banquet was given in the model-room at the bridge works, the chair being occupied by Mr. M. W. Thompson. The Prince of Wales, responding to the toast of "The Prince of Wales and other members of the Royal Family," spoke as follows :—

"I feel very grateful for the kind words which have fallen from the chairman in proposing the toast, and I thank you all most heartily for the cordial way in which you have received it. The day has been a most interesting day to all of us, and especially so to me, and I feel very grateful that I have been asked to take part in so interesting and important a ceremony as the one at which we have all assisted. I had the advantage, nearly five and a half years ago, of seeing the Forth Bridge at its very commencement, and I always looked forward to the day when I should witness its successful accomplishment. I may perhaps say that in opening bridges I am an old hand. At the request of the Canadian Government I performed the opening ceremony 30 years ago of opening the Victoria Bridge over the St. Lawrence at Montreal, putting in the last rivet, the total of rivets being one million. To-day I have performed a similar ceremony for the Forth Bridge, but on this occasion the rivets number nearly eight millions instead of one million. The construction of the bridge has been on the cantilever principle, which has been known to the Chinese for ages, and specimens of it may be seen likewise in Japan, Tibet, and the North-West Provinces of India. Work of this description has hitherto been carried out on small dimensions, but in this case the engineers have had to construct a bridge in 30 fathoms of water, at the height of 150 feet above high water mark, and crossing two channels, each one-third of a mile in width. Had it not been for the intervening island of Inchgarvie the project would have been impracticable It may perhaps interest you if I mention a few figures in connection with the construction of the bridge. Its extreme length, including the approach viaduct, is 2765 yards, one and one-fifth of a mile, and the actual length of the cantilever portion of the bridge is one mile and 20 yards. The weight of steel in it amounts to 51,000 tons, and the extreme height of the steel structure above mean water-level is over 370 feet; above the bottom of the deepest foundation 452 feet, while the rail-level above high water is 156½ feet. Allowance has been made for contraction and expansion and for changes of temperature to the extent of one inch per 100 feet over the whole bridge. The wind-pressure provided for is 56 lb. on each square foot of area, amounting in the aggregate to about 7700 tons of lateral pressure on the cantilever portion of the bridge. About 25 acres of surface will have to be painted with three coats of paint. As I have said, about eight millions of rivets have been used in the bridge, and 42 miles of bent plates used in the tubes, about the distance between Edinburgh and Glasgow. Two million pounds have been spent on the site in building the foundations and piers; in the erection of the superstructure; on labour in the preparation of steel, granite, masonry, timber, and concrete; on tools, cranes, drills, and other machines required as plant; while about two and a half millions has been the entire cost of the structure, of which £800,000 (nearly one-third of this amount) has been expended on plant and general charges. These figures will give you some idea of the magnitude of the work, and will assist you to realize the labour and anxiety which all those connected with it must have undergone. The works were commenced in April 1883, and it is highly to the credit of everyone engaged in the operation that a structure so stupendous and so exceptional in its character should have been completed within seven years. The opening of the bridge must necessarily produce important results and changes in the railway service of the east coast of Scotland, and it will, above all, place the valuable manufacturing and mineral-producing district of Fife in immediate communication with the south side of the Firth of Forth. When the Glenfarg line, now nearly completed, is opened for traffic, the distance between Edinburgh and Perth will be reduced from 69 to 47 miles, and instead of the journey occupying, as at present, two hours and 20 minutes, an express will be able to do it in an hour. Dundee, likewise, will be brought to within 59 miles of Edinburgh, and Aberdeen 130 miles, and no sea ferries will have to be crossed. The construction of the bridge is due to the enterprise of four important railway companies—(1) North British (the bridge is in its district), (2) North-Eastern, (3) Midland, and (4) Great Northern—and the design is that of two most eminent engineers, Sir John Fowler and Mr. Benjamin Baker. The contractor was Mr. William Arrol, and the present Tay Bridge, and the bridge which I have inaugurated to-day, will be lasting monuments of his skill, resources, and energy. I have much pleasure in stating that, on the recommendation of the Prime Minister, the Queen has been pleased to create Mr. Matthew William Thompson, Chairman of the Forth Bridge Company and of the Midland Railway Company, and Sir John Fowler, engineer-in-chief of the Forth Bridge, baronets of the United Kingdom. The Queen has also created, or intends to create, Mr. Benjamin Baker, Sir John Fowler's colleague, a Knight Commander of the Order of St. Michael and St. George, and to confer on Mr. William Arrol, the contractor, the honour of a knighthood. I must not allow this opportunity to pass without mentioning the valuable assistance which has been rendered to the companies by Mr. Wieland, their able and indefatigable secretary, who deserves especial praise for the admirable way in which he has carried out the important financial arrangements essential in a scheme of such magnitude. Before concluding I must express my pleasure at seeing here Major-General Hutchinson and Major Marindin, two of the nspecting officers of the Board of Trade. Although in this country great undertakings of the kind which we are celebrating this day are wisely wholly left to the enterprise and genius of private individuals without aid or favour from the State; yet, in connection with these particular works, Parliament, I am informed, for the first time associated officers of the Board of Trade with those practically engaged in the construction of this magnificent bridge from its commencement by requiring the Board of Trade to make quarterly reports to be laid before Parliament as to the nature and progress of the works. This most important and delicate duty has been performed by Major General Hutchinson and Major Marindin; and I now congratulate them on the completion of their responsible duties, which they have carried out in a way that redounds credit to themselves and to the department which they so ably serve. Allow me again, gentlemen, in thanking you for the kind way in which you have received this toast, to assure you of the great pleasure and gratification it has been to me to have been present on this occasion to inaugurate this great success of the skill of engineering."

Sir John Fowler, in acknowledging the toast of the Forth Bridge, said he begged to return his most grateful thanks to His Royal Highness the Prince of Wales for the flattering manner in which he had spoken of their work. It was now seven years ago since the foundations of the bridge were commenced, but up to two years ago they had to endure not only the legitimate anxieties of their duties, but the attacks and evil predictions which were always directed against those who undertook engineering work of novelty or exceptional magnitude. It was very curious to watch the manner of retreat of these prophets of failure. The results had proved them to be mistaken. But he could tell some very curious stories connected with the bridge. He pointed out how, from the nature of the materials which had been used in the construction of the bridge, and from the nationality of the men who had been engaged in that construction, the bridge possessed an international character. He also predicted that the bridge would last for many, many years, and he cordially acknowledged the workmanship and ability of all who had assisted in its erection. As to the workmen themselves, he said they had done admirable work, and had never knowingly scamped a rivet.

Mr. Arrol also acknowledged the toast, and Mr. Baker, in response to calls from the audience, made a few remarks.

Mr. John Dent, Deputy-Chairman of the Forth Bridge Railway Company, in proposing the toast of "The Guests," congratulated the recipients of the special honours bestowed by the Queen, and he spoke of the universal reputation which had become attached to the bridge, which stood as a monument of industry, of genius, and of ability.

After a clever speech from Lord Rosebery, Herr Mehrtens, of the Prussian Railway Department, replied for himself and in the name of his companions from Saxony, Austria, and Hungary. He expressed their feelings of thankfulness that they had been permitted to be present on so interesting an occasion, and their admiration at all the wonderful things they had seen that day. That day, he said, marked the commencement of a new era in iron bridge building. He congratulated Great Britain, which had led the way in iron bridge building, on now having the largest span bridge and the strongest bridge in the world.

M. Picot, on behalf of the railway engineers of France, also replied in a speech in which he eulogized the bridge and its engineers and contractors.

## *UNIVERSITY AND EDUCATIONAL INTELLIGENCE.*

CAMBRIDGE.—The General Board of Studies announce that they will this term appoint an additional Lecturer in Botany for three years, from the beginning of the Easter term 1890. The stipend is £100 a year. Names of candidates are to be sent to the Vice-Chancellor on or before March 8.

The Syndics of the Press propose that a gift of books published by them shall be made to the Library of the University of Toronto, lately destroyed by fire.

The discussion by the Senate of the proposal to accept the Newall telescope was on the whole favourable to the proposal, though the difficulty of finding the funds required for its adequate maintenance and use has not yet been made. From remarks made by members of the Observatory Syndicate, it appears that it regards the purchase of a large reflecting telescope as the first claim on the Sheepshanks Fund; and it is unwilling to deplete the fund until this purchase can be effected. Prof. Liveing referred to the recent development of astronomical physics, and said the University was bound to further it. The Newall telescope was specially suited for physical researches, and to reject it as a "white elephant" would damage the University by discouraging other benefactors. The matter is to be referred to the Financial Board.

At the meeting of the Philosophical Society on March 10, the following papers are promised:—W. Gardiner, on the germination of *Acacia sphærocephala*; M. C. Potter, the thickening of the stem in Cucurbitaceæ; Dr. Lea and W. L. Dickinson, note on the action of rennin and fibrin-ferment; W. Bateson, on some skulls of Egyptian mummified cats.

## *SOCIETIES AND ACADEMIES.*

### LONDON.

**Royal Society**, February 20.—"A Comparative Study of Natural and Artificial Digestions" (Preliminary Account). By A. Sheridan Lea, Sc.D., Fellow of Gonville and Caius College, Cambridge, University Lecturer in Physiology. Communicated by Prof. Michael Foster, Sec. R.S.

The objects of the investigation were (i.) to obtain in artificial digestions some closer approximation to the general conditions under which natural digestion is carried on in the body, and (ii.) to apply the improved methods of carrying on artificial digestions to the elucidation of some special differences, which so far have appeared to exist between the natural and artificial processes.

An apparatus was described by means of which digestions can be carried on in a dialyzer in such a way as to provide for the constant motion of the digesting mixture and the removal of digestive products: by this method a partial reproduction of two of the most important factors in natural digestion is provided.

So far the method has been employed for

I. *The salivary digestion of starch.* Experiments conducted under otherwise similar conditions in the dialyzing digester and a flask, showed that—(i.) The *rate* of digestion in the former is always greater than in a flask, and at the same time the tendency to the development of bacteria is greatly lessened. (ii.) The amount of starch converted into sugar is always greatest in the dialyzer. (iii.) The total sugar formed and small residue (4·29 per cent.) of dextrin left during an active and prolonged digestion in the dialyzer justify the assumption that, under the more favourable conditions existing in the body, the whole of the starch taken is converted into sugar before absorption.

The above results afford an explanation of the existing discordant statements as to the nature and amount of products formed during starch digestion.

II. *The tryptic digestion of proteids.* The experiments made dealt chiefly with the formation of leucin and tyrosin, and were undertaken, initially, in order to find out why these crystalline products are formed in large amount during an artificial digestion, while they have so far been described as occurring in mere traces during natural digestion. The results of the experiments made it probable that leucin and tyrosin should be formed during natural digestion. Examination of the contents of the small intestine during proteid digestion showed that, contrary to existing statements, leucin and tyrosin are formed in not inconsiderable quantities during the natural process.

The last part of the communication dealt with the probable physiological importance of the formation of amidated bodies during tryptic digestion, and a view was put forward as to the possible and probable importance of amides in the chemical cycle of animal metabolism.

The experiments are being extended to the pancreatic digestion of starch.

**Linnean Society**, February 20.—W. Carruthers, F.R.S., President, in the chair.—Mr. G. C. Druce exhibited specimens of *Agrostis canina*, var. *Scotica*, and a small collection of flowering plants dried after treatment with sulphurous acid and alcohol, and showing a partial preservation of the natural colours of the flowers.—Mr. F. P. Pascoe exhibited a series of Coleopterous and Lepidopterous insects to show the great diversity between insects of the same family.—The Right Hon. Sir John Lubbock, Bart., M.P., P.C., then gave an abstract of four memoirs which he had prepared: (1) on the fruit and seed of the Juglandiæ; (2) on the shape of the oak-leaf; (3) on the leaves of Viburnum; and (4) on the presence and functions of stipules. An interesting discussion followed, in which Mr. J. G. Baker, Mr. John Fraser, Mr. D. Morris, and Prof. Marshall Ward took part.

### Edinburgh.

**Royal Society**, February 17.—Sir W. Thomson, President, in the chair.—Prof. Crum Brown communicated a paper, by Mr. Tolver Preston, on Descartes' idea of space and Sir W. Thomson's theory of extended matter.—The following communications from the chemical laboratory of the University were read:—(*a*) Prof. Crum Brown, on a new synthesis of dibasic organic acids. The method proposed was the electrolysis of potassium ethyl salts of lower dibasic acids which would take place according to the scheme

$$2EtOOC.R''.COOK = EtOOC.R''.R''.COOEt + 2CO_2 + 2K,$$

thus giving the diethyl ether of a higher acid of the same series. (*b*) Prof. Crum Brown and Dr. James Walker, on the electrolysis of potassium ethyl malonate, and potassium ethyl succinate. The reaction actually takes place in great measure in the above indicated sense, the yields of pure succinic ether and of adipic ether respectively being from 20 to 30 per cent. of the theoretically obtainable quantities. The method is thus proved to be of practical as well as of theoretical importance. (*c*) Dr. John Gibson, on the action of bromine and carbonate of soda in solutions of cobalt and nickel salts.—Mr. W. Calderwood read a paper on the swimming bladder and flying powers of *Dactylopterus volitans*.

### Paris.

**Academy of Sciences**, February 24.—M. Hermite in the chair.—The proofs of the separation of the south-east extremity of the Asiatic continent during recent times, by M. Émile Blanchard. The author advances proofs from the resemblance of animal and vegetable life in Further India, on the peninsula of Malacca, and Sunda Islands.—The *Dryopithecus*, by M. Albert Gaudry. The relation of *Dryopithecus* to the ape and to man has been investigated.—A contribution to the chemical study of the truffle, by M. Ad. Chatin. The researches have been directed to the quantitative determination of the organic and other matter in truffles.—Scrotal pneumocèles, by M. Verneuil.—On the anatomy and the physiological pathology of the retention of urine, by M. F. Guyon.—Transformations in kinematic geometry, by M. A. Mannheim.—On the constitution of the line spectra of elements, by M. J. R. Rydberg. This is a note on the periodic recurrence of doubles and triplets in the spectrum of an element. It is shown how this periodicity enables the spectrum of an element to be found by interpolation when the spectra of elements of the same group are known, the case of gallium being given as an example of the verification of the principle.—Electrical oscillations in rarefied air, without electrodes; demonstration of the non-conductivity of the vacuum, by M. James Moser. It is well known that vacuum-tubes become luminous when near an induction coil in action. The author, by enveloping one vacuum-tube with another, in which the rarefaction could be varied, finds that the excitation may take place without any electrode. If the pressure in the outer tube be equal to 760 mm., the inner tube, under the influence of the coil, becomes luminous and of a clear blue colour; if, however, the pressure be diminished to 1 mm. of mercury, the air in the outer tube becomes luminous and of a pronounced red colour, thus reversing the phenomena.—Upon the variation, with the temperature, of the bi-refractions of quartz, barytes, and kyanite, by MM. Er. Mallard and H. Le Chatelier. This variation has been studied by the aid of a photographic spectroscopic method: with quartz a singular point is detected at 570°, at which temperature the law of variation suddenly changes; a similar phenomenon is indicated as occurring in the case of kyanite somewhere between 300° and 600°.—The vapour-pressure of acetic acid solutions, by MM. F. M. Raoult and A. Recoura. It has been previously shown by one of the authors (*Comptes rendus*, May 23, 1887; *Annales de Chimie et de Physique*, 6th series, t. xv., 1888) that, if $f$ represents the vapour-tension of a solvent for a certain temperature, $f'$ the vapour-tension under similar conditions when a non-volatile body is in solution, P the weight of substance dissolved in 100 grms. of the solvent, M the molecular weight of the dissolved body, and M' the molecular weight of the solvent, then for dilute solutions—

$$K = \frac{100(f - f')}{f' P} \cdot \frac{M}{M'},$$

K being a constant generally near to unity. Employing the dynamical method, the mean value of K for acetic acid is found to be 1·61, taking 60 as the molecular weight of acetic acid; but if the molecular weight of a liquid be the same as that of the saturated vapour, the apparent anomaly disappears, for with molecular weight 97 (deduced from density of saturated acetic acid vapour at 118°, viz. 3·35), the above formula gives K = 1.—The action, in the dry way, of various arseniates of potassium and sodium upon the oxides of the magnesia series, by M. C. Lefèvre.—Note on the volumetric estimation of copper, by MM. A. Etard and P. Lebeau. A method of titration is given by the authors, for which they claim a rapidity and accuracy comparable to the permanganate method for iron; it is based upon the formation of a characteristic violet coloration on the addition of concentrated hydrobromic acid to a solution of the copper salt, and the subsequent decoloration of the solution by standardized stannous chloride solution containing much hydrochloric acid; thus—

$$\underbrace{2CuBr_2 + nHBr}_{\text{Coloured.}} + SnBr_2 = SnBr_4 + \underbrace{Cu_2Br_2 + nHBr}_{\text{Uncoloured.}}$$

—Preparation of hydroxycamphocarbonic acid from camphocarbonic acid, by MM. A. Haller and Minguin.—Upon the organization of left-handed Prosobranchiate Gastropoda (*Neptunea contraria*, Linnæus), by MM. P. Fischer and E. L. Bouvier.—

Upon the initial cells of the ovary in fresh-water Hydræ, by M. Joannes Chatin.—Note on a new putrefaction ptomaine, obtained by the culture of *Bacterium allii*, by Mr. A. B. Griffiths. The author gives analyses of an alkaloid, produced by the decomposition of albuminoids by this organism, showing it to belong to the hydropyridine series, and to possess the formula of hydrocoridine, $C_{10}H_{17}N$.—On the chromogenous functions of the pyocyanic bacillus, by M. C. Gessard.—Fossil Radiolarians inclosed in albite crystals, by M. A. Issel. The author concludes from the data given—(1) that a sedimentary fossiliferous rock has become crystalline and rich in plagioclastic crystals, without the stratification being sensibly altered; (2) that this change has been produced in a Tertiary formation; (3) that a hydrothermal action is indicated.—A contribution to the history of chrome-iron, by M. Stanislas Meunier.

## DIARY OF SOCIETIES.

### London.

*THURSDAY*, March 6.

Royal Society, at 4.30.—On a Second Case of the Occurrence of Silver in Volcanic Dust—namely, in that thrown out in the Eruption of Tunguragua, in the Andes of Ecuador, January 11, 1886: Prof. J. W. Mallet, F.R.S.—On the Tension of Recently-formed Liquid Surfaces: Lord Rayleigh, Sec.R.S.—(1) On the Development of the Ciliary or Motor Oculi Ganglion; (2) The Cranial Nerves of the Torpedo (Preliminary Note): Prof. J. C. Ewart.

Linnean Society, at 8.—On the Production of Seed in some Varieties of the Common Sugar-Cane (Saccharum officinarum): D. Morris.—An Investigation into the True Nature of Callus; Part 1, the Vegetable Marrow, and Ballia callitricha: Spencer Moore.

Royal Institution, at 3.—The Early Developments of the Forms of Instrumental Music: Frederick Niecks.

*FRIDAY*, March 7.

Physical Society, at 5.—On Bertrand's Refractometer: Prof. S. P. Thompson.

Geologists' Association, at 8.—On some Pleistocene (non-Marine) Mollusca of the London District: B. B. Woodward.—Notes on some Pleistocene Sections, in and near London: W. J. Lewis Abbott.—Note on a Curious Appearance produced by the Natural Bisection of some Spherical Concretions in a Yoredale Stone Quarry near Leek: Dr. Wheelton Hind.

Institution of Civil Engineers, at 7.30.—Telephonic Switching: C. H. Wordingham.

Royal Institution, at 9.—Electrical Relations of the Brain and Spinal Cord: Francis Gotch.

*SATURDAY*, March 8.

Royal Botanic Society, at 3.45.

Royal Institution, at 3.—Electricity and Magnetism: Right Hon Lord Rayleigh, F.R.S.

*SUNDAY*, March 9.

Sunday Lecture Society, at 4.—Pasteur, and his Discoveries (with Oxyhydrogen Lantern Illustrations): Sir Henry E. Roscoe, M.P., F.R.S.

*MONDAY*, March 10.

Royal Geographical Society, at 8.30.—On Lieut. H. B. Vaughan's Recent Journey in Eastern Persia: Major-General Sir Frederic J. Goldsmid, K.C.S.I.

Victoria Institute, at 8.—On the Monism, Pantheism, and Dualism of Brahmanical and Zoroastrian Philosophers: Sir M. Monier-Williams, K.C.I.E.

*TUESDAY*, March 11.

Society of Arts, at 8.—The Claims of the British School of Painting to a Thorough Representation in the National Gallery: James Orrock.

Anthropological Institute, at 8.30.—Exhibition of the Skull of a Carib, from a Cave in Jamaica: Prof. Flower, C.B., F.R.S.—Manners, Customs, Superstitions, and Religions of South African Tribes: Rev. James Macdonald.

Institution of Civil Engineers, at 8.—The Hawksbury Bridge, New South Wales: C. O. Burge.—The Erection of the Dufferin Bridge over the Ganges at Benares: F. T. G. Walton.—The New Blackfriars Bridge on the London, Chatham, and Dover Railway: G. E. W. Cruttwell. (Discussion.)

Royal Institution, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

*WEDNESDAY*, March 12.

Geological Society, at 8.—On a Deep Channel of Drift in the Valley of the Cam, Essex: W. Whitaker, F.R.S.—On the Monian and Basal Cambrian Rocks of Shropshire: Prof. J. F. Blake.—On a Crocodilian Jaw from the Oxford Clay of Peterborough: R. Lydekker.—On Two New Species of Labyrinthodonts: R. Lydekker.

Society of Arts, at 8.—The Chemin de Fer Glissant, or Sliding Railway: Sir Douglas Galton, K.C.B., F.R.S.

*THURSDAY*, March 13.

Royal Society, at 4.30.

Mathematical Society, at 8.—Some Groups of Circles connected with Three given Circles: R. Lachlan.—Perfect Numbers: Major P. A. MacMahon, R.A.

Society of Arts, at 5.—Agriculture and the State in India: W. R. Robertson.

Institution of Electrical Engineers, at 8.—The Theory of Armature Reactions in Dynamos and Motors: James Swinburne.—Some Points in Dynamo and Motor Design: W. B. Esson. (Discussion.)

Royal Institution, at 3.—The Early Development of the Forms of Instrumental Music (with Musical Illustrations): Frederick Niecks.

*FRIDAY*, March 14.

Royal Astronomical Society, at 8.

Royal Institution, at 9.—The Glow of Phosphorus: Prof. T. E. Thorpe, F.R.S.

*SATURDAY*, March 15.

Society of Arts, at 3.—The Atmosphere: Prof. Vivian Lewes.

Royal Institution, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

## BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Prodromus Faunæ Mediterraneæ, Part 2: J. V. Carus (Stuttgart, Koch).—The Elements of Laboratory Work: A. G. Earl (Longmans).—History of Botany (1530–1860): J. von Sachs; translated by H. E. F. Garnsey; revised by I. B. Balfour (Clarendon Press).—Traité Encyclopédique de Photographie, neuv. fasc.: C. Fabre (Paris, Gauthier-Villars).—A Syllabus of Elementary Dynamics: Prof. W. N. Stocker (Macmillan).—Synoptical Tables of Inorganic and Organic Chemistry: C. J. Leaper (Gill).—The Growth of Capital: R. Giffen (Bell).—Coal Gas as a Fuel: T. Fletcher (Warrington, Mackie).—The Zoological Record for 1888 (Gurney and Jackson).—An Elementary Treatise on Light and Heat, 2nd edition: Rev. F. W. Aveling (Relfe).—Demoids: J. B. Sutton (Baillière).—The Railways of Scotland: W. M. Ackworth (Murray).—Electrical Engineering: W. Slingo and A. Brooker (Longmans).—Un Viaggio a Nias: E. Modigliani (Milano, Fratelli Treves).—Transactions of the Astronomical Observatory of Yale University, vol. i. Part 2 (New Haven).—Cycles of Drought and Good Seasons in South Africa: D. E. Hutchins (Wesley).—How to Know Grapes by the Leaves: A. N. M'Alpine (Edinburgh, Douglas).—Boilers, Marine and Land, 2nd edition: T. W. Traill (Griffin).—Four-Figure Mathematical Tables, 2nd edition: J. T. Bottomley (Macmillan).—The Cultivated Oranges and Lemons, &c., of India and Ceylon, text and plates: Dr. E. Bonavia (Allen).—Elementary Manual of Magnetism and Electricity, Part 2: Prof. Jamieson (Griffin).—Quarterly Journal of Microscopical Science, February (Churchill).—Zeitschrift für Wissenschaftliche Zoologie, 49 Band, 3 Heft (Williams and Norgate).—Journal of the Royal Microscopical Society, 1889, Part 6*a*, 1890, Part 1 (Williams and Norgate).—Studies from the Biological Laboratory, Johns Hopkins University, vol. 4, No. 6 (Baltimore).—Transactions and Proceedings of the Botanical Society, vol. xvii. Part 3 (Edinburgh).—Annual Report of the Canadian Institute, Session 1888–89 (Toronto).

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