THURSDAY, MARCH 5, 1885

ORE DEPOSITS

A Treatise on Ore Deposits. By J. Arthur Phillips, F.R.S., &c. Demy 8vo, pp. 624 and Index; 95 Woodcuts and 1 Plate. (London: Macmillan and Co., 1884.)

A WORK in the English language upon ore deposits has long been wanted, and geologists and mining engineers may be congratulated that one so well qualified to do justice to the subject as Mr. Phillips, undertook the laborious task of writing a general treatise.

Mr. Phillips divides his book into two parts; Part I., occupying one-sixth of the volume, treats of ore deposits in general, and Part II. is devoted to a description of the principal metal-mining regions of the world.

We are glad to see that he admits a wide definition of the term "ore:" "Although perhaps not strictly correct, any material obtained by mining that contains a workable proportion of a metal is often called an ore, even if the whole of the metal be present in the native state." This is a common-sense and practical way of dealing with the question.

The classification of ore deposits is in the main the same as that adopted by Whitney, in his "Metallic Wealth of the United States," thirty years ago. Mr. Phillips divides metalliferous deposits into the following groups:—

I. SUPERFICIAL

a. Deposits formed by the mechanical action of water.
b. Deposits resulting from chemical action.

II. STRATIFIED

a. Deposits constituting the bulk of metalliferous beds formed by precipitation from aqueous solutions.
b. Beds originally deposited from solution, but subsequently altered by metamorphism.
c. Ores disseminated through sedimentary beds, in which they have been chemically deposited.

III. UNSTRATIFIED

a. True veins.
b. Segregated veins.
c. Gash veins.
d. Impregnations.
e. Stockworks.
f. Fahlbands.
g. Contact deposits.
h. Chambers or pockets.

This classification is the only blemish of any importance in the volume, and we greatly regret that the author did not cast off the trammels of tradition and strike out a new line for himself.

In the first place, we are disposed to quarrel with the separation of superficial deposits, as described by Mr. Phillips, from stratified deposits; we do not see how they can be logically separated. Speaking of the old auriferous gravels of the Sierra Nevada of California, the author's words are:—"These, which are sometimes known as *blue gravels*, were formerly believed to be of marine origin, but are now recognised as materials brought down by the agency of currents of fresh water from the mountains high above them and deposited, either in the beds of ancient rivers, or in lake-like expansions of such streams." Surely, therefore, they are stratified. Furthermore, the term "superficial" should have been avoided as misleading, because the student will naturally infer that the chief characteristic of such deposits is that they occur at the surface; but when we find gold- and tin-bearing gravels buried at a depth of more than 100 feet under other strata and lava-flows of Pliocene or Miocene age, and worked as true mines, the word "superficial" seems singularly inappropriate. The author admits that the term "might at first sight appear a misnomer," but defends it on the ground that "a volcanic capping is by no means universal, and the uncovered beds of this age are of the greatest importance to the miner." When Whitney's book was written the name was more in harmony with the facts, as the deep leads had not been discovered; but even then there was nothing to justify the separation of the old alluvia from the class of stratified deposits.

The three subdivisions of the stratified deposits are useful for impressing upon the mind the most important varieties of this class; on the other hand, when we come to the unstratified deposits we consider that the author has been unwisely conservative.

Why should the geologist step in and call certain mineral veins "true," and thereby cast a sort of stigma upon others which do not fit in with his preconceived theories? Mr. Phillips, like most authors, uses the word "true vein" as synonymous with "fissure vein"; but as it appears that many of the sheet-like mineral masses called "lodes" or "reefs" by miners are not filled up cracks, it seems a pity that the worship of the "fissure vein" should be continued.

A useful summary is given of the various hypotheses which have been propounded concerning the genesis of mineral veins, and due attention is paid to the discoveries of Prof. Sandberger, which have an important bearing upon the theory of lateral secretion. The author ultimately concludes that both lateral secretion and the ascension of mineral-bearing waters have contributed to the filling up of fissures with the various minerals now constituting the veins.

Though he keeps up the old subdivision of "segregated veins" as distinct from true veins, Mr. Phillips is justly doubtful whether such a distinction can always be logically maintained.

With reference to the "impregnations," it is probable that some of the most important Cornish tin lodes in granite may be classed under this head, and therefore this subdivision might be made to include a good deal more than the *carbonas*.

The word "Stockwork" is unfortunately consecrated by long usage, and it will not be easy to evict it from mining literature; but it is a pity that no English term has been coined to denote this mode of occurrence. Speaking of Polberrow mine, Mr. Phillips says it "appears to be the only stockwork ever extensively worked in clay slate." This is not the case. The three openworks in Cornwall known as Minear Downs, Mulberry and Wheal Prosper are other instances of stockworks in clay slate. They produced 203 tons of dressed tin ore in 1883, and the excavations are certainly sufficiently large to say that the deposits have been "extensively worked." Henwood's account of Wheal Music (*Trans. Roy. Geol. Soc. Cornwall*, vol. v. p. 98) shows that it was a stockwork in clay slate, worked for copper ore.

It seems a mistake to retain the "fahlbands" among the unstratified deposits. They are pyritiferous beds among metamorphic rocks. At Kongsberg they constitute the *congenial country* and not the deposit worked; whilst the Skutterud fahlbands, which are simply cobaltiferous quartzite and mica schist, deserve a place among the stratified deposits quite as much as the magnetite of Arendal or Philipstad.

In Part II. we consider that Mr. Phillips does good service by giving statistics of the production of ores, in addition to the descriptions of their modes of occurrence. As stated by him in the preface, "This appears to be the only way of accurately expressing the relative importance of different metalliferous regions." This feature of Mr. Phillips's book, apart from anything else, at once renders it more valuable than the works of von Cotta, Grimm, and von Groddeck.

The United Kingdom is so rich in minerals that a large amount of space is very fairly allotted to it, and, though Cornwall receives the lion's share of attention, it must be recollected that it is the birthplace of British mining and the school from which a set of hardy and intelligent miners have been dispersed among all parts of the globe.

Speaking of an issue of carbonic acid gas from the lode at Foxdale Mine in the Isle of Man, the author says (p. 212): "At the present time (1883) in the eastern end of the 185-fathom level, the amount of gas is so large that, although volumes of compressed air are continually being poured in from two air-pipes, the men experience the greatest difficulty in working; and, as candles will not burn, the value of the end can only be determined by the ore brought out." This account is somewhat overdrawn. The gas has been troublesome at times, but not to the extent stated, for we are led to infer that the men worked in the dark. Even a Manxman is scarcely capable of driving levels without a light.

The small value of the metalliferous ores raised in France is remarkable, and the prosperity of the Belgian metal mines appears to be on the wane, as the value of the metalliferous minerals decreased from 563,080*l.* in 1872 to 148,720*l.* in 1881.

The famous mines of the German Empire at Commern, in the Upper and Lower Harz, the Mansfeld district and the Erzgebirge are described at as great a length as the space available in a general treatise will admit, and many interesting and important details are given concerning the mines of Austria, Hungary, Italy, Greece, Scandinavia, Spain, and Russia. The statement, "Spain takes the lead of all other countries in the amounts of lead and quicksilver which it produces," is scarcely accurate, unless Mr. Phillips is referring solely to Europe. The United States are now the greatest producers of lead, and the Californian quicksilver mines have for several years surpassed those of Almaden in productiveness. However, as far as the output of quicksilver last year is concerned, Mr. Phillips is doubtless correct, for statistics published within the last few weeks show that the yield of California in 1884 was only 1089 statute tons, which is less than the average amount produced by Spain.

The account of the metalliferous minerals of the Australasian colonies will be read with interest. Though the output of gold is on the whole decreasing, tin ore has within the last ten years become a great source of wealth. An important discovery is that there are *deep leads*, *i.e.* old tin-bearing alluvia, of Miocene age, and the figure representing the deposit worked by Wesley Brothers at Vegetable Creek, New South Wales, gives a good idea of this mode of occurrence. It is startling to learn that Queensland produced 106,488 tons of tin ore, worth 2,168,790*l.* in 1881; unfortunately for the colony, but luckily for Cornwall, the output of the following year was only 27,312 tons.

It was certainly no easy task for Mr. Phillips to compress into 65 pages an account of the principal metalliferous regions of the United States: but he has succeeded in furnishing a very useful *résumé*, the only fault of which is the meagreness with which it has been illustrated. The metal mines of the United States deserve more than seven woodcuts, and we should like to have seen figures to explain the wonderful deposits at Leadville and on the shores of Lake Superior.

It is to be regretted that apparently there is so little available information concerning the mines of Mexico, a country so highly favoured as far as mineral wealth is concerned. South America, too, has to be treated very summarily.

Excepting for having followed a beaten track in his classification, the author deserves much praise for his work. The descriptions of the metal-mining districts are very good, being based upon personal knowledge and the latest published accounts; both Mr. Phillips and his assistant, Mr. Brough, must be commended for the care with which they have ransacked all sorts of British and foreign publications relating to mining. The references are very full and complete, and much vigilance has been exercised in correcting for the press. Finally, we must congratulate the author upon his excellent index, occupying no less than twenty-five closely-printed pages. This adds greatly to the utility of the book, which will doubtless become the standard work upon ore deposits.

OUR BOOK SHELF

Madagascar and France; with some Account of the Island, its People, its Resources, and Development. By George A. Shaw, F.Z.S. (London: Religious Tract Society, 1885.)

THE incident connected with Mr. Shaw's imprisonment on board a French war-ship at Tamatave will be remembered—an incident for which the French Government had to make substantial amends. Mr. Shaw has been a missionary in Madagascar for many years, and has thus had ample opportunity of gaining a knowledge of the interesting island. To those familiar with the literature of Madagascar the volume will not present much of novelty; it is, however, interesting reading, and contains some of the results of Mr. Shaw's own observations. On the physical geography and ethnology of this country there is nothing new, but Mr. Shaw presents the results of previous investigations clearly and briefly. He in the main adopts the generally-accepted conclusions as to the Malay origin of the bulk of the Malagasy people, though we suspect that the aboriginal Vazimba are greater, and the intercourse between the mainland and the island of much older date than he is prepared to admit. He gives many interesting details as to the industries of the people, their social habits, the progress of Christianity and education, the past history of the island, and other points. A large portion of the volume is occupied with the history of the relations between France and Madagascar, in which he tells the story of his own imprisonment. To the scientific reader the concluding chapters on the fauna, flora, and meteorology of the island will prove useful; they summarise what is already known, with some additional facts obtained by the observation of himself and his brother missionaries. There is a map and a few good illustrations.

Three Months in the Soudan. By Ernestine Sartorius. (London: Kegan Paul and Co., 1885.)

MRS. SARTORIUS spent most of her three months in 1883-84 at Suakim, of which her husband, Gen. Sartorius, was Commandant. Her book deals chiefly with the events which culminated in the disaster of El-Teb. It is mostly a pleasant, gossipy record of the daily life of the town, and of the alarms created by the attempted raids of the rebellious natives in the district around. It affords a good idea of the character of the town and its immediate surroundings.

Lectures on Agricultural Science and other Proceedings of the Institute of Agriculture, South Kensington, London, 1883-84. (London: Chapman and Hall.)

THIS volume contains abstracts of lectures delivered by a considerable number of well-known authorities upon agricultural matters. Mr. Carruthers and the late Prof. Buckman give their experiences upon grasses and farm seeds; Prof. Wrightson has a paper upon land drainings; dairy management and farm crops are treated of by Professors Huldon and Fream and Mr. Bernard Dyer; Mr. Henry Woods contributes lectures upon Southdown sheep and ensilage; while Mr. Warrington has a contribution upon the nitrogenous matter in soils; and Mr. Worthington Smith gives some good observations upon corn mildews. The names of the authors of the various lectures are a sufficient guarantee of their soundness and worth.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Sir William Thomson's Baltimore Lectures

As it is possible that some of your readers may have obtained copies of the Papyrograph Report of my Lectures on "Molecular Dynamics," delivered at Baltimore during October 1884, I should be obliged by your giving publicity to the following corrections:—

Page 34, lines 18 and 19.—Delete "we may call it a dynamox but not a paradox." I have no recollection nor can I imagine what the word was that I suggested as more logical than "paradox"!

Page 59, line 14.—For "Distortional" substitute "Condensational."

Page 296.—In the two expressions for ψ, given in equation (17), insert "tan i" before "$\frac{(\mu^2-1)^2}{\mu^2+1}$"; also, in the expressions for "tan e" and "tan e_1" of equation (20) insert "tan i" before "$\frac{(\mu^2-1)^2}{\mu^2+1}$".

The formula from which these expressions are deduced is correctly given at the foot of page 295.

Page 296.—In line 13 from the top of the page, and in the left hand members of equations (19) and (21), for "ω" and "ω_1" read "$\bar{\omega}$" and "$\bar{\omega}_1$" respectively.

WILLIAM THOMSON

The University, Glasgow, February 27

Civilisation and Eyesight

IT would take too much of your space to discuss at length the theoretical limit of resolving-power as depending upon aperture. The reader may be referred to some papers in the *Philosophical Magazine* for 1879 and 1880, where he will also find references to the work of other investigators. I will only say that (as indicated by the word *fairly* in my statement) resolution admits of various degrees. Doubtless a practised observer would judge a star to be double whose components subtend a decidedly smaller angle than two minutes, but he would not see them separated. I purposely rather understated the case. The higher the visual power of civilised men, the less room is there for savages with eyes of equal aperture to surpass them.

With respect to my short-sightedness in a bad light, I shall be glad if you will publish the accompanying two short papers from the *Cambridge Philosophical Proceedings*. They will show how I was led to make the discovery.

RAYLEIGH

"The Use of Telescopes on Dark Nights." By Lord Rayleigh. (From the *Camb. Phil. Proc.*, March, 1882.)

In *Silliman's Journal* for 1881 Mr. E. S. Holden, after quoting observations to a like effect by Sir W. Herschel, gives details of some observations recently made with a large telescope at the Washburn Observatory, from which it appears that distant objects on a dark but clear night can be seen with the telescope long after they have ceased to be visible with the naked eye. He concludes, "it appears to me that this confirmation of Herschel's experiments is important, and worth the attention of physicists. So far as I know there is no satisfactory explanation of the action of the ordinary night-glass, nor of the similar effect when large apertures are used."

It is a well-known principle that no optical combination can increase what is called the "apparent brightness" of a distant object, and indeed that in consequence of the inevitable loss of light by absorption and reflection the "apparent brightness" is necessarily *diminished* by every form of telescope. Having full confidence in this principle, I was precluded from seeking the explanation of the advantage in any peculiar action of the telescope, but was driven to the conclusion that the question was one of apparent magnitude only,—that a large area of given small "apparent brightness" must be visible against a dark ground when a small area would not be visible. The experiment was tried in the simplest possible manner by cutting crosses of various sizes out of a piece of white paper and arranging them in a dark room against a black background. A feeble light proceeded from a nearly turned-out gas-flame. The result proved that the visibility was a question of apparent magnitude to a greater extent than I had believed possible. A distance was readily found at which the larger crosses were plainly visible, while the smaller were quite indistinguishable. To bring the latter into view it was necessary either to increase the light considerably, to approach nearer, or lastly to use a telescope. With sufficient illumination the smallest crosses used were seen perfectly defined at the full distance.

There seems to be no doubt that the explanation is to be sought within the domain of physiological optics. It has occurred to me as possible that with the large aperture of the pupil called into play in a dark place, the focussing may be very defective on account of aberration. The illumination on the retina might then be really less in the image of a small than in the image of a large object of equal "apparent brightness."

"On the Invisibility of Small Objects in a Bad Light." By Lord Rayleigh. (From the *Cambridge Phil. Proc.*, Feb., 1883.)

In a former communication to the Society (March 6, 1882) I made some remarks upon the extraordinary influence of apparent magnitude upon the visibility of objects whose "apparent brightness" was given, and I hazarded the suggestion that in consequence of aberration (attending the large aperture of the pupil called into operation in a bad light) the focussing might be defective. Further experiment has proved that in my own case at any rate much of the effect is attributable to an even simpler cause. I have found that in a nearly dark room I am distinctly short-sighted. With concave spectacles of 36″ negative focus my vision is rendered much sharper, and is attended with increased binocular effect. On a dark night small stars are much more evident with the aid of the spectacles than without them.

In a moderately good light I can detect no signs of short-sightedness. In trying to read large print at a distance I succeeded rather better without the glasses than with them. It seems therefore that the effect is not to be regarded as merely an aggravation of permanent short-sightedness by increase of aperture.

The use of spectacles does not however put the small and the large objects on a level of brightness when seen in a bad light, and the outstanding difference may still be plausibly attributed to aberration.

MR. CARTER'S recent paper on "Civilisation and Eyesight" has called up interesting remarks from Lord Rayleigh and Mr

J. R. Capron, but there seems to be a factor yet unconsidered connected with sharpness of eyesight which is not dependent on the varying aperture of the pupil of the eye. The same amount of light exerts different degrees of stimulus on different individuals, and even in the same person the optic nerves are differently affected, according to his health or age. The pathologist is familiar with the exalted irritability induced by inflammation.

The observer of close double stars becomes in time painfully aware that through age his power of appreciating minute points of light is blunted, although his eye may be in a healthy condition, and quite equal to microscopic work under suitable illumination.

The flattening of the cornea, together with the slow reduction of the curves of the crystalline lens, is a common occurrence, and this change is said to commence at the age of forty-five.

Modification of form and the inability to vary the distance between the lens and the retina, due to defective power in the muscles of the iris, are the chief causes of short sight. On the other hand, the eye appears to have great capabilities of modifying itself to circumstances. It may degenerate by disuse, and even become oblitorated, as may be seen in the blind aquatic beetles of dark caverns, the flea of the bat, and in many species of underground Aphides. Similarly it would seem that the eyes of the student who habitually pores over half-legible German or other type, or the eyes of the watchmaker or the engraver, who use lenses, will permanently accommodate themselves to the short foci required to view objects at short distances, and such modifications may be conceived to become hereditary.

The pupil of the eye perhaps has an aperture wide enough to admit the pencil of light from any telescope; yet it may be worth some consideration whether the sensitiveness of the eye may not for certain purposes be increased, under due precautions, by the use of some such drug as atropa belladonna. The iris thus might be made less contractile under the overpowering light of a planet, and perhaps allow a better observation of a minute satellite revolving close to its primary. It is a well-recognised fact that a faint star once seen may often afterwards be detected with comparative ease by other persons, if its position be truly shown.

Venus may be often seen in broad daylight, if the planet be pointed to by suitable marks.

Care of course would be taken that the use of belladonna shall not cause the observer to see too much. G. B. BUCKTON

THE controversy in NATURE on this subject has brought back to my thoughts a singular illustration of the power of trained eyesight which seems worth noting, though it does not touch the exact comparison between savage and civilised eyes which is the immediate subject of the letters which have appeared in your columns. I refer to the vastly greater capacity for determining visual direction supplied by the sense of symmetry than by actual discrimination between two slightly distant visible points. If you look at a circle, you can aim at its centre with far greater exactitude than you could aim at a point in the true centre of the figure. Every rifleman and every billiard-player exemplifies this. Suppose a billiard-ball placed a little less than five feet from a pocket, and played at as a half-ball stroke from an equal distance for a winning hazard. This is something like what has to be done from baulk in making a pair-of-breeches stroke into the corner pocket. A fair amateur will pot his ball pretty often; a first-rate professional will do it very often. No one, perhaps, can make it a really safe stroke. But observe the accuracy required. The margin of error allowed on each side of the perfect stroke is, on a severe table, not more than an inch at the pocket. This allows an error on each side of about one degree in the point of impact with a radius of one inch (the ball being two inches in diameter). This one inch subtends at the distance from which the stroke is played (nearly 5 feet), an angle of $1^\circ \times \sin 60^\circ, \frac{1}{60} =$ about ·8′. To make the stroke you must first, by eye, place your striking-ball right, then you must, by eye, aim the stroke right, and finally you must make the muscles follow the eye rightly. These three elements of error combined must leave a resultant error of not more than four-fifths of a minute; that is to say, a successful stroke must have a total angular error very considerably less than the smallest angular distance which the eye can appreciate between two visible points. This, of course, explains also the superiority of a rifle foresight, which surrounds the object by a symmetrical figure over one which depends on making one point visibly cover another. G. W. H.

Human Hibernation

As it is obvious that Mr. A. H. Hulk is unacquainted with the facts of what he designates a "well-known Indian trick," and as the matter is one of considerable physiological interest, I think it well to place before your readers the nature of the evidence which satisfied me of the genuineness of this condition, when I referred to it in the fourth edition of my "Human Physiology," published thirty-two years ago—a reference retained by the present editor of that treatise. This evidence had been obtained by Mr. Braid from Indian sources, and published by him in a collected form in 1850, the greater part of it having previously appeared in the pages of the *Lancet*. The most important feature of it was the testimony of British medical officers who witnessed the exhumation—most explicitly given in at least three distinct cases—*to the corpse-like condition of the buried man*, a condition which could not be simulated.

I have since learned from a variety of trustworthy sources, that similar testimony has been over and over again given in India by competent witnesses. Moreover, in one of the cases adduced by Mr. Braid, on information supplied to him direct by the British resident in the summer-house of whose garden the man was buried, the circumstances of the inhumation and of the exhumation were such as absolutely to exclude the "tunnel" hypothesis; while in the case narrated by Lieut. A. Boileau in his "Narrative of a Journey in Rajwarra," 1835, the man was buried in a grave lined with masonry and covered with large slabs of stone.

It is further worthy of mention that this performance is not carried on for the sake of gain, but as a religious observance. Many years ago Prof. Max Müller, finding that I was interested in the matter, kindly placed in my hands a pamphlet printed in India, containing a summary of what is termed the Yoga or Yogi philosophy. The devotees of this system have from time immemorial been in the habit of artificially inducing states of more or less complete abstraction, corresponding closely with those of Braidism; and the condition of apparent death, in which the soul is supposed to leave the body for a time, for communion with the higher world, is the culmination of these conditions, only to be reached by the few; to whom, in consequence, a character for the highest sanctity attaches itself.

With the well-authenticated fact of Col. Townsend's self-induction of a state of apparent death, and of his spontaneous recovery from it, as a "leading case," I cannot regard it as incredible that such a condition of "dormant vitality" might be prolonged for days, weeks, or even months, *in a warm atmosphere*. The suspension of the heat-producing power would of course leave the body susceptible of a fatal reduction of temperature, if its warmth were abstracted by a surrounding medium much cooler than itself WILLIAM B. CARPENTER

Athenæum Club, February 20

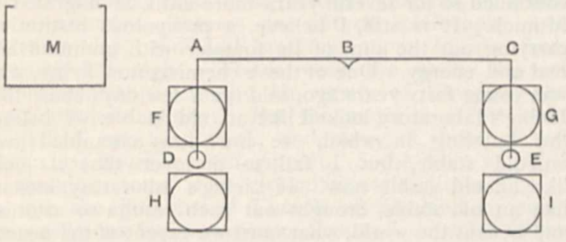
Methods of Determining the Density of the Earth

I HAVE just seen in the report of the proceedings of the Physical Society (NATURE, January 15, p. 260) the account of the ingenious and very important experiments proposed by Drs. König and Richarz to determine the density of the earth. I would suggest that mercury be substituted for lead as the attracting masses. The homogeneity of density, the precision with which its density and temperature can be determined and the ease with which transport from one side of the balance to the other can be effected will commend the use of mercury. The mode of experimenting suggested is the plan of Cornu—used in his determination of the density of the earth by the (Cavendish) Michell experiment—adapted to the same determination by means of the balance.

Let A, B, C be the balance, D E the attracted balls, and F G, H I, the attracting masses of mercury contained in iron spheres of the same capacity, size, and weight. A large mass of mercury is contained in the vessel M, so placed that it has no effect on the balance or on D or E. The balance being in equilibrium with the mass D and E, mercury is allowed to fill F and I, and the effect noted in oscillations after F and I are filled. Then the mercury is drawn from F into H, and G is filled from the reservoir M, and I is emptied, and the second observation obtained. Then D and E are interchanged, and a third observation obtained. Then the mercury in G is run into I, F is filled, and H emptied, and the third observation made, the combination of these four observations making one determination. Electrical effects of the friction of the mercury are avoided by connecting the vessels by wires with the earth.

If H and I form a mass of lead, I infer that three interchanges of D and E will be required, so that each weight shall be brought opposite the top and bottom of each mass to eliminate want of homogeneity in the lead. In the plan I propose only one interchange will be needed.

The effect, if any, of the vessels full of mercury being at unequal distances from the arms of the balance can be readily determined and allowed for.



The plan I suggest may have already presented itself to the eminent scientists who have originated their notable improvement on Von Jolly's plan. The pleasure I had in reading the account of their proposed research has prompted me to make these suggestions. ALFRED M. MAYER

Stevens Institute of Technology, Hoboken, New Jersey,
February 7

Bees and Flowers

As there is a prevailing idea that bees prefer red and blue to other colours, the following observations on their habits may be of interest:—The common hive bees were very busy among the flowers in the garden this morning. Those most frequented were yellow crocus, snowdrop, and Christmas rose. Next in order, winter aconite, yellow jessamine, and blue scilla. On sweet blue violets and on a dwarf erica, which is now flowering, I could see none. Hitherto my observations led me to suppose they never visited the blue scilla for honey, as I had never seen them settle down to it in a business-like manner, but simply flit over it and go to something else. G. W. BULMAN

Corbridge-on-Lyne

Free Lectures

I OBSERVED in NATURE for February 19 (p. 367) a reference to the free lectures at Liverpool, and the inquiry, Why cannot the same thing be done in other large towns? It may interest your readers to learn that a series of free lectures has been given during the past two winters by the professors of this College. Tickets for these lectures are distributed through the agency of a committee composed partly of employers, and the attendance at each lecture numbers between 600 and 700. The audience consists wholly of persons in receipt of weekly wages, the services of the lecturers are given gratuitously, and no charge whatever is made for admission. The small expenses of printing and issuing programmes and tickets are defrayed by the Committee. I inclose the syllabus of this, the second year's course, now drawing to a conclusion.

In addition to these lectures we have from time to time free lectures by gentlemen possessing special knowledge of the contents of the Free Libraries. These, too, are attended by a large number, chiefly of working people, and when the art galleries are completed next year arrangements of a similar kind will doubtless be made in connection with them. WILLIAM A. TILDEN

The Mason Science College, Birmingham, February 24

A Tracing Paper Screen

I CAN add to the testimony of Mr. Charles Taylor about the efficiency of a screen of tracing paper. I have used for several years a small screen of tracing cloth mounted on rollers like a map. It is very portable and soon fixed. With a sciopticon lantern (oil lamp) I have shown transparencies in the winter months to an audience of seven hundred men in a Midland Railway mess-room during the breakfast hour—8.15 to 8.50 a.m.—though the windows are by no means in the best position, and the room is lighted by skylights as well as by side windows. It is a pity this screen is not better known and more extensively used for scientific lectures. H. ARNOLD BEMROSE

Irongate, Derby, February 28

An Author's Gratitude

I WISH to express my gratitude to NATURE and to the reviewer in NATURE of my little pamphlet on Electrical Units for exposing a compound error by which the farad came to be described as a fraction of the electrostatic C.G.S. unit of capacity instead of the electro-magnetic unit. Was there ever a greater blunder? It was as if I had said the value of the tenth part of a farthing is sufficient to pay off a million times the National Debt of Great Britain. On recovering from the shock occasioned by the revelation, I hastened to the printer, and got him to correct the error "ere the sun went down," and now I overflow with gratitude to your reviewer, who has relieved me of the awful incubus of an error of the 10^{20} magnitude.

RICHARD WORMELL

SCIENTIFIC LABORATORIES[1]

I FEEL that the present occasion, upon which you have done me the honour to ask me to preside, is one of very great importance indeed, and I wish some person more competent to preside on such an occasion and give a suitable inaugural address were in my place. I am afraid I must confine myself to something not at all worthy of the greatness of an occasion which is almost the opening of a new university. Not quite so, because the real opening of this college took place several months ago; but still it is an occasion which I feel to be much more than merely the opening of a department—a working department—in the college; an occasion of so great moment that I regret that I shall not be able to give anything that could be properly considered a worthy inaugural address. I shall be obliged to ask your indulgence if I confine myself specially to departments with which I am personally familiar—scientific laboratories. The laboratory of a scientific man is his place of work. The laboratory of the geologist and of the naturalist is the face of this beautiful world. The geologist's laboratory is the mountain, the ravine, and the seashore. The naturalist and the botanist go to foreign lands, to study the wonders of nature, and describe and classify the results of their observations. But they must do more than merely describe, represent, and depict what they have seen. They must bring home the products of their expeditions to their studies, and have recourse to the appliances of the laboratory properly so-called for their thorough and detailed examination. The naturalist in his laboratory with his microscope and appliances for the keenest examination, learns to know more than can be learned by merely looking at external beauties. The geologist brings his specimens to the chemist—is himself a chemist perhaps—brings his crystals to the physical laboratory to be examined as to their physical properties, their hardness, the angles between their faces, their optical qualities. Some people might think this an ignoble way to deal with crystals. But it is not so to the trained eye and deeper thought of the scientific man. The scientific man sees and feels beauty as much as any mere observer—as much as any artist or painter. But he also sees something underlying that beauty; he wishes to learn something of the actions and forces producing those beautiful results. The necessity for study below the surface seems to have been earliest recognised in anatomy, and earliest carried out in human anatomy. I am not going to speak of the work of scientific research generally, but with reference to the special occasion which brings us here this day—the opening of the chemical and physical laboratories of the University College of North Wales. I am going to speak

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[1] Address by Prof. Sir William Thomson, F.R.S., on the occasion of the opening of the Laboratories of University College, Bangor.

of laboratories for students, laboratories in which the students work with their own hands. There have been laboratories of investigation from the earliest times. No doubt Aristotle had his; and Archimedes had a laboratory wherever he went—in his bath, even, he observed, and studied, and thought out the laws of hydrostatics. But those were not students' laboratories, and our special subject to-day is a students' laboratory, where they can meet together for the practical study of the various departments of science, where they will be brought together to use their eyes and hands—their eyes otherwise than in merely reading books and looking at pictures or drawings; their eyes to observe accurately, and their hands to experiment, in order to learn more than can be learned by mere observation. To teach students to so work and so learn is the object of a scientific students' laboratory.

The first scientific laboratory that ever existed was that of Frederick II., King of Sicily, and was established between 1200 and 1250. Acting under the advice of his chief physician, Martianus, he made a law that nobody should practise physic or surgery without having studied anatomy practically. He established a school of practical anatomy, to which students flocked from all parts of Europe for many years. Subsequently there was an anatomical school instituted at Bologna; and in those two schools we hear the first of students working in laboratories. The anatomical students' working-room has for several hundred years been generally recognised as an absolute necessity of medical education. But I believe there was no other branch of physical science where students worked in the laboratory until probably twenty years of the present century had passed away. The University of Glasgow is, I think, justly entitled to take some pride in the great modern expansion and extension of the system of giving students practical work in laboratories, as an addition to the education which previously had been confined almost entirely to book-work, or, at best, to attending lectures illustrated by experiments and diagrams. The first chemical laboratory for students, so far as I know, was that founded by a colleague of my own name, though no relation—Thomas Thomson,[1] the great chemist and mineralogist. Prior to 1831 a students' chemical laboratory, under Thomas Thomson, at Glasgow University, flourished and was attended by a large number of students. These were chiefly medical students, but a considerable number also were students who wished to learn chemistry to practise it in the various chemical manufactories in Glasgow and the North of England, while some went to learn chemistry solely for the sake of science. A chemical laboratory has now become indispensable in all universities. A notable development of chemical laboratories with reference to practical education in chemistry, was made by Liebig not many years after 1831. I fix that date from personal recollection. In 1831 I first came to Glasgow, and I well remember that the building containing the chemical lecture room and laboratory existed then. How long before 1831 it was built I do not at this moment recollect. The world-renowned laboratory of Liebig brought together all the young chemists of the day. If I were to name the great men who studied at Giessen I should have to name almost every one of the great chemists of the present day who were young forty years ago. His laboratory was in full and flourishing activity between 1841 and 1845, and continued so for several years more until he migrated to Munich. It is still, I believe, a prosperous institution, carrying out the aims of its founder with undiminished zeal and energy. One of those chemists now living, who was young forty years ago, told me a few days since that Liebig's laboratory looked like an old stable. I believe the building in which we are now assembled *was* an old stable, but I fail to discover that it looks like an old stable now. If Liebig's laboratory, looking like an old stable, brought out such results to astonish and benefit the world, what must we expect of the beautiful laboratory in which we are now met? What would Liebig not have given for the appliances and advantages afforded by the well-equipped buildings of the North Wales College at Bangor? What would Liebig not have given for the facilities which now exist in these admirably-appointed lecture-rooms in which we are now met, and for the carefully-equipped laboratories and working-rooms, and places for special experimental work covering the area of the old stables and coach-houses of the "Penrhyn Arms Hotel"! If the professors and the students in this College—I think I may already say this thriving College—will be inspired by the zeal of those who have worked before them, a great reward will result even in the first year of the existence of the institution.

With respect to physical laboratories I may be allowed, without being thought egotistical, to say something in which I must speak of my own action. The physical laboratory in the University of Glasgow is, I believe, the first of the physical laboratories of which we have now so many. When I entered upon the professorship of natural philosophy at Glasgow I found apparatus of a very old-fashioned kind. Much of it was more than a hundred years old, little of it less than fifty years old, and most of it was of worm-eaten mahogany. Still with such appliances year after year students of natural philosophy had been brought together and taught. The principles of dynamics and electricity had been well illustrated and well taught: as well taught as lectures and so imperfect apparatus—but apparatus merely of the lecture-illustration kind—could teach. But there was absolutely no provision of any kind for experimental investigation, still less idea, even, for anything like students' practical work. Students' laboratories in physical science were not then thought of. I remember one of the chemists of the Liebig school asking me what was the object of a physical laboratory. I replied that it was to investigate the properties of matter. I could give no better answer now. I may remind you that there is no philosophical division whatever between chemistry and physics. The distinction is that different properties are investigated by different sets of apparatus. The distinction between chemistry and physics must be merely a distinction of detail and of division of labour.

Soon after I entered my present chair in the University of Glasgow in 1845 I had occasion to undertake some investigations of certain electrodynamic qualities of matter, to answer questions which had been suggested by the results of mathematical theory, questions which could only be answered by direct experiment. The labour of observing proved too heavy, much of it could scarcely be carried on without two or more persons working together. I therefore invited students to aid in the work. They willingly accepted the invitation, and lent me most cheerful and able help. Soon after, other students, hearing that some of their class-fellows had got experimental work to do,

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[Note added February 12, 1885:—First Professor of Chemistry in Glasgow University; appointed 1818; held the chair till his death, 1852.

The minutes of the Faculty of Glasgow College show that as early as the first month of 1828, Prof. Thomas Thomson began applying for more commodious premises in which to carry on his work in the department of chemistry. For two years he kept his wants persistently before the Faculty (of which he, being only a "Regius Professor," was not a member) until January 1830, when his efforts were crowned with success. A plot of ground was then purchased at the corner of College Street and Shuttle Street, outside the College precincts, and operations were at once begun, and pushed on with such vigour that the buildings seem to have been finished towards the end of the same year. The building thus erected contained ample and well-designed accommodation for teaching and experimental work. There was a large class-room and a large and conveniently-arranged public laboratory for students, with private rooms for the professor and for the prosecution of experimental research by the professor and his assistants, or by students and others.

Part of the ground-floor of the premises was let to a tenant (the "Falstaff Tavern" for many years!). To-day I found the building still in existence, and occupied by "George Younger and Co.'s Yarn Stores." Nearly all the rest of the University Buildings within the College precincts have been pulled down within the last twelve years for the "College Railway Station," which now occupies the site of the old Glasgow College and University.—W. T.]

came to me and volunteered to assist in the investigation. I could not give them all work in the particular investigation with which I had commenced—"The electric convection of heat"—for want of means and time and possibilities of arrangement, but I did all in my power to find work for them on allied subjects (Electrodynamic Properties of Metals,[1] Moduluses of Elasticity of Metals, Elastic Fatigue, Atmospheric Electricity, &c.) I then had an ordinary class of a hundred students, of whom some attended lectures in natural philosophy two hours a day, and had nothing more to do from morning till night. Those were the palmy days of natural philosophy in the University of Glasgow—the pre-Commissional days. But the majority of the class really had very hard work, and many of them worked after class-hours for self-support. Some were engaged in teaching, some were city-missionaries, intending to go into the Established Church of Scotland or some other religious denomination of Scotland, or some of the denominations of Wales, for I always had many Welsh students. But about five and twenty of the whole number found time to come to me for experimental work several hours every day. In those days, as now, in the Scottish Universities all intending theological students took the "philosophical curriculum"—*zuerst collegium logicum*—then moral philosophy, and (generally last) natural philosophy. Three-fourths of my volunteer experimentalists used to be students who entered the theological classes immediately after the completion of the philosophical curriculum. I well remember the surprise of a great German Professor when he heard of this rule and usage: "What! do the theologians learn physics?" I said, "Yes, they all do; and many of them have made capital experiments." I believe they do not find that their theology suffers at all from having learned something of mathematics, and dynamics, and experimental physics before they enter upon it. I had then no other premises than the old lecture-room and the adjoining apparatus room. To meet my requirements for my new volunteer laboratory corps, the "Faculty" (the then governing body of the College) allotted to me an old wine-cellar, part of an old professor's house, the rest of which had been converted into lecture-rooms. This, with the bins swept away, and a water-supply and a sink added, served as physical laboratory (a name then unknown) for several years, till the University Commissioners came and abolished a certain old function of Glasgow University, the "Blackstone Examination." The examination room was left unprotected, its talisman, the old "Blackstone Chair," removed. I instantly annexed it (it was very convenient, adjoining the old wine-cellar and below the apparatus room); and, as soon as it could conveniently be done, obtained the sanction of the Faculty for the annexation. The Blackstone room and the old wine-cellar served well for physical laboratory till 1870, when the University was removed from its old site imbedded in the densest part of the city, to the airy hill-top on which it now stands. In the new University buildings ample and commodious provision was made for experimental work.

In that good old time some students used to come to me under the impression that the laboratory would prove an agreeable lounge, where they could meet pleasantly and spend the forenoon talking matters over. They were soon undeceived as to its being a lounge for idly whiling away time. I hope they were not altogether disappointed when they thought it would be agreeable, and I almost hope they found it even more agreeable than they expected. They certainly learned something of patience and perseverance, if not much science, in the six months of the College session. As a matter of general education for those not going to practise medicine, was it of any use entering a chemical or physical laboratory? I found as many as three-quarters of the students were destined for service in the religious denominations in after-life. I have frequently met some of those old students who had entered upon their profession as ministers, and have found that they always recollected with interest their experimental work at the University. They felt that the time they had spent in making definite and accurate measurements had not been time thrown away, because it educated them into accuracy,—it educated them into perseverance if they required such education. Some students even worked so hard in my laboratory that I had to interpose for the sake of their health. There is one thing I feel strongly in respect to investigation in physical or chemical laboratories—it leaves no room for shady, doubtful distinctions between truth, half-truth, whole falsehood. In the laboratory everything tested or tried is found either true or not true. Every result is *true*. Nothing not proved true is a *result;* —there is no such thing as doubtfulness. The search for absolute and unmistakable truth is promoted by laboratory work in a manner beyond all conception. It is a kind of work in which also patience and perseverance are promoted in a most marked degree. No labour must be shrunk from; everything must be carefully done. There is this which is satisfactory about it: that perseverance is sure to be rewarded. There is no failure in physical science. We do not always find the particular thing looked for; we often find that what we looked for does not exist, or that something else exists very different from what we expected to find; but that something is to be found in any investigation entered upon with intelligence and pursued with perseverance, is a certainty; and also that that something is not valueless follows as a matter of course. Every additional knowledge of the properties of matter is of value.

A large part of the work of a physical or chemical laboratory must be measurement. That might seem rather trying work; "harsh and crabbed" shall we say? Who cares to measure the length of a line in land surveying, or of a piece of cord, or of ribbon, or of cloth? These may not be in themselves essentially interesting occupations; but if it becomes necessary to measure something smaller than can be seen with the eye, the measurement itself becomes an object to inspire the worker with the greatest ardour. Dulness does not exist in science. What do you think of a measurement of something you can only gauge by inference from the performance of the apparatus tested in some peculiarly subtle way? The difficulties to be overcome in physical science in mere measurement are teeming with interest. Properties of matter, or forces to be contended with, oblige us to be always digressing. We cannot go on saying—"We will think of nothing but the object before us." Every person who aims at one object of course perseveres until he attains it; but he keeps his mind open until he can return to some other object never thought of at first, but which thrust itself on him as a difficulty occurring in the pursuit of the first object. The very disappointments in attaining objects sought after in the investigations of physical science are the richest sources of ultimate profit, and present satisfaction and pleasure, notwithstanding the difficulties and disappointments contended with. But I am afraid I am taxing your patience too much. I will only just say with reference to physical laboratories that they are now advancing to something of the method and consistent system that Thomas Thomson and Liebig so greatly gave to chemical laboratories. I, myself, have not done so much as I might have done in that way. The physical laboratory at Glasgow has, I believe, been, more than most others, devoted to whatever work occurred in physical investigation, measuring properties of matter, comparing thermometers, electrometers, galvanometers, and doing other practically useful work. We put the junior students at once into investigations, and let

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[1] Results up to 1856 published under this title, as Bakerian Lecture for 1856 (*Trans. R. S.*, and republished recently in vol. ii. of "Collected Papers."—W. T.)

them measure and weigh whatever requires measurement and weighing in the course of the investigation. I look with admiration to what has been done by those who have worked up physical laboratories to their present advanced condition. The physical laboratories of King's College and University College, London, under the admirable organisation and work of Professor Adams and Professor Carey Foster; the Cavendish laboratory at Cambridge, originated by Clerk Maxwell, and admirably systematised and perfected by Lord Rayleigh, have rendered splendid services to physical science all over the world. Much has been done even to provide suitable text-books for use in the systematic practical training of students in laboratory work: for example, the "Treatise on Physical Measurement," by Kohlrausch, which has been for several years a most serviceable manual, and the lately published "Practical Physics" of Glazebrook and Shaw. The physical laboratory system has now become quite universal. No university in the world can now live unless it has a well-equipped laboratory. I hope you will all do your best to make the physical and chemical laboratories of this college a great success; that you will follow example in everything exemplary until the Bangor laboratories become a model to be followed in future laboratories in Wales, England, or any other part of the world. I was not quite accurate when I spoke of this new college in this City of Bangor as *the* University College of North Wales. My friend, Mr. Cadwaladr Davies, your secretary, has reminded me that there was a university of North Wales at Bangor-is-y-coed, in Flintshire—not a city, because it did not combine a bishop and a mayor—but a town which had the honour of having been the seat of the first Welsh university known to history. There may have been universities in Wales before the one which flourished 1200 years ago at Bangor-is-y-coed; but their history is lost in the long night of silence, because no sacred bard sung of their existence. The university of Bangor-is-y-coed had its bard, who tells us that the institution had 2100 students. There you have a worthy object of ambition for the city of Bangor! May it soon have a goodly proportion of the 2100. Perhaps not so long a time may elapse before your college and the other colleges in Wales may reach to such a number. Indeed, I do not see anything unreasonable in hoping and expecting that in a dozen years there will be 2100 university-students in Wales. The population of Wales is more than a million and a half, which is, I think, about a fourth of the population of Scotland; and I do not see why Wales should not have university students in proportion to its population as well as Scotland. I believe the brightness and activity of the Welsh intelligence will thoroughly take up the idea of a university, and profit by it to the utmost, and, I believe, the existence of this institution at Bangor will before twenty years have passed away, be looked upon as having been a great benefit to the Principality. What Wales gained by the university at Bangor-is-y-coed can scarcely now be told, but alas, for that university with its 2000 students, it was destroyed in the year 613 by Ethelfred, King of Northumbria, and its destruction was followed by 900 years of dark ages. Thus we see what the world lost by the annihilation of the first university of North Wales. Another bard, Lewis Glencothy, advocated and sang of the possibility of a university in Wales in the time of Henry VII. Richard Baxter, not a Welshman nor a bard but the great English Puritan divine, reported to the then Government under Cromwell in favour of a university for Wales. Cromwell died before action was taken, and nothing was done in the matter for nearly 200 years, when a very active desire sprang up and active co-operation among all parties was entered upon, for having a university established in Wales. We see everything now prospering in that direction. I look forward hopefully to the time when this college of Bangor—if not an independent university of its own—will be a college of the University of Wales. All the colleges of Wales, equipped to do the work of a university, might be united to form a University of Wales. There are very many important advantages in favour of such an arrangement. No doubt it is an object of honourable ambition; but it may be asked if a college does all the work of a university, what does it matter whether it is called a university or not? It is of considerable importance that your college should be either a university itself, or part of a university of which it is an integral college. One of the advantages would be that the teaching of the college would be enabled to take a more practical form than it can possibly take as long as its main purpose is that of preparing students for the degree examinations of London University. The degree system of London University fills a widespread want—a want felt over the whole range of the British empire; a want of marking by the stamp of a university degree, if not by some more suitable title, the possession of knowledge and of a certain amount of training by those who have not had the opportunity of obtaining that knowledge in any thoroughly equipped college or university. That is a splendid reason for the existence of the London University, and it has well fulfilled its reason for existence. But, for all that, it would be greatly better for the students of the University College of North Wales if the teaching were conducted with reference to an examination carried on by their own professors and colleague professors in other properly equipped Welsh colleges. It is the greatest mistake in respect to teaching and examining to think that the examiner is an inspector. An examiner of schools must to some extent take that position. But in university work teaching and examining must go side by side, hand in hand, day by day, week by week together, if the work is to be well done. The object of a university is teaching, not testing. Testing products comes at some times, and for some special purposes, to be a necessity; but in respect to the teaching of a university, the object of examination is to promote the teaching. The examination should be, in the first place, daily. No professor should meet his class without talking to them. He should talk to them and they to him. The French call a lecture a *conférence*, and I admire the idea involved in that name. Every lecture should be a conference of teacher and students. It is the true ideal of a professorial lecture. I have found that many students are afflicted when they come up to college with the disease called "aphasia." They will not answer when questioned, even when the very words of the answer are put in their mouths, or when the answer is simply "yes" or "no." That disease wears off in a few weeks, but the great cure for it is in repeated and careful and very free interchange of question and answer between teacher and student. Professors and students must speak to one another. One of the greatest things is to promote freedom of conversation in such classes, to cultivate in them the power of expressing ideas in words. Then something more definite than *viva voce* examination can come. Written examinations are very important, as training the student to express with clearness and accuracy the knowledge he has gained, and to work out problems, or numerical results, but they should be once a week to be beneficial. If only occurring once in two or three months they will lose their effect in promoting good teaching, and can be scarcely more than a test; if only once a year they are merely inspector's work. The object of the university should be teaching, and examining should only be part of its work, and that only so far as it promotes teaching. The credit of the University should depend on good teaching, and no candidate should be granted a degree who does not show that he has taken advantage of the good teaching. But it is impossible to carry out that programme to best advantage by a college which is not in itself an integral part of a university. Such examinations as those of the London University are necessarily arranged to suit thousands of candidates who have learned in different schools, and cannot always contain questions that would be most suitable for one particular mode of teaching. The kind of questions set would be of a different nature if the giving of the questions devolved upon those who had in hand the teaching. Those who have the teaching can give an examination vastly more useful and one that would re-act on the teaching in a way that an examination of a multitude of students trained at all kinds of institutions, and many merely by private reading, could not possibly do. Therefore, it seems to be a matter of high importance indeed that there should be a University of Wales; that you should consider it to be a great object to be attained, sooner or later—but the sooner the better—the establishment of the University of Wales, with the University College of North Wales an integral part of it. I have much pleasure in wishing the University College of North Wales every success, and I trust that the laboratories now opened may prove of great value in promoting and aiding the study of science.

POLYNOMIALS IN ZOOLOGY [1]

SINCE the days of Linnæus scientific zoologists have universally adopted the binomial system of nomenclature, which was invented and introduced by that great naturalist. So long as the idea of the fixity of species, as originally created entities, prevailed, there was no excuse for deviating from the Linnean plan. Such an idea as a transitional series between two species, or the division of a species into two or more local forms, was hardly understood by the older authors. But of late years, since the general acceptance of the derivative origin of species, it has become universally acknowledged that sub-species and transitional forms do exist in Nature, and many and various plans have been proposed for indicating them. Trinomials—that is, the usage of three names, of which the last is that of the sub-species—are in great favour with a rising section of American zoologists, and there is much to be said in their defence. But the concession of three terms, it is said, would in some cases not be sufficient. Quadrinomials and Polynomials must necessarily follow, and render nomenclature inconveniently long. Mr. S. Garman, the well-known herpetologist of the Comparative Museum of Zoology at Harvard College, Cambridge, replies, in the pamphlet now before us, to the assertion "that there is no other or better method than "polynomials." Mr. Garman proposes to designate the different forms or sub-species of a species by symbols such as (A), (B), (C), (D). Supposing that the (C) form is found to consist of several sub-varieties he would name them ($C.^a$), ($C.^b$), ($C.^c$). Still further subdivisions might be indicated as forms (C^{a1}), (C^{a2}), (C^{a3}), and ($C^{a1'}$), ($C^{a1''}$), &c. Thus the polynomial "*Amblystoma tigrinum mavortium hallowelli suspectum maculatissimum*" would be reduced to "($C.^{a1'}$) *Amblystoma tigrinum*," the "advantage" of which for general literature is "apparent"! But is not this a case in which it may be said that the proposed remedy is as bad as the disease?

TEMPERED GLASS

WE are very pleased to be able to chronicle an application which Mr. Frederick Siemens has recently made in his regenerative gas radiating furnace, described in the autumn of last year (NATURE, vol. xxxi. p. 7). It consists in the production of glass which appears to be of a very homogeneous character and of considerable strength and hardness, and will doubtless become available for a number of useful purposes. The scientific principle which is applied in the three distinct processes to which we propose to refer shortly, is that of keeping the whole body of the glass at a uniform temperature during the operations of heating and cooling—that is to say, that at each unit of time the whole mass shall be at one temperature. Two methods have hitherto been employed by means of which glass has been rendered more or less independent of variation of temperature. The oldest of these is that carried on in the annealing kiln, in which the manufactured articles of glass are allowed to cool very slowly. The more modern is that of De la Bastie; in this process the finished articles of glass had generally to be annealed in the first instance, then heated to such a temperature as to soften them, when they were immersed in a bath of heated oil maintained at a temperature above 300° C., which was said to make them tough. The objection to annealing is mainly that of cost, but the objection to the De la Bastie process is that it is wrong in principle, as, owing to the manner in which cooling is effected, the glass is in a state of tension throughout, which is brought to evidence by means of the polariscope. The glass produced by the processes to be described are almost free from internal strain, and Mr. Siemens holds that, could the principle he propounds be carried out perfectly in practice, the glass would be free from tension throughout its whole mass. A corollary which may apparently be drawn from this proposition is that every metal not cooled in the way proposed is in strain; but that, owing to the much greater tensile strength of metals, the state of tension does not become evident in the same manner as in glass, which is notably brittle.

Press-hardened Glass.—Only glass of the very best quality is suitable for hardening. It is cut into the proposed shapes and placed in the radiation furnace until soft; it is then removed and placed between cold metal plates, and cooled down in the proportion of its volume or capacity for heat. Glass may be cooled so rapidly by this means that the diamond will not touch it; the process is mostly applied to sheet and plate glass, which may either be plain or decorated, and whose strength is thereby increased eight times. The degree of hardening which may be attained depends on the temperature to which the glass is heated and the rate at which it is cooled. The higher the temperature, and the more quickly the glass is cooled, the harder is the glass. Thus, for very quick cooling copper plates are used in the presses, and the glass is rendered exceedingly hard; when a less degree of hardness is desired, iron plates, or even these covered with asbestos, or clay slabs, may be employed.

Sheet-glass of ordinary thickness is heated in a minute and cooled in half a minute. It is remarkable that this can be effected in so short a space of time without injury to the glass, and is due to the uniformity of the heating and cooling operations. Owing to the high temperature at which this process is carried on, more refractory enamels, such as those used for porcelain, can be applied, and the enamel is thus rendered as indestructible as the glass itself.

Semi-hardening is employed for goods to which presses cannot be easily applied. The glass is heated up to a high temperature, but not to such a degree as to affect its shape, and is then placed within an iron casing having internal projecting ribs so arranged as to hold the glass article in position and to touch it at the fewest possible points. The casing with its inclosure is cooled in the open air. The process is only applicable to articles of nearly uniform thickness throughout; it increases the strength of the glass about three times, and renders it less liable to be effected by changes of temperature than ordinary glass is.

The third kind of glass, which is known as *hard-cast glass*, has not yet been introduced commercially, but samples of the work produced in the form of sleepers, tramway-rails, grindstones, and floor-plates were exhibited at the meeting. The method of production is very simple.

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[1] "On the Use of Polynomials and Names in Zoology." By S. Garman, Cambridge, Mass., U.S.A. From the *Proceedings* of the Boston Society of Natural History, March 19, 1884.

Glass made in a continuous glass-melting furnace is run into moulds as with iron castings. The only precaution that has to be taken is that the moulding material shall have as nearly as may be the same specific heat and the same conductivity for heat as glass. Various mixtures of materials that are easily obtainable and not costly are suitable, such as mixtures of powdered porcelain, glass pots, metal turnings and filings, and such minerals as heavy spar and magnetic iron ore. These are pulverised and mixed in certain proportions, and then moulded in the ordinary way. The glass being run into the mould, the mould and its contents are heated up together, and then cooled together, and, when cool, the mould is opened and the glass removed. Glass may thus be cast into forms which it would be impossible to produce otherwise. That glass may thus be manufactured of great homogeneity was proved by the clear ring of a large tuning-fork made of the material, and in the manner described. Mr. Siemens promises on a future occasion to bring this matter again before the Society of Arts, after the completion of the works which he is now erecting for the manufacture of glass according to the process last described. As regards the other processes, the manufacture has increased in six years from 600*l.* to 7000*l.*, and, considering the very cheap rate at which hard glass castings can be produced—viz. about 5*s.* 6*d.* a hundredweight—Mr. Siemens feels satisfied that a large business will be done, more particularly as they supply a want which is felt on all sides; and thinks that glass not being liable to oxidation, as soon as it could be depended upon as regards strength, it would be applied for purposes for which metals, stone, and porcelain have hitherto been used.

THE PHYSIOLOGICAL LABORATORY AND OXFORD MEDICAL TEACHING

[WE regret to learn that another attempt is being made to suppress physiological teaching at Oxford. The not-over-scrupulous foes of scientific teaching and research have, we understand, distributed manifestoes by thousands all over the country. We hope, therefore, that the following statement will receive equally wide circulation. Scarcely any of the well-known men who have signed the statement are in any way connected with what is generally known as science; certainly not one of them would have signed it had there been the least suspicion that in the Oxford Laboratory there would be any approach to cruelty:—]

A decree to provide for the expenditure of the department of Physiology will be submitted to Convocation on Tuesday, March 10. The annual sum required for this purpose is 300*l.*, besides 200*l.* for the salary of the Demonstrator of Histology.

The arrangements for the organisation of a complete system of instruction in the subjects of the first B.M. Examination and of the first and second Professional Examinations of the Conjoint Board of the College of Physicians and of the College of Surgeons in London are in progress, and will soon be completed. The new Lecturer on Human Anatomy will very shortly be appointed, and the Physiological Laboratory will be completed and ready for occupation by the end of the summer; so that before next October the University will be in a position to undertake the teaching of Human Anatomy and Physiology. The arrangements for teaching the other subjects in which instruction is required by medical students are also in progress.

As, in accordance with the recent resolution of the Colleges of Physicians and Surgeons, Candidates who have satisfied the Oxford Examiners in Anatomy, Physiology, and the other subjects of the first and second Professional Examinations, will be exempted from further examination in these subjects, Members of the University will in future be able to complete their first two years of medical study without leaving Oxford.

The purpose for which the expenditure is required is instruction not research, and no experiments upon the living animal involving pain will be used for demonstration to students or instruction, with or without anæsthetics.

It is, however, intended by those who desire absolutely to prohibit such experiments in physiological inquiry, to oppose the decree for the maintenance of the laboratory. Energetic measures are being taken to this end. The rejection of the decree would involve fatal consequences as regards the above-mentioned scheme for the teaching of medical science. The University has already twice pronounced upon the issues now sought to be raised, by votes taken in unusually full Convocations on June 5, 1883, and February 5, 1884. We, therefore, trust that you will be good enough to attend and vote in favour of the Decree on March 10, at 2 p.m.

H. G. LIDDELL, Dean of Christ Church.
J. FRANCK BRIGHT, Master of University.
GEORGE C. BRODRICK, Warden of Merton.
J. P. LIGHTFOOT, Rector of Exeter College.
DAVID B. MONRO, Provost of Oriel.
JOHN R. MAGRATH, Provost of Queen's.
J. E. SEWELL, Warden of New College.
W. W. MERRY, Rector of Lincoln.
W. R. ANSON, Warden of All Souls.
E. H. CRADOCK, Principal of B.N.C.
T. FOWLER, President of Corpus.
J. PERCIVAL, President of Trinity.
H D. HARPER, Principal of Jesus College.
G. E. THORLEY, Warden of Wadham.
EDWARD S. TALBOT, Warden of Keble.
WILLIAM INCE, Regius Professor of Divinity.
H. W. ACLAND, Regius Professor of Medicine.
W. H. FREEMANTLE, Fellow of Balliol College.
JOHN CONROY, Christ Church
ALFRED ROBINSON, Fellow of New College.
T. HERBERT WARREN, Fellow of Magdalene College.
F. MAX MÜLLER, Corpus Professor of Comparative Philology.
BARTHOLOMEW PRICE, Sedleian Professor of Natural Philosophy.
HENRY NETTLESHIP, Corpus Professor of Latin.
JAMES LEGGE, Professor of Chinese.
J. EARLE, Professor of Anglo-Saxon.
JOHN RHYS, Professor of Celtic.
T. H. T. HOPKINS, Fellow of Magdalen.
W. LOCK, Fellow of Magdalen College, Sub-Warden of Keble College.
W. W. JACKSON, Fellow of Exeter, Censor of Non-Collegiate Students.
H. F. TOZER, Fellow and Tutor of Exeter.
A. G. BUTLER, Fellow and Tutor of Oriel.
AUBREY MOORE, Tutor of Keble and Magdalen.
ROBERT L. OTTLEY, Student of Christ Church.
W. MARKBY, Reader in Indian Law, Fellow and Tutor of Balliol College, and Fellow of All Souls' College.
H. F. PELHAM, Exeter College.

THE MAXIM GUN

MR. HIRAM STEVENS MAXIM, the well-known American engineer, has lately brought out a new form of a machine-gun, which is attracting a great deal of attention in military and naval circles. This gun is a completely new departure. It takes the cartridges out of the box in which they were originally packed, puts them into the barrel, fires them, and expels the empty cartridges, using, for this purpose, energy derived from the recoil of the barrel. Of course it is necessary to put the first cartridge into the barrel by hand. When, however, this is done, and the trigger pulled, the gun will go on and fire as long as there are any cartridges in the box.

The cartridges are placed in a belt formed of two bands of tape, before they are placed in the box, and one end of this belt is placed in the gun at the time of starting, the action of the gun drawing in one cartridge every time that one has exploded. The gun is really a veritable gunpowder-engine, the recoil of the barrel, the block, and the lock corresponding to the piston and cross-head of the engine. The recoil drives the barrel and its attachments backwards, opens the breech, cocks the hammer, and expels the empty shell. The return of the block is effected by a spring. As the bolt returns, it forces a loaded cartridge into the barrel and pulls the trigger.

It would naturally be supposed that a gun which loads and fires itself would be somewhat complicated. This, however, is not the case when the gun is considered simply as a self-loading gun. The additional parts which form a part of Mr. Maxim's new gun are due rather to the mode of feeding than to the fact that the gun is automatic. It is certainly a very great advantage to have the gun supplied from a very large magazine from below.

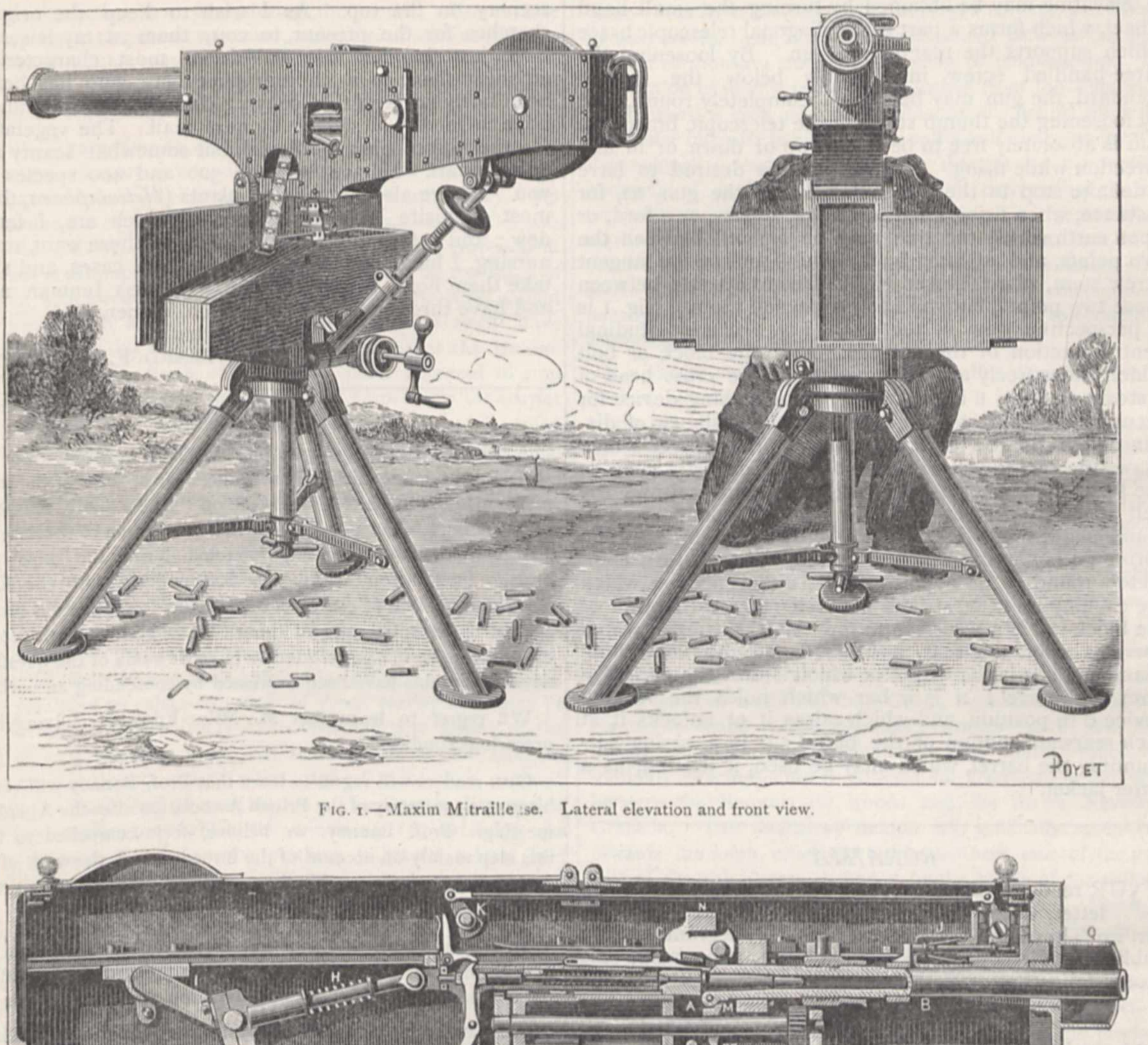

Fig. 1.—Maxim Mitrailleuse. Lateral elevation and front view.

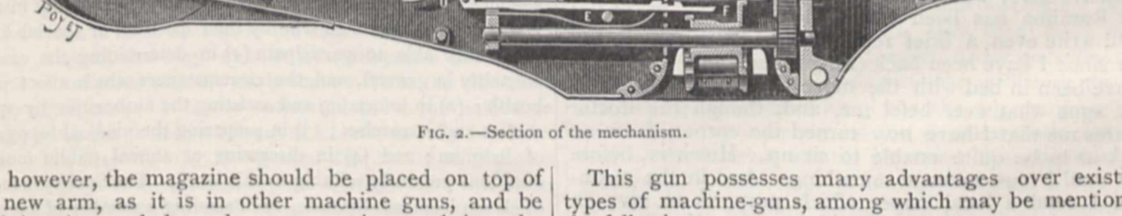

Fig. 2.—Section of the mechanism.

If, however, the magazine should be placed on top of this new arm, as it is in other machine guns, and be small in size and depend upon gravity to bring the cartridges into their respective places, the gun would be quite as simple as any existing guns.

The rapidity of fire in this gun is regulated by a cataract chamber, and the gun may be fired at any speed from one round per minute up to 600 per minute for guns of rifle calibre and slower for larger sizes.

This gun possesses many advantages over existing types of machine-guns, among which may be mentioned the following :—

As it furnishes its own power, it does not require to be firmly fixed upon its platform as other guns do, so that it is quite free to move in any direction while being fired.

Cartridges which hang fire, and which have proved so disastrous to other forms of machine guns, do not present any obstacle to the operation of this arm. As each particular cartridge depends upon its own power to withdraw itself from the barrel, it will be obvious that the cartridge cannot remove itself from the barrel before it explodes.

A gun which loads and fires itself is certainly a novelty, and presents many interesting features and possibilities to any one who takes an interest in implements of warfare.

The gun may be trained in any direction by turning the crank which operates a tangent screw, the stem of which projects from the platform immediately below the cartridge box seen on the top of the tripod, whilst a fine adjustment in elevating may be obtained by turning the small hand wheel, which forms a part of the diagonal telescopic brace which supports the rear of the gun. By loosening the three-handled screw immediately below the central standard, the gun may be turned completely round, and by loosening the thumb screw of the telescopic brace, the gun is absolutely free to be moved up or down or in any direction while firing. If, however, it is desired to have a definite stop to the horizontal play of the gun, as, for instance, when firing upon a bridge, a pass, or a ford, or upon earthworks, the gun may be sighted between the two points, and adjusted by the thumb nuts on the tangent screw stem, when the gun will be free to operate between these two points, but will not go beyond them. Fig. 1 is a perspective view of the gun. Fig. 2 is a longitudinal central section of the weapon. A is the block or bolt which slides freely after the manner of the cross head of a steam-engine ; B is the barrel ; C the locking device for securing the block to the barrel at the instance of discharge ; D is the cocking lever ; E, the carrier which draws the cartridges out of the belt and deposits them in the feed wheel G ; F is the belt wheel which draws the belt of cartridges into the gun ; H is a connecting rod made slightly elastic by being provided with a strong spring ; I is a crank which does not, however, turn completely round ; L is a point of resistance, against which the cocking-lever, D, strikes at each rearward motion of the block ; K is a shaft connected with the trigger, which operates upon the sear and also upon the controlling chamber J ; M is the extractor which starts the cartridge from the barrel ; N is a bar which holds the locking device C in position, and which raises it or unlocks it at each rearward motion of the barrel ; O is a casing surrounding the barrel, which may be used, if desired, as a water jacket.

RORAIMA

OUR readers will be interested in reading the following letter, which has just been received at Kew, from Mr. im Thurn, in confirmation of his telegram already published (NATURE, vol. xxxi. p. 342), announcing his successful ascent of Roraima :—

Georgetown, *February* 4, 1885

I have just sent a most brief telegram (such things are expensive here) which will, I hope, give the first news that Roraima has been ascended ; and I much wish I could write even a brief report to go by this mail, but ever since I have been back (we got back four days ago) I have been in bed with the most severe attack of fever and ague that ever befel me, and, though the doctor assures me that I have now turned the corner, I am so weak as to be quite unable to sit up. However, before next mail I must manage something. And in the meantime I send a local paper which purports to give an account of the expedition derived from myself. The main facts are tolerably correct, but the details are much blurred.

We were quite successful in getting to the top, and have found that the plateau is by no means the isolated spot it has sometimes been supposed to be. It was, however, a great disappointment that, our way up being so extremely laborious, it would be quite impossible, without a very large expenditure in somewhat smoothing the path, to carry up hammocks, &c., provisions, and firewood (for there are no trees on the top and it is bitterly cold)—it was a great disappointment, I say, that we could only explore the top for a short distance from the point which we first reached. I see, however, no reason to believe but that the whole top is of one character. The scenery is in the highest degree wonderful. I made many fairly successful sketches (considering I am no artist), which will give a very fair idea of the mountain and of the scenery on the top. As I wish to keep the original sketches for the present, to copy them at my leisure, I have just handed the half-dozen most characteristic amongst them to a photographer here, who has before been fairly successful in copying drawings for me, and I hope to send you copies by next mail. The vegetation (on the top) is most wonderful, but somewhat scanty and quite dwarf. I have between 300 and 400 species for you. I have also some living plants (*Heliamphora*, three most exquisite Utricularias, two of which are, I fancy, new ; and a few other things), but, as these want much nursing, I have put them into wardian cases, and shall take them home for the present. (I miss Jenman now, and have throughout the expedition, immensely.)

Yours very truly,
(Signed) EVERARD F. IM THURN

NOTES

AT the moment of going to press we have received from Sir E. J. Reed a communication protesting against some of the statements made in our article last week on "The Relative Efficiency of War-Ships," and pointing out that the system of construction advocated by him was greatly modified during the ships' progress. So far from wishing to deal unfairly with Sir E. J. Reed's views, one of our chief objects was to support his protest against the existing state of things, by suggesting that scientific experiments should be resorted to to settle some of the questions on which doubts have been expressed by contending authorities.

WE regret to learn that M. Milne-Edwards is lying in a precarious condition.

OUR readers will regret to learn that Prof. Bonney will resign his post as secretary of the British Association after the Aberdeen meeting. Prof. Bonney, we believe, feels compelled to take this step mainly on account of the inroads which the work of the Association makes upon his time. No one will regret his retirement more than the council and his fellow officials.

M. BOUQUET DE LA GRYE has received a mission from the French Minister of Public Instruction to proceed to Teneriffe in order to study the variations of gravitation according to altitude.

WE have received from the Royal Society of Public Medicine of Belgium its recent monthly tables. With the present year it assumes a new field of usefulness. Founded originally in 1876, it was composed of men who by their position or special knowledge were able to participate (1) in determining the cause of mortality in general, and the circumstances which affect public health ; (2) in informing and assisting the authorities by special studies and researches ; (3) in preparing the medical topography of Belgium ; and (4) in discussing at annual public meetings questions presently relating to this work. The Society is formed of eleven local subdivisions, each sending a number of members to form the general council. But in addition to these subdivisions, for administrative purposes, the Society is also divided for the scientific service into a number of zones limited according to the physical nature of the districts. The medical topography of the kingdom, and all questions relating to it, are studied according to these zones. During last year the Society made a systematic inquiry into the sanitary situation of the country, which was highly approved of at the Health Exhibition in London, and now it has been determined to continue the investigations on a systematic and permanent basis. The members of the Society scattered over Belgium are called on to assist in the new undertaking, and the specimen forms which they are required to fill in monthly are now before us. There are thirteen zones, each zone being subdivided into districts. The physicians who are members of the Society, or who are willing to participate in its labours, are requested to state the diseases of which patients in their practice have died during the month. From these various reports a general statement, and tables of relative statistics are issued by the central body. In course of time a medical topography of the country, the enormous public advantages of which are apparent, will be issued.

THE *Transactions* of the Seismological Society of Japan for 1884 (vol. vii. part 2) contains a paper, by Prof. Milne, on 387 earthquakes observed during two years in North Japan. To determine the extent of country over which an earthquake was felt, he distributed bundles of post-cards to the Government officials at all important towns within a distance of 100 miles of Tokio, with a request that every week one of the cards should be posted with a note of any earthquakes that might have occurred. By this expedient it was discovered that the Hakme Mountains to the south of the Tokio plain appeared to stop every shock coming from the north, and accordingly the barrier of post-cards was stopped in that direction, but was extended gradually to the north until it included the forty-five principal towns in the main island to the north of Tokio, besides several places in Yezo. In Tokio, observations as to direction, velocity, and intensity were made with various earthquake instruments. A description of the principal instruments used, with a comparison of their relative merits, has already been given by Prof. Milne in vol. iv. of the *Transactions* of the Society. The second part of the paper is devoted to a list of the 387 earthquakes recorded, with particulars of each; 124 maps of earthquake districts, as well as numerous other illustrations, are appended. The results of an exhaustive study of these earthquakes may be summed up as follows:—(1) As to distribution in space: of the 387 shocks, 254 were local, that is, they were not felt over an area greater than 50 square miles; 198 of these were confined to the seaboard, and 56 were inland. The average diameter of the land surface over which the remaining 133 extended was about 45 miles, but four or five of them embraced a land area of about 44,000 square miles. These great shocks originated far out at sea, and consequently were not so alarming in their character as many which originated nearer to or beneath the land. (2) Simultaneous shocks: some of the disturbances took place at areas remote from each other, whilst intermediate stations did not record them. (3) Origins of earthquakes: the general result under this head is that the greater number of earthquakes felt in Northern Japan originated beneath the ocean; 84 per cent. of the whole having so originated. The district which is most shaken is the flat alluvial plain around Tokio. Indeed, the large number of earthquakes felt in low ground as compared with the small number felt in the mountains is very remarkable. It is also noticeable that in the immediate vicinity of active or recent volcanoes seismic activity has been small. The map marking the general distribution of volcanoes and the regions of the greatest seismic activity shows that these are not directly related to each other. The district, too, where earthquakes are the most numerous, is one of recent and rapid elevation, and it slopes down steeply beneath an ocean which, at 120 miles from the coast, has a depth of about 2000 fathoms, whilst on the other side of the country, where earthquakes are comparatively rare, at the same distance from the shore the depth is only about 120 fathoms. In these respects the seismic regions of Japan resemble those of South America, where the earthquakes also originate beneath a deep ocean, at the foot of a steep slope, on the upper parts of which there are numerous volcanic vents, whilst on the side of this ridge opposite to the ocean earthquakes are rare. (4) Relation of earthquakes to various natural phenomena: the preponderance of shocks in winter, as revealed by this investigation, is really remarkable; 278 took place in the winter months, as against 109 in the summer, and of the former number, 195, or more than half of the whole number for the two years, took place in the three coldest months of the year—viz. January, February, and March, in other words, there is a general coincidence between the maximum of earthquakes and the minimum of temperatures. But the relation of seismic *intensity* (as distinct from the number of earthquakes) is even more remarkable, for the figures show that the winter intensity is nearly three and a half times as great as the summer intensity. M. Perrey thought he discovered a maximum of earthquakes for the moon's perigee, but no such maximum has been found for Japan. Speaking generally, no marked coincidence was found in the present instance in the occurrence of earthquakes and the phases of the moon. The above are the general results, stated briefly, of the most exhaustive and remarkable study yet undertaken in the domain of seismology.

La Nature contains a long report on the Andalusian earthquakes, from the pen of M. Noguès, a mining engineer of Granada, which, as being the first scientific investigation of the catastrophe, is worthy of special notice. The whole movement presented three phases. The first manifested itself, prior to December 25, at Pontevedra, Vigo, and in Portugal; in other words, in the eastern part of the Iberian peninsula. The second was very short and intense, and made itself felt in the centre and south; it reached its maximum intensity on the night of December 25. The third phase lasts still in the provinces of Granada and Malaga, and extended east to Valencia. The oscillatory movement of December 25 embraced a considerable superficial extent. The disturbed area in the peninsula is comprised between Cadiz and Cape Gaeta, between Malaga and the Carpetena chain. The movement became more and more intense as it left this mountainous mass and travelled in a southerly direction, until it attained its maximum in the region between the Serrania de Ronda and the Sierra Nevada of Granada. The oscillatory motion was gradually accentuated towards the south, especially on the southern side of the great central Spanish plateau, bounded by the slope of the valley of the Guadalquivir (Seville, Cordova, Malaga, and Granada). M. Garcia Alvarez localises the phenomenon in Andalusia, and regards the Sierra Nevada as the point of departure. M. Noguès then deals in succession with the relations between the seismic motions and the geological structure of the district, the geological phenomena, such as fissures in the earth, produced by the earthquakes, and alterations caused by them in the level of springs. He sums up his conclusions by pointing out that the geological observations which have been made so far, although local, limited, and imperfect, demonstrate that there were two different kinds of motions—one oscillatory, the other a trembling movement. Every one who felt the great earthquake of the 25th experienced first a vertical shock, and then, after a short interval, another movement like a balancing. A great fissure at the village of Guevejar presents at two points two interesting sections. At one the trunk of an olive-tree has been split in two from its root to the branches, as if from a blow of a hatchet, each part occupying a side of the fissure, one on one side, the other on the other. At another part the fissure has divided in two the wall supporting the wheel of the powder-factory at Guevejar. The cracks in the houses in the village are in lines parallel to these fissures, and the marks left in the soil indicate an oscillatory motion. The chimneys, in many cases, were turned half around on their axes, without any further disturbance of a single portion of the structure; and, in fact, an examination of the various marks left by the earthquake of December 25 places it beyond doubt that there was a trembling as well as an oscillatory movement.

ON Wednesday evening last week, at half-past eight, three heavy shocks of earthquake, lasting for two seconds, and passing from west to east, were felt at Temesvar, in Southern Hungary. On Thursday morning there was another and slighter shock. Two sharp shocks were felt on Friday in Spain, most severely in Granada, Loja, Alhama, and other districts on both sides of the Sierra Tejea. In the Provinces of Granada and Malaga many houses were damaged, and buildings that had suffered in the previous earthquake were now knocked down.

THE last number of the *Bulletin* of the Essex Institute (Salem, July to December, 1884) is of especial interest, as it contains the proceedings held in commemoration of the fiftieth anniversary of the foundation of the Essex County Natural History Society, of which the Institute is the natural heir and successor. The papers which were read were all appropriate to the occasion. Prof. Morse dealt with the condition of zoology fifty years ago and now, in connection with the growth of the Institute. Mr. Robinson discussed the progress of botany in Essex county during the half-century, and the influence of the Society on it, dividing his paper into three parts: (1) The condition of botanical knowledge now as compared with fifty years ago; (2) the progress made in that period in the district, as shown by the increase of libraries, public museums, private herbaria, &c.; and (3) the practical benefit and general knowledge bestowed upon the people of the county by such increased accurate knowledge of the subject and the facilities for obtaining it. It would be impossible to sum up more clearly and thoroughly, from all points of view, the benefits of such societies as the Essex Institute to their localities and to the progress of science in general than is done in this paper. Mr. McDaniel deals with geology and mineralogy, in which the work has not been so great as in botany, zoology, and prehistoric archæology, "owing to the bent and profession of the leading members." The commemoration papers conclude with a brief historical sketch by Mr. Samuel Fowler, who notices as evidence of the liberality of the founders of the Society that, though nothing was heard of women's rights fifty years ago, they invited ladies to join them, adding in their circular: "It is anticipated that they will contribute much to the success of the Society." The historiographer is able to add that these anticipations were realised, for "ladies have always taken a deep interest in the Society and its work, and have greatly aided us in many ways." The result of this "stock-taking" after half a century is a legitimate source of pride to the inhabitants of the good old town of Salem and its neighbourhood.

IT will interest many of our readers to know that an Exhibition of Photographs by Amateurs will be held at 103, New Bond Street, from April 23 to May 9, under the auspices of the "London Stereoscopic Company." This, as far as we know, will be the first of its kind, and will doubtless be patronised by a large number of exhibitors, and tend to encourage the growing popularity of photography amongst amateurs. Several photographs by the late Mr. Cameron, of the *Standard*, will form an interesting feature of the Exhibition. Prizes to the value of 200*l*. will be awarded. Intending exhibitors are requested to communicate with Mr. T. C. Hepworth, at 108, Regent Street, W.

THE popular Chinese practice and superstition with regard to persons in an epileptic fit are not a little curious. When a person gets an attack of epilepsy, those about him rush away for a few blades of grass, which they put into his mouth. They believe that during an attack of epilepsy the spirit leaves the body, and, there being a vacancy within, it is immediately filled by the spirit of an animal, generally a sheep or a pig, and the sound in the person's throat as he begins to revive is taken for the bleating of the one or the grunting of the other. Under these circumstances they attempt to propitiate the animal by putting grass into the man's mouth, possibly under the impression that they can entice the animal's spirit in the man to remain till his own returns; and on no consideration will they remove him till the fit is over, for, if they did, they believe his own spirit would not be able to find him again, and thus he would die.

MESSRS. W. SWAN SONNENSCHEIN & CO. will shortly publish a translation, by Prof. Hillhouse, M.A., of the Mason Science College, of Strasburger's "Das kleine botanische Practicum."

THE next Ordinary General Meeting of the Institution of Mechanical Engineers will be held on Friday, March 20, at 25, Great George Street, Westminster. The chair will be taken at 7.30 p.m. by the President, Mr. Jeremiah Head. The following papers will be read and discussed, as far as time will admit:—On recent improvements in wood-cutting machinery, by Mr. George Richards, of Manchester (adjourned discussion); description of the tower spherical engine, by Mr. R. Hammersley Heenan, of Manchester; on the history of paddle-wheel steam navigation, by Mr. Henry Sandham, of London.

THE Annual Report of the Belfast Naturalists' Field Club is a respectable volume of about 260 pages, with twenty-four plates containing about fifty illustrations, devoted in the present number wholly to cromlechs and other prehistoric remains in the north and west of Ireland. The Society has attained its majority (the past year being its twenty-first), and the secretary is able to report that it was never more prosperous, either as regards increased membership, financial condition, or the value of the work done. Among the papers read during the winter session we notice: on the antiquities of the West of Ireland, on a microscopical examination of a bit of groundsel, Magilligan strand after a storm (in which Canon Grainger describes the castaways after a gale), ants, a trip to America, the age of the basalts of the North-East Atlantic (by Mr. J. Starkie Gardner), while the appendix contains three longer papers:—Notes on Irish coleoptera, by Messrs. Hallilay and Stewart; the cromlechs of Antrim and Down, by Mr. Gray; and notes on prehistoric monuments at Carrowmore, near Sligo, by Mr. Elcock. It is to the two last that the numerous illustrations are attached.

M. WALDEMAR CZERNIAWSKY, already known for his works on the fauna of the Black Sea, has now published at Kharkoff a work on the "Crustacea decapoda Pontica littoralia," accompanied by several plates, being a very elaborate description of the Black Sea Decapods. The number of Pontic species of Decapods has been increased by twenty, reaching thus forty-eight species, with numerous varieties, though it will probably be greater when the depths of the Black Sea have been better explored. The results of this work are numerous and interesting. The species offer altogether a very great variety of forms. The Black Sea contains the local forms of Mediterranean varieties, while in the Celtic region are found the local forms of other varieties. The author asserts that the metamorphosis of the superior crabs, such as *Carcinus*, which presents nine different stages, are a repetition of their genealogy, and arrives at a series of very interesting conclusions as to the genealogy of different species. All three species of *Astacus* which are found in the Ponto-Caspian fauna are maritime forms which have immigrated into sweet water, and even the *Astacus pachypus*, Rathke, of the mountain-lake Abrau, is a remainder of a maritime fauna; so also *Thelphusa*, which has gigantic representatives in the South Caspian. Certain crabs reach really gigantic size in the Ponto-Caspian region; such as *Eriphia spinifrons* and *Carcinus mænas* on the shores of Crimea and at Odessa. While most crabs reach a great development only in very salt and warm water, others reach the same size under the influence of reverse conditions. The Decapods of the Azov Sea have not yet been explored. The descriptions of the species and their varieties being given in Latin, as also the explanations to the plates, the work is rendered accessible to all zoologists, many of whom, however, will regret not to be able to understand the notes (mostly zoo-topographical, and sometimes adding minor details to the description), which are in Russian.

WE have received from the Johns Hopkins University the two last of the Studies on Historical and Political Science. One deals with land laws in mining districts, and describes the regulations for the use of land made by agreement among the miners themselves in the Western States. They show a return to primitive ideas, where use is made the proof of ownership, and equality in the size of the various lots is of prime importance. Mr. Shinn is the author of this number. The second, by the editor, Dr. Adams, describes the influence of the State of Maryland upon the land cessions of the United States, and is specially interesting for its references to Washington's project for devoting the present made to him by his native State, Virginia, to the establishment of a National University.

WITH the exception of a few pages, the whole of the last number (vol. vi. No. 4) of the *Boletin de la Academia Nacional de Ciencias* of Cordova (Argentine Republic) is occupied by a paper by M. Oscar Dœring on meteorological observations made by him at Cordova during 1883. These were a continuation of those made by him in 1882 on evaporation, and the various temperatures at six different depths. But for 1883 he has added other observations and arranged the tables as follows:—Atmospheric pressure, temperature of the air, the elastic force of the atmospheric vapour, relative humidity, evaporation in the shade and in the sun, temperature of the soil, solar radiation, storms, and rainfall. There is also a short paper on the observations of the German expedition to Bahia Blanca, to observe the transit of Venus.

THE additions to the Zoological Society's Gardens during the past week include two Wood Hares (*Lepus sylvaticus*) from North America, presented by Mr. F. J. Thompson; an Alexandrine Parrakeet (*Palæornis alexandri* ♀) from India, presented by Mr. W. Hay; a Common Magpie (*Pica rustica*), British, presented by Mr. H. Clare; a Slowworm (*Anguis fragilis*), British, presented by Mr. R. Gunter; a Short-tailed Wallaby (*Halmaturus brachyurus*) from Western Australia, deposited; two Brown Pelicans (*Pelecanus fuscus*) from the West Indies, purchased; an Isabelline Lynx (*Felis isabellina* ♂) from Tibet, received in exchange; two Spotted Ichneumons (*Herpestes nepalensis*) from Assam, received on approval.

OUR ASTRONOMICAL COLUMN

A COMET IN 1717.—In a note to the Royal Society (*Phil. Trans.*, No. 354) Halley reported that on Monday, June 10, 1717, in the evening, the sky being very serene and calm, he was desirous of examining Mars, then very near the earth, to ascertain whether in his 20-foot telescope he could distinguish the spot said to be seen upon his disk, and directing his telescope for that purpose he accidentally met with a small whitish appearance near the planet, which seemed to emit from its upper part a short kind of radiation, directed nearly towards the point opposite to the sun. The great light of the moon, then not far from full, and close at hand, hindered the object from being distinctly seen, but he determined its place to be nearly in 17° 12′ of Sagittarius with 4° 12′ south latitude. The position, he adds, would be more exactly found by means of two small stars near it, the more northerly of which had the same latitude and followed at the distance of about six minutes; the other was about four minutes south of the former, and followed it about a minute, "the angle at the northern star was somewhat obtuse, of about 100 degrees, and the distance of the nebula from it was sesquialteral to the distance of the two stars, or rather a little more." No motion being detected in over one hour, Halley doubted if it were a comet, but on June 15, the moon being down and the sky clear, he had a distinct view of the two stars, but there was no sign of the nebulosity where it had been observed on June 10. He was led by this circumstance to remark upon the number of comets which might escape notice, from their being telescopic objects, and adds that, although comets had been seen elsewhere in 1698, 1699, 1702, and 1707, he could not learn that any comet had been perceived in this country for the thirty-five years previous to the observation above described, which implies that none had been seen here since the year 1682, that of the appearance of the famous comet which bears Halley's name.

The small stars to which Halley refers would appear to be Nos. 16,627 and 16,631 in Oeltzen's Argelander.

THE VARIABLE STAR S CANCRI.—A minimum of this short-period variable being due during the night of February 20, Mr. Knott availed himself of a fine sky at Cuckfield to observe it as long as it was possible to do so. The watch commenced at 8h. 40m., and ended at 17h. 15m. At 9h. 23m. no change was noticeable, but soon after 9h. 30m. the star began to decline, and gradually fell from 8·1 to 10·4 mag., which point was reached about 15h. 30m. From that time till 17h. 15m. no certain change was detected, though at 17h. 15m. there was a suspicion of the star being possibly a trifle brighter. By this time it was 17h. past the meridian, and getting too low for observation. As it was not possible to follow the star till its advance on the rising curve, Mr. Knott was unable to fix the time of minimum with certainty, but considered the predicted time (16h. 22m.) was pretty correct. He remarks further that Prof. Schönfeld gives 8½h. as the time of decrease, and 13h. as that of increase. If this held for the minimum of February 20, and the decrease began at 9h. 30m., the minimum would not be reached before 18h., and the normal magnitude would not be attained before February 21, 8h. At 6h. 30m. on the latter date he doubted whether the star had recovered its normal brightness, but by 7h. or 7h. 30m. there seemed no doubt about it. Comparing the form of his curve with Prof. Schönfeld's, it appeared that on this occasion the star was longer in falling from 9·4 and 9·9 m. to the lowest point reached, than the observations of Prof. Schönfeld indicated; but Mr. Knott writes doubtfully upon this point, not having previously watched S Cancri through its changes. The next minimum may be expected on March 11, between 15h. and 16h. Greenwich time.

THE MELBOURNE OBSERVATORY.—We have received the nineteenth annual report of the Government Astronomer of Victoria to the Board of Visitors of the Melbourne Observatory. The new transit circle of 8 inches aperture, constructed for that establishment by Mr. Simms, was received in May last, and the mounting was completed early in July. At the time of drawing up the report (August, 1884) there were only wanting some steps and observing chairs, for the instrument to be brought into regular use. It is stated to be very similar in form and dimensions to the transit circles constructed by the same firm for the observatory at Cambridge and for that of Harvard College, U.S. The great reflector was in better condition than at the date of the previous report, nevertheless it is proposed to send the two specula, one after the other, to England, to be repolished. A number of stars selected by Prof. Auwers had been observed with the old transit circle, to assist in the formation of a fundamental catalogue of southern stars. Mr. Ellery mentions those of Herschel's nebulæ, which had been observed, and of which drawings had been made with the great telescope; the nebula of η Argûs, 30 Doradus, and the "Horseshoe" nebula are included in his list. Pons' comet was observed for position from January 6 to March 18. The completion of the telegraphic determination of Australian longitudes, it is reported, was only waiting a new series of exchanges between Sydney, Adelaide, and Melbourne; New Zealand had been connected with Sydney by a most successful set of time-exchanges through the cable. The connection of Brisbane and Sydney was in progress, and, on this being completed, there would only remain to connect Western Australia, to have the longitudes of all the chief Australian and New Zealand cities and ports determined upon the same system.

Mr. Ellery recommends that a small expedition should be despatched from Melbourne to New Zealand for the observation of the total eclipse of the sun on September 9 in the present year, when the central line passes through Cook's Straits. Sir W. Jervois, the Governor of New Zealand, had promised all the aid he could render in the matter. The Board of Visitors supported an application to the Government of Victoria for the necessary funds. [Full details of the circumstances of this eclipse were given by Mr. Hind in the *Monthly Notices* of the Royal Astronomical Society for January last.]

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, *MARCH* 8-14

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 8

Sun rises, 6h. 31m. ; souths, 12h. 10m. 51·6s. ; sets, 17h. 51m. ; decl. on meridian, 4° 42′ S. : Sidereal Time at Sunset, 4h. 57m.

Moon (at Last Quarter at 19h.) rises, 1h. 5m. ; souths, 5h. 40m. ; sets, 10h. 12m. ; decl. on meridian, 17° 25′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on Meridian ° ′
Mercury	6 36	11 56	17 18	8 22 S.
Venus	6 13	11 18	16 23	11 26 S.
Mars	6 28	11 52	17 16	7 44 S.
Jupiter	15 46	22 58	6 10*	13 8 N.
Saturn	9 56	18 0	2 5*	21 41 N.

* Indicates that the setting is that of the following nominal day.

Occultations of Stars by the Moon

March	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
10	B.A.C. 6287	6	4 25	5 38	90 228
10	B.A.C. 6292	6	5 6	6 26	54 280
11	ρ′ Sagittarii	4	5 18	6 38	60 272

Phenomena of Jupiter's Satellites

March	h. m	
8	2 46	II. ecl. reap.
	6 12	IV. occ. disap.
9	20 8	II. tr. egr.
10	5 23	III. occ. disap.
11	6 0	I. occ. disap.
12	3 19	I. tr. ing.
	5 38	I. tr. egr.
13	0 27	J. occ. disap.
	3 14	I. ecl. reap.
	19 1	III. tr. ing.
	21 45	I. tr. ing.
	22 39	III. tr. egr.
14	0 5	I. tr. egr.
	18 53	I. occ. disap.
	21 43	I. ecl. reap.

The Occultations of Stars and Phenomena of Jupiter's Satellites are such s are visible at Greenwich.

March 13, 19h.—Mercury in superior conjunction with the Sun.

RECENT ENGINEERING PATENTS[1]

SIR FREDERICK BRAMWELL stated that he had been determined in his choice of a subject by the consideration that H.R.H. the Prince of Wales had seen fit to appoint him chairman of the Executive Council of the International Inventions Exhibition, to be held at South Kensington this year. He therefore proposed to direct attention to some of those objects that ought to be contributed to that Exhibition which were more particularly connected with civil engineering.

Dealing, first, with materials of construction, the President remarked that probably few materials had been more generally useful to the civil engineer, in works which were not of metal, than Portland cement. During the last twenty-two years great improvements had been made in the grinding and in the quality of the cement. As regards bricks, although not now superior in quality to those made by the Romans, there was progress to be noted in the mode of manufacture and the materials employed. The brick-making machine and the Hofmann kiln had economised labour and fuel, while attempts were being made to utilise the waste of slate quarries. Certain artificial stones appeared at last to be produced with such a uniformity and power of endurance as to compare favourably with the best natural stone, or were even better, for they could be produced of the desired dimensions and shape, and were thus ready for use, without labour of preparation. The employment of wood, except in newly-developed countries, was decreasing, for one reason, because it was practically impossible so to use it as to obtain anything approaching to the full tensile strength. Many attempts had been made to render timber proof against rapid decay and ready ignition, and it was in these directions alone that progress could be looked for. With respect to preservation from fire, the wooden structures of the Health Exhibition were coated with asbestos paint, and to this their escope from destruction by a fire was due. Leaving the old-world materials of stone and wood, attention was directed to that form of iron known as steel. The President remarked that, in his judgment, the making of steel in crucibles was not so satisfactory a mode of obtaining uniformity in large masses as was either of the other two great systems of manufacture—the Bessemer and the Siemens—the two processes which had changed the whole complexion of the iron industry. He further said that, eight years ago, in a lecture he delivered at the Royal Institution, he had ventured to predict that steel made by fusion would supersede iron made by the puddling process, and that the use of iron so made would be restricted to the small articles produced by the village blacksmith. The first important revelation in steel manufacture was the ingots shown by Krupp, with other products, in the Great Exhibition of 1851. These showed an enormous step at the time when the production of steel involved the employment of the crucible. Within the last eight years a great improvement had been made by Messrs. Thomas and Gilchrist, by which it had been rendered possible to employ successfully, in the production of steel, iron derived from ores that, prior to the date of this invention, had been found wholly inapplicable for the purpose. In the manufacture of pig-iron improvement had been effected by increasing the dimensions of the furnaces and the temperature of the blast, by the better application of chemistry to the industry, by the total closing of the bottom of the furnace, and by the greater use of the waste gases. Copper, so long used in its alloyed condition of "gun-metal," had, within the last few years, been still further improved by alloying it with other substances so as to produce "phosphor-bronze" and "manganese-bronze," very useful materials to those engaged in the construction of machinery. With the increased dimensions of the main-shafts of engines, and of the solid forgings for the tubes of cannon, obtaining at the present day, composed, as they were, of steel, the operations of light steam-hammers were absolutely harmful, and the blows of even the heaviest hammers were not so efficacious as was pressure applied without blow. The time was not far distant when all steel in its molten state would be subjected to presusre, with the object of diminishing the size of any cavities containing imprisoned gases.

Within the period under consideration the employment of testing-machines had come into the daily practice of the engineer, for determining, experimentally, the various physical properties of materials—and of those materials when assembled into forms to resist strain, as in columns or in girders.

In those matters which might be said to involve the principles of engineering construction, there must of necessity be but little progress to note. Principles were generally very soon determined, and progress ensued, not by additions to the principles, but by improvement in the method of giving to those principles a practical shape, or by combining in one structure principles of construction which had hitherto been used apart.

Taking up, first, the subject of bridge construction—the President thought the St. Louis bridge might fairly be said to embody a principle, novel since 1862, that of employing for the arch ribs tubes composed of steel staves hooped together. Further, in suspension bridges, there had been introduced the light upper chain, from which were suspended the linked truss-rods, doing the actual work of supporting the load, the rods being maintained in straight lines, and without flexure at their joints due to their weight. In the East River Bridge at New York, the wire cables were not made as untwisted cables, and then hoisted into place, imposing severe strains upon many of the wires, but the individual wires were led over from side to side, each having

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[1] Abstract of Presidential Address at the Institution of Civil Engineers, by Sir Frederick J. Bramwell, F.R.S., on January 13.

the same initial strain. So far as novelty in girder-construction was concerned, the suspended cantilever of the Forth Bridge, now in course of construction, afforded the most notable instance. It was difficult to see how a rigid bridge, with 1700 feet spans, and with the necessity for so much clear headway below, could have been devised without the application of this principle. A noteworthy example of the use of pneumatic appliances in cylinder-sinking for foundations was also in progress at the Forth Bridge. At the New Tay Viaduct, the cylinders were being sunk while being guided through wrought-iron pontoons, which were floated to their berths and were then secured at the desired spot by the protusion, hydraulically, of four legs, which bore upon the bottom, and they, until they were withdrawn, converted the pontoon from a floating into a fixed structure.

The President next traced the contest between canals and canalised rivers as modes of internal transit, in contrast with railways, and referred to the improved rate of transport on canals by the substitution of steam- for horse-haulage, and by a diminution in the number of lockages. He also alluded to the hydraulic canal lift on the River Weaver, and to a similar application in the Canal de Neufossé, in France, for overcoming a great difference of level, and reducing the consumption of water and the expenditure of time to a minimum. The great feature, however, of late years in canal engineering, was not the preservation, or improvement, of the ordinary internal canal, but the provision of canals such as the completed Suez Canal, the Panama Canal in course of construction, the contemplated Isthmus of Corinth canal—all for saving circuitous journeys in passing from one sea to another—or in the case of the Manchester Ship Canal, for taking ocean steamers many miles inland. The rivalry between canal-engineers and railway-engineers was illustrated by the proposal to connect the Atlantic and Pacific oceans by means of a ship-railway, the details of which scheme were before the public.

In harbour-construction, the principle adopted in the Liffey at Dublin was referred to, where cement-masonry was moulded into the form of the wall, for its whole height and thickness, and for such a length forward as could be admitted, having regard to the practical limit of the weight of the block. The block was then carried to its place, was lowered on to the bottom, which had been prepared to receive it, and was secured to the wall by groove and tongue. The apparatus by which the blocks, weighing 350 tons each, were raised, and transported to their destination, was then described.

Consideration of sub-aqueous works necessarily led to appliances for diving; and here the President said a few words about the "bateau-plongeur" used on the "barrage" of the Nile. Beyond improvement in detail and the application of the telephone, there was probably no novelty to record in the ordinary dress of the diver. But one great step had been made in the diver's art by the introduction of the chemical system of respiration. A perfectly portable apparatus had been devised, embracing a chemical filter by which the exhaled breath of the diver was deprived of its carbonic acid. The diver also carried a supply of compressed oxygen to be added to the remaining nitrogen, in substitution for that which had been burnt up in the process of respiration. Armed with this apparatus, a diver during one of the inundations which occurred in the construction of the Severn tunnel, descended into the heading, proceeded along it for some 330 yards (the depth of the water above him being 35 feet) and closed a sluice-door through which the water was entering the excavations, and thus enabled the pumps to unwater the tunnel. Altogether, this man was under water for one hour and twenty-five minutes without any communication with those above.

There were, happily, cases of sub-aqueous tunnelling where the water could be dealt with by ordinary pumping power, and where the material was capable of being cut by a tunnelling machine. In the Mersey Tunnel, in the New Red sandstone, a heading 7 feet 4 inches in diameter, a speed of 10 yards in 24 hours had been averaged, while a maximum of over 14 yards had been attained. In the experimental Channel Tunnel in a 7-feet heading in the gray chalk, a maximum speed of 24 yards had been arrived at in the 24 hours on the English side, and on the French side of 27⅓ yards in the same time. In ordinary land-tunnelling, since 1862, there had been great progress, by the substitution of dynamite, and preparations of a similar nature, for gunpowder, and by improvements in the rock-drills worked by compressed air, used in making the holes into which the explosive was charged. In boring for water, and for many other purposes, the diamond drill had proved of great service. Closely connected with tunnelling-machines were the machines for "getting" coal, which, worked by compressed air, reduced to a minimum the waste of coal, relieved the workman of a most fatiguing labour in a constrained position, and saved him from the danger to which he was exposed in the hand operation. The commercial failure of these machines was due to trade opposition, and it was to be feared that like prejudices would prevent the introduction of the lime-cartridge in lieu of gunpowder.

With regard to the great source of motive power—the steam-engine—it was difficult to point to any substantive novelty since 1862. But the machine had been more and more scientifically investigated, and the results had been practically applied with corresponding advantages. The increase in initial pressure, the greater range of expansion, the steam-jacketing of the vessels in which the expansion took place, had all led to economy. Double-cylinder non-condensing engines were now currently produced, which worked with a consumption of only 2¾ lbs. of coal per I.H.P., or 2·7 lbs. per H.P. delivered off the crank shaft, equal to 82 millions of duty on the Cornish-engine mode of computation. When these results were augmented by the employment of surface-condensation, an I.H.P. had been obtained for as low as 1½ lbs. of coal, and it was commonly obtained, in daily work, for from 2 lbs. to 2½ lbs. But in the use of steam as a heat-motor, the largest portion of the heat passed away unutilised. This defect had been sought to be overcome by a regenerative steam-engine, but it was not successful. Heated-air engines had hitherto only been found applicable where small power was required. Another form of heat-motor—the gas-engine—was daily coming into general use up to 30 I.H.P.. By a change in the mode of burning the mixture, and of utilising the heat thereby generated, the injurious shock of the early forms of gas-engine, and their large consumption of gas, were obviated. Comparing a gas-engine with a non-condensing steam-engine consuming 5 lbs. of coal per I.H.P. per hour, and demanding therefore, at one shilling per cwt., only one halfpenny for the purchase of coal, the extra cost for working the gas-engine was well repaid by the saving of boiler-space, of the wear and tear of the renewal of the boiler, of the consumption of coal while getting up steam and during meal-times, of the saving of wages, of the freedom from boiler explosions, and of the cessation of smoke production. A motor had been recently tried where no fuel was employed directly, but where a boiler, being filled with water and steam under pressure, had its heat maintained by exposing caustic soda, contained in a vessel surrounding the boiler, to the action of the waste steam from the engine, the result being that, as the moisture combined with the caustic soda, sufficient heat was developed to generate steam and keep the engine working for some time. Trials had been made with this motor for propelling a launch and for working a tramcar.

With respect to other motors, viz. those driven by wind or by water, in France an improvement had been made in water-wheels by which it was asserted that 85 per cent. of all the energy residing in a low fall of water had been converted into power. In turbines also there had been considerable development during the last twenty-two years, and they were very efficient where a high fall of water had to be utilised, or where, in the case of a low fall, great difference in the working head, and in the level of the tail-water, had to be provided for.

Next to the subject of motors came the transmission of power. In its restricted sense, the transmission from one part of a machine to another, reference might be made to the increasing use of multiple-rope driving-gear in lieu of belts, to inclined spur-gear for diminishing noise, and to that kind of frictional gearing to which the name of "nest-gearing" had been given. Where, however, the transmission was to long distances, means were being adopted for supplying power—*i.e.* water under pressure or compressed air—through mains laid down in the streets, in a manner similar to that in which gas and water were now supplied for domestic use; and in New York and other cities of the United States high-pressure steam was similarly conveyed and delivered to the consumers, both for power and for heating.

Sir Frederick Bramwell also remarked upon the continuous rolling of bars of steel for tyres, upon the right way of making boiler-shells and boiler-flues, upon tidal motors, upon "dirigible" balloons, upon the Maxim machine-gun, and upon the application of submarine mines and torpedoes for the defence of seaports. In regard to waterworks, he could not adduce any material improvements in those dependent upon storage, or in pumping machinery; but in the matter of house-fittings there had been great progress, especially in the detection and prevention of waste of water. With respect to gas as a distributed illuminant, considerable improvements had lately been made, due to a greater liberality on the part of lighting-authorities, and to the use of multiple burners in street-lanterns, by which a greater amount of light was obtained from the same volume of gas. The regenerative gas-burners, and other modes, promised largely to increase the candle-power per cubic foot of gas burnt.

In conclusion, the President stated that, during his term of office, he would do all that lay in his power, as he had done in the past, to uphold the honour, the dignity, and the usefulness of the Institution; and in these efforts he felt satisfied that all the members would cheerfully and gladly assist.

HOW THOUGHT PRESENTS ITSELF AMONG THE PHENOMENA OF NATURE[1]

EVERY phenomenon which a human being can perceive may be traced by scientific investigation to motions going on in the world around him. This is obvious to every scientific man in regard to such phenomena as those of colour and sound, and these simpler cases were first adduced by the lecturer. He then pointed out that the statement is also true of all other material phenomena, and be specially dwelt on the phenomena investigated in the science of mechanics, showing that all the quantities treated of in that science, such as force and mass, prove, when the investigation is pushed far enough, to be expressible in terms of mere motion. He also showed that the prevalent conviction that motion cannot exist unless there is some "thing" to move will not stand examination. It proves to be a fallacious conviction traceable to the limited character of the experience of motions which we and our ancestry from the first dawn of organised thought on the earth have had within reach of our senses. This conviction accordingly has no authority with respect to molecular motions and to some others that have been brought to light by scientific study. He also showed that the "thing" which in common experience moves, proves in every case to be nothing else than these underlying molecular motions, the transference of which from place to place is the only kind of motion which common experience can reach, when unassisted by science.

The intermediate steps between the world external to our bodies and the brain which take place in our organs of sense and nerves can also be ascertained to be motions. And finally, a change consisting of motions takes place in the brain itself, whereupon we become conscious of thought: *i.e.* a change occurs within the brain which would be appreciated as motions by a bystander who could search into our brains while we are thinking, and could witness what is going on there, while all the time the change that we experience is thought. It must be borne in mind that our brain is a part of the external world to the bystander whom we have supposed to be observing what is going on in it. It thus appears that every phenomenon of the external world is reducible to motions and their modifications, while all that is within the mind is thought.

Now this motion to which all other material phenomena are reduced, this motion as it exists in nature, must be distinguished from man's conception of motion, which, after all, is one of his thoughts—a very complex one, no doubt, but not part of the external world. This particular conception in our minds is one remote effect of the motion as it exists outside us, and what we really know of that external cause is that it is a cause which does unfailingly produce this effect if the intermediate appliances of our senses and nerves are also present. Motion, the cause, must no doubt stand in absolutely rigorous relations to its effect, viz. our conception of motion: but it need not be like its effect, the presumption being quite the other way. The lecturer pointed out that, under these circumstances, the simplest and so far the most probable, hypothesis that can be advanced is the monistic hypothesis that this unknown cause is itself thought; and he pointed out that it is no objection to this view that we are unconscious of all the thought here supposed, for this is only to say that it is external to that particular group of interlacing and organised thoughts which we call our own mind, just as the thoughts of the many millions of our fellow-men and of all other animals are external to our little group.

The lecturer accordingly recommended the following hypothesis: (1) as consistent with everything we know, (2) as the simplest hypothesis, (3) as an hypothesis which dispels all the difficulties that encumber the dualistic supposition that there are two kinds of existence, viz. the hypothesis that if a bystander were armed with adequate appliances to ascertain what is going on in our brain while we are thinking, then 'what we should experience to be thought is itself the remote cause with several intermediate causes of that change within the observer's brain which determines his having that complex thought which he would call perceiving some of the motions in our brain—in short, that what he appreciates as motion we experience to be thought.

If this view be correct, it will follow that the thoughts of which we are conscious are but a small part of the thought going on even in our own brain, and which would be seen by a beholder as motions, the rest being unconscious cerebration and as much outside our consciousness as are the thoughts of other people. We are led also to the conclusion that the thought which is going on in the brains of all the animals that exist is but the "small dust of the balance" compared with what is going on throughout the rest of the mighty universe.

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[1] Short Abstract of Royal Institution Friday evening discourse (February 6), by G. Johnstone Stoney, M.A., D.Sc., F.R.S.

SCIENTIFIC SERIALS

THE *American Journal of Science*, February.—Obituary notice of Benjamin Silliman, son of Benjamin Silliman, the founder of that *Journal*, and long one of its editors, who died in his sixty-ninth year at New Haven, Connecticut, on January 14, 1885.—The organisation and plan of the United States Geological Survey, with a map, by J. W. Powell. The organisation, as at present established, comprises: (1) an astronomic and computing division, the officers of which are engaged in determining the geographic coördinates of certain primary points; (2) a triangulation corps engaged in extending a system of triangulation over various portions of the country from measured base-lines; (3) a topographic corps, organised into twenty-seven parties scattered over various portions of the United States.—Memorial of the late distinguished botanist, George Bentham, by Asa Gray.—Palæontological notes on the material from the St. John group of New Brunswick contained in the Hartt Collection at Cornell University, by Charles D. Walcott.—On the rotation of the equipotential lines of an electric current by magnetic action, by E. H. Hall. The results are given of experiments made during the month of August, 1883, and at intervals since in the physical laboratory of Harvard College, the substances examined being chiefly copper, zinc, certain of their alloys, iron, and steel.—On the use of the term "Esker, or Kam drift," by J. Henry Kinahan. Both terms are traced to a Keltic source, *căm*, short (not *kāme*, long, as wrongly pronounced in England and the Lowlands), meaning, in Irish, *crooked* or *winding*, as in the river Cam, while *Eskir* or *Eiscir* denotes a small but well-defined ridge.—On the cause of mild polar climates, by James Croll. In this third paper the author discusses the climate of the Tertiary period in so far as affected by eccentricity, the evidence of climatic alterations and of glaciation during the same period.—Notice of the remarkable marine fauna occupying the outer banks of the southern coast of New England, by A. E. Verrill.—Note on a fossil coal-plant found at the graphite deposit in mica schist at Worcester, Massachusetts, by Joseph H. Perry.—The test-well in the Carboniferous formation at Brownville, Nebraska, by Prof. L. E. Hicks.—Review of Hill's supplement to Delaunay's "Lunar Theory," by John N. Stockwell.

THE *Journal of Botany* for February contains a plate of several new or rare species of Desmid to illustrate one of a series of papers on these organisms, by Mr. W. Joshua. It contains also the annual list of new flowering plants published in periodicals in Britain in 1884. Most of the other articles are descriptive.

Bulletin de l'Académie Royale de Belgique, December, 1884.—On the microscopic intrusions of sagenite in the titaniferous oolitic hematite of the clay-slates, by A. F. Renard.—On the external branchial apertures of the Ascidians, and on the formation of the intestine in *Phallusia scabroïdes* (new species), by Edouard Van Beneden and Charles Julin.—On certain new animal organisms forming a local fauna peculiar to the neighbourhood of Thornton Bank, by Ed. Van Beneden.—On the presence of *Niphargus puteanus*, Sch., in the Liége district, by Ed. Van Beneden.—Action of high pressure on the vitality of yeast, and on the phenomena of fermentation, by A. Certes and D. Cochin.—On the presence of duodenal anchylostoma in some Belgian hospitals, by Ch. Fisket.—On the presence of a coxal gland in *Galeodes araneoïdes*, by J. MacLeod.—Note on G. Edon's work on the Carmen Arvale, by Alph. Le Roy.—Some details on Wissant and its identification with the Portus Iccius of the Romans, by Alph. Wauter.—On the apparent enlargement of the orbs of the sun and moon, by Paul Stroobant.—On a new *Balenoptera rostrata* in the Mediterranean, by J. Van Beneden.—Discourse on geological chronology, by Ed. Dupont.—On the chief cause of cyclones and tropical calms, by M. Folie.

Bulletin de l'Académie des Sciences de St. Pétersbourg, tome xxix. No. 4.—On the applications of the interpolation method proposed by M. W. Tchebychef, by O. Backlund (in German). The fine method of the Russian mathematician is shown to be easily represented in a simple scheme, appropriate to calculations, and the author applies it to three examples, one of which is the calculation of Hasselberg's spectral observations. He shows that, with regard to the easiness and simplicity of calculations, the Tchebychef method leaves nothing to desire, while its results are as reliable as those obtained by the much more tedious method of least squares. Two other examples, one for the declinations as taken from the Cape Catalogue, and compared with those measured at Pulkova, and another for interpolating Pulkova double-star observations, give the same satisfactory results. As known, Tchebychef's method permits also to proceed without making any previous hypothesis as to the degree of the interpolation formula. On the whole, when a considerable number of data is given, and the least squares' method becomes especially tedious, Tchebycheff's method gives excellent results.—The elements and the ephemerides of the Encke comet for its appearance in 1884-85, by O. Backlund. The ephemerides are given from November 7, 1884, to May 6, 1885.—Demonstration of several theorems relative to the numerical function $E(x)$, by V. Bouniakovsky (in French).—Contributions to the Ornis of the Ternate Island, by Th. Pleske (in German). The birds brought from the above island by Dr. Fischer are determined with the help of Salvadori's "Ornithologia della Papuasia," &c. There are eighty-five species described.—Remarks on the *Elapomorphus* genus of Calamaride serpents, by A. Strauch (in German) Having received an herpetologic collection from Brazil, from Dr. Ihering, Prof. Strauch found in it a new species of *Elapomorphus*, which he describes under the name of *E. Iheringii*, and he accompanies the description by a thorough critical revision of all known species of the same genus. The paper is thus a systematic monograph of the genus, which contains now eighteen species.

THE *Belgique horticole* for July to September, 1884, contains a retranslation of Prof. Jacobsthal's essay on "The Evolution of Vegetable Forms in Decorative Art," and M. Guirand's on the gardens of the Mediterranean coast, which have already appeared in our columns. We have also other articles of interest taken from other journals, and the usual descriptions and admirable coloured plates of new plants.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 12.—"Note on the Condensation of Gases at the Surface of Glass." (Preliminary.) By J. T. Bottomley, M.A., F.R.S.E. Communicated by Prof. Sir William Thomson, F.R.S.

It is well known to those who have endeavoured to obtain, in glass vessels, the very perfect vacuums first sought after and obtained by Crookes, and producible by the mercurial pumps, that the operation is much assisted by heating the glass vessels to be exhausted, and even the tubes of the pump, to a high temperature. The difficulty of removing the film of air and moisture adhering to glass tubes is also well known to makers of barometers and thermometers.

When the Sprengel pump is used for producing a vacuum, and when a tolerably good vacuum has been produced, so that the barometric gauge indicates a presence of one millimetre or half a millimetre of mercury, the drops of mercury falling in the tube of the Sprengel give rise to a loud metallic hammering sound; and they fall with such unbroken sharpness that those who use this form of pump are often troubled by the "fall-tubes" splitting longitudinally through a length of several inches—a phenomenon in itself very remarkable, considering the strength of the tubes and the smallness of the mercurial drops. If, while this hammering is going on, the glass vessel which is being exhausted and the leading tubes of the Sprengel pump be heated by passing the flame of a spirit-lamp or of a Bunsen burner over them, the hammering immediately ceases, and on looking closely at the fall-tubes it is seen that they are carrying down air which the heat has liberated from the glass walls of the apparatus. The ordinary barometer-gauge is scarcely sensitive enough to show an increase of pressure, but the McLeod gauge readily shows it.

There is another well-known phenomenon connected with the condensation of gases and vapours on the surface of glass: viz. the condensation of a watery film over the glass of electric apparatus, in virtue of which, at temperatures considerably above the dew point, the glass supports are not insulators of electricity. This film of moisture is removed by exposing the glass stems to heat, or to an artificially dried atmosphere. Some years ago, at the wish of Sir William Thomson, I endeavoured to weigh this film of moisture, but was absolutely unsuccessful. The film must be of extreme tenuity. Prof. Quincke has, however, made important researches on the "distance of capillary action" and on some of the properties of these very thin films. His results are given in two papers: *Poggendorff's Annalen*, 108, p. 326, 1859; and *Wiedemann's Annalen*, vol. ii. 1877, p. 145. He finds their thickness to be comparable with 5×10^{-5} cm.

With the view of measuring the quantity of gas condensed upon a given surface of glass, I caused to be prepared in August last a large quantity of fine glass thread. Some of this was of flint glass rod or cane, which was softened in the blowpipe flame, and drawn out on to a wheel. The remainder was of flint glass tubes, drawn out in a similar way. The spun glass was carefully parcelled up in paper and put aside till I should be ready to use it.

On January 3 I put a quantity of the non-tubular glass fibre into a glass tube 2 cm. in diameter and 12 cm. long, and attached it by a glass sealing to a five-fall Gimingham Sprengel pump. The pump, which was in excellent order, was then worked rapidly till I had produced a very good vacuum, which by the McLeod gauge gave me an indication of 0·3 **M** pressure.[1] The pump was then left for about an hour, and at the end of that time, passing one more bottle full of mercury through the pump, I ascertained that the vacuum had not sensibly deteriorated, the McLeod gauge giving identically the same reading as before. This exhaustion was performed without the application of any unusual heat to the tube containing the glass fibres. The temperature of the room was about 56° F.

I now raised the mercury to the upper level and allowed it to flow through the pump, and the drops fell with the well-known loud hammering noise. While this was going on I applied a Bunsen burner to the tube containing the spun glass. In a few seconds the hammering of the mercury ceased, and on applying the test of the McLeod gauge the pressure within the pump was found to have risen largely. I did not, however, obtain a measurement with the gauge corresponding to the maximum pressure of the gas driven off, or to any particular state.

I now proceeded to pump out all the gas I could, working the pump and heating the tube containing the glass fibres strongly. The heating was carried on from time to time till the tube, which was of German glass, showed signs of softening and of falling in; and the glass fibres were likewise, some of them, slightly softened and bent.

The pump was worked for over an hour, the heating being applied, and the gas, which was easily seen being carried down, was collected in a tube made for the purpose, which was fitted on over the upturned ends of the five fall-tubes. At the end of this time the vacuum was again fairly good, though not so good as it was before the heating commenced. The McLeod gauge indicated 1·2 **M**. It was seen that very little more air was being carried down, and I did not wish to push the vacuum farther than, or quite so far as, the vacuum which had been obtained before the liberation by heat of the condensed gas.

The collecting tube was now removed, and the gas obtained was measured and analysed, so far as it was possible to analyse a quantity so small.

The total amount of gas collected was calculated to be, at 15° C. and a pressure of 760 mm., 0·45 of a cubic centimetre. To this a small quantity of strong solution of caustic potash was added, and time was given for absorption. A small quantity of pyrogallic acid was next added, and the further absorption observed. The residue was so small that I could do nothing farther.

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[1] **M** standing for one-millionth of an atmo.

The result of the analysis showed 8·24 per cent. of the whole to be carbonic acid gas (absorbable by caustic potash). Of what remained 24·8 per cent. was oxygen (absorbable by pyrogallic acid and caustic potash mixed). The residue, 75·2 per cent., was, I presume, mainly, if not wholly, nitrogen. I ought to remark that my pump was furnished, as is usual, with the phosphoric acid drying tube. The gas, therefore, which I collected was perfectly dry, and I have no way at present of ascertaining how much moisture adheres to the spun glass. In stating the results of the analysis I have made no correction for moisture introduced with the potash solution.

In order to make an estimate of the amount of surface exposed by the spun glass, I measured, with a screw micrometer gauge, the diameters of 200 of the fine glass fibres taken at random. I found them, as I expected from the care with which they had been prepared, fairly uniform, and the average diameter was 7·06 hundredths of a millimetre. Weighing also the 200, and then the whole quantity, I found the whole number of the fibres to be 6370. The average length was 10·25 cm. The surface was thus 1448 sq. cm., or equal to that of a square 38 cm. in the edge.

I am preparing for further experiments on this subject, and hope soon to be able to add to it observations on the amount and on the electric conductivity of the film of moisture condensed upon the surface of glass.

Additional Note.—Since the writing of my former communication on this subject, I have made some further experiments on it, and I beg leave to give an account of the results of one of these experiments.

Having filled a fresh tube with fresh spun glass, I carefully exhausted with the Sprengel pump on January 24, and the exhaustion was kept up till February 5—that is, for twelve days. During this time I frequently tested with the McLeod gauge. A very slight increase of pressure was found during that interval, but it was so slight that I am not able to say that it was greater than that which is observed at all times, even with the Sprengel pump in excellent order, when a vacuum is maintained for several days.

On February 5 I passed three or four bottlesful of mercury through the pump, and had a vacuum of about 0·5 **M**, as shown by the McLeod gauge. I then applied heat, and had instantly an abundance of gas given off from the spun glass. This was collected as before, and analysed.

The number of glass fibres was 15,500, giving an estimated surface area of 3527 sq. centims. The amount of gas given off was 0·41 c.c., which is considerably less in proportion than in my first experiment.

Of this gas it was found that 78·6 per cent. was carbonic acid gas (absorbable by caustic potash). Of the remainder 10·5 per cent. was oxygen (absorbed by pyrogallic acid and potash), while 89·5 per cent. was left unabsorbed, and may be supposed to be mainly nitrogen.

The very large proportion of carbonic acid gas is remarkable, and it is difficult to account for, unless we may suppose that it was taken up by the glass in large quantity during the operations of drawing out the glass into fibres and inclosing it in the containing tube—operations during which there was, in these preliminary experiments, an abundant supply from the blowpipe flames.

Chemical Society, February 5.—Dr. W. H. Perkin, F.R.S., President, in the chair.—A lecture was delivered "On Chemical Changes in their relation to Micro-Organisms," by Professor Frankland, F.R.S., a plant being defined as an organism performing synthetical functions, or one in which these functions are greatly predominant; an animal, as an organism performing analytical functions, or one in which these functions greatly predominate. The micro-organisms were classed by the lecturer among animals. Their life essentially depends upon the taking asunder of more or less complex compounds, resolving them into simpler compounds at the expense of potential energy. As micro-organisms are commonly termed "ferments," and their analytical operations "fermentations," it is necessary to sharply distinguish between organised ferments and certain bodies which bring about analogous chemical changes, but which are not only not organised, but exist in solution. These latter, or "soluble ferments," as they are commonly termed, are said to act by contact: they produce certain chemical changes in the fermentescible substances without themselves furnishing from their own substance any of the products of change; the effects they produce are essentially analytical, consisting in the assimilation of water and the splitting up of the fermentescible substance into two or more new molecules, and may be brought about by purely chemical means. They differ only, or chiefly, from the organised ferments in that they are unorganised and do not increase in amount during their action upon fermentescible substances, of which a very large, although limited, quantity may undergo transformation by the action of a very minute quantity of the ferment. A list of changes brought about by unorganised ferments was given. In that portion of the animal kingdom with which we are best acquainted, oxidation is the essential condition of life: it is the kind of action by which the animal changes actual into potential energy. The changes effected by micro-organisms are essentially of the same character as those brought about by the higher orders of animals: that is to say, they are all changes by which potential becomes actual energy. With one or two exceptions, the chemical changes effected by micro-organisms—unlike those produced by soluble ferments—cannot be brought about by other means. The observations of Hatton and others have shown that micro-organisms retain their vitality in presence of a variety of substances which rapidly prove fatal to higher animals; the unexpected fatal effects of pongy iron would seem to promise, however, that there are substances fatal to bacterial life which have no toxic effect on more highly organised animals. It has not yet been shown that any degree of cold, however intense, is fatal; animation may be suspended, but it is restored when the temperature rises. With regard to heat, the lowest fatal temperature recorded is 40° C., but many species can withstand much higher temperatures. Chloroform and compressed air are said to arrest their action, but have no influence in preventing the changes brought about by unorganised ferments. The position of micro-organisms in nature is only just beginning to be appreciated; their study both from chemical and biological points of view is, however, of the highest importance to the welfare of mankind, and leads the inquirer right into those functions of life which are still shrouded in obscurity. In the course of the lecture the best known micro-organisms and the chemical reactions due to them were passed in brief review. Prof. Frankland also referred to the following results of an experiment made in the month of June, in which fresh urine was allowed to stand for 25 days in a clean glass vessel:—

	Residue left on evaporation and drying at 100° C.	Organic carbon.	Nitrogen as urea and other organic matter.	Ammonia.	Microscopical observations.
Fresh urine	4817·0	943·81	1080·27	142·40	No bacilli.
" after 1 day	—	940·46	1095·05	136·65	"
" " 3 days	—	928·76	1106·70	136·50	"
" " 5 "	—	882·66	983·26	288·55	Sparse bacilli.
" " 7 "	—	739·82	900·80	338·60	
" " 9 "	—	682·99	784·93	485·12	Numerous bacilli.
" " 11 "	—	621·02	744·64	534·80	Very numerous bacilli.
" " 14 "	—	559·22	481·49	870·62	Vast numbers of bacilli.
" " 16 "	—	530·68	492·04	881·87	Mostly still.
" " 18 "	—	487·01	355·25	990·78	" "
" " 21 "	—	466·43	278·22	1105·75	All dead or still.
" " 23 "	—	451·43	347·45	1017·25	" "
" " 25 "	2718·0	460·78	283·90	1070·50	
After allowing for evaporation	2045·5	346·77	213·66	805·63	

The results of these observations and determinations which were made during the month of June show conclusively that, previously to the development of *Bacillus ureæ*, the chemical composition of the urine remained practically unchanged; but with the appearance of micro-organisms, a diminution of organic carbon and a transference of nitrogen from the organic to the ammonia column immediately began. As regards rapidity, this change marched *pari passu* with the density of population, and reached its maximum about the 12th day; for during the three days (11th to 14th) nearly 10 per cent. of carbon disappeared, whilst more than 85 per cent. of the organic nitrogen became ammonia. After the 14th day the rate of change became much slower, on the 18th day the bacilli were mostly either dead or motionless, whilst on and after the 23rd day no more moving bacilli was seen. Altogether the quantity of carbon converted into carbonic anhydride, after allowing for concentration of the liquid by evaporation, amounted to 597·04 parts per 100,000 of liquid, or 63·3 per cent. of the total quantity; whilst the quantity of organic nitrogen converted into ammonia was 546·19 parts per 100,000, or 50·6 per cent. of the whole. These proportions show that all the organic nitrogen contained in the urea was not converted into ammonia. It no doubt escaped as free nitrogen, in accordance with Frank Hatton's observation. In the original urine the proportion of organic carbon to organic nitrogen was as 1 : 1·15, whilst, after the action of the bacilli, it was 1 : 0·62. Prof. Burdon Sanderson said that the main difficulty met with in studying the effects of micro-organisms arose from the fact that it was always difficult and often impossible to distinguish between different organisms. Chemists might naturally turn to biologists for aid in the matter, but biologists must admit the existence of this difficulty. We are fully acquainted with the life history of only one pathogenic organism—*Anthrax bacillus;* of this, thanks to Koch, we know, however, a great deal. The method followed by biologists in studying pathogenic forms was, in the first instance, to prepare a pure cultivation of the organism, and then to obtain the proof that the organism produces its proper effect when transferred to a living animal. The morphological relations of bacteria with plants could not be questioned, but he thought it was really of little consequence for practical purposes whether ferment organisms were regarded as animals or plants; what we want to know is, what are the conditions under which an organism is produced, and its life history. He was in the habit of calling them microphytes, as being a neutral term.—Prof. Ray Lankester was astonished at the definite way in which Prof. Frankland had classed the ferment organisms with animals. Naturalists were led to regard them as plants from examining their relations to other organisms. He agreed with Prof. Sanderson that "microphyte" was a good name for them, although not precisely for the same reason, but because it really meant a little plant. He stated that it was held hitherto that a micrococcus induced the ammonic change in urine, and not a bacillus as figured by the lecturer. For the purpose of chemical investigation, it was essential to have a pure cultivation. It was curious that the nitrifying organism had not been isolated; its presence had only been inferred, and it had never been satisfactorily separated and identified, although inconclusive statements and observations purporting to inform us as to the form of that organism had been published.—Dr. Brunton said that it was highly probable that the symptoms occurring in certain diseases were due to poisons formed by the action of organisms and not directly to the organisms themselves. This was not improbably the case in cholera. Micro-organisms may even produce substances fatal to themselves, *e.g.*, phenyl compounds. This is also the case with higher organisms, the retention of the urine in man being often attended with fatal results. Although cholera was very probably due to the presence of low organisms, the symptoms were so very like those produced by certain poisons, that it was very difficult to diagnose cases of poisoning by arsenic from cholera cases. The cholera poison was probably of an alkaloïdal character and related to the ptomaïnes. Pepsin converted albuminoïds into peptones, but it was important to note that Brieger had observed that sometimes an alkaloïd having an action similar to curare was formed during peptic digestion, and an alkaloïd having a similar action had been obtained from human urine. These facts rendered it probable that alkaloïds might be formed in the intestinal canal and absorbed into the general circulation. Prof. M. Foster said that the question whether the micro-organisms in question were plants or animals was to him a matter of indifference compared to the question—what was the exact nature of the action by which the organism effected the chemical change? He desired to point out that in certain cases, as in the ammonic conversion of urea, the same change, in this case the conversion of urea into ammonium carbonate, was effected, on the one hand, by a micro-organism, a micrococcus or bacillus, and on the other hand by an unorganised ferment. His friend Mr. Sheridan Lea informed him that he had evidence of both these causes of ammonic conversion of urea. Now, was the action in both cases the same? The idea had naturally occurred that the organism produced its effect by producing an organised ferment. But all attempts to prove the production of such a secretion, so to speak, of a ferment had failed. If such a ferment were produced, it was destroyed or disappeared during its action, whereas ordinary unorganised ferments such as pepsin, &c., were not destroyed at all during their activity, or were destroyed very slowly. On the whole, the probability was that the micro-organism and the unorganised ferment produced the same result in different ways; ought not the *difference* to offer the key for solving the problem? He further desired to remind the Fellows that actions similar to those of these micro-organisms were continually being carried on by the constituent elements of man and other macro-organisms, and would wish, in illustration, to call their attention to the act of secretion by a secreting cell, such as the pancreatic cell. We had evidence that certain constituents of pancreatic juice existed in the cell, not in the form in which they appear in the juice itself, but in an anterior, more complex condition. Thus trypsin occurs in the pancreatic cell not as trypsin but as trypsinogen. Now this trypsinogen, and also probably other "mothers" of the constituent of the juice, exist in the protoplasm of the cell as *discrete granules, lodged in the meshes of the protoplasm, separated from the protoplasm by films of fluid.* Yet the protoplasm, stirred by some nervous impulse, is able to produce a change in these granules, so that they are discharged to form the secretion. How does the protoplasm work upon these granules? Does it discharge something into the fluid of its meshes, which something acts upon the granules? or does it work upon the granule through the film of fluid surrounding the granule, by something which is a sort of "action at a distance"? The action, then, in this case is very comparable to the action of the micro-organisms in question. It is for the chemists to throw light on the exact nature of the changes produced, and, when this is done, we may hope to learn how the change is brought about; but not until this is done.—Mr. Thistelton Dyer said that from the botanist's point of view he was struck with the universality of fermentative changes. Though they were so predominant a feature in the life of the lower plants, this was only an extreme manifestation of what, perhaps, all plants were capable of, if the conditions demanded it. Thus Pasteur, following up an experiment of Bérard's, found that a rhubarb leaf in an atmosphere of carbon dioxide yielded, after 48 hours, though apparently unchanged, small quantities of alcohol. The breaking up of molecules of large thermic equivalent into those of less, supplies the energy needed for the continued life of the tissues, and is the *raison d'être* of the process. But plants also set up fermentative changes external to themselves, as it were incidentally and without any obvious benefit. The investigation of Beyerinck on the production of gum by plants yielded most remarkable results. It is due to a disease which is highly contagious, and which is caused by a fungus (*Coryneum*). This produces a ferment which changes the cell-walls into gum. But what is most remarkable is that even after the disappearance of the fungus which initiated the changes, the the cells of the host plant take on a morbid habit of growth, and themselves continue the production of the ferment and therefore of gum to their own hurt. The problem is here of the most complicated kind. The series is ended by cases such as that of *Withania coagulans* (and many others are now known), where plants throw off, as bye-products of their metabolism, ferments as effective as rennet, without deriving any perceptible advantage from their possession. That plants use in working up their reserve-proteïd proteolytic ferments just as animals do, cannot be doubted. But even these they occasionally, as in the Papaw, produce in utter disproportion to their own possible requirements. Mr. Warrington said with regard to the difference between animals and plants, he thought the fact had been somewhat overlooked that plants are able to obtain their nitrogen from such simple compounds as ammonia and nitrates, whereas animals appear to require to have the nitrogen presented to them in an albumenoïd form. As to the nature of the nitrifying organism, Müntz and Schlösing claim to have isolated it and have described it. A friend who had microscopically examined his purest cultivations at Rothamsted, had been unable to find bacilli, but they appeared to contain a micrococcus. [Prof. Lankester, interposing, remarked that the growth sent to him by Mr. Warrington consisted of bacilli, and nothing else.] In explanation, Mr. Warrington said that in one of his earlier papers he had mentioned that white films appeared on some of his solutions. Prof. Lankester had examined these, but he had since found that the bacilli of which they consisted were incapable of nitrifying ammonia. Latterly he had followed Dr. Klein's method, and had introduced the infecting matter into the sterilised cultivation liquid by means of a capillary pipette, which was pushed through the cotton-wool plug closing the tube or flask; since he had done this, the films referred to had never been formed. Dr. Thudichum agreed that the ammonic changed was produced in urea by a micrococcus. The study of microphytes and of the chemical changes produced by them in the human body and in the bodies of animals was of the greatest importance. He questioned whether their action was always so specific, however, as was commonly supposed. He would also call attention to the fact that one micro-organism will kill another: thus, after plastering wine, in consequence of the removal of the tartrate, the microphyte which produces ropiness is crowded out by alcoholic forms. Dr. Stevenson calleda ttention to the importance of obtaining more information as to the alkaloïdal bodies formed by the action of micro-organisms. Prof. Frankland replied that he did not mean absolutely to say that in his experiments the work was done by the *Bacillus ureæ*, but the diagram was a faithful representation of what he saw; he attributed the action to the particular organism, because it commenced when the organism appeared, and ceased when the bacilli became motionless. The necessity of studying the actions of pathogenic organisms had been prominently brought forward in the discussion. He thorght there was a substantial difference between the class of chemical changes effected by plants on the one hand and by animals on the other; animals more particularly consumed as food those compounds in which much energy was stored up.

Geological Society, February 11.—Prof. T. G. Bonney, F.R.S., President, in the chair.—Arthur William Clayden, Samuel Kideal, and H. W. Williams were elected Fellows of the Society.—The following communications were read:—The Tertiary and older peridotites of Scotland, by John W. Judd, F.R.S., Sec.G.S. The very interesting rocks known as "peridotites" have been regarded by many petrographers as peculiar to, and, indeed, characteristic of, the older geological periods; but in the Western Isles of Scotland there occur a number of rocks of this class, constituting portions of intrusive masses, which the author, in a previous paper, has shown to be the central cores of Tertiary volcanoes of vast dimensions. These Tertiary peridotites are most intimately associated with the gabbros and dolerites, the felspathic and non-felspathic rocks passing into one another by insensible gradations, and the rocks of either class being intersected by veins of the other. The peridotites exhibit the same varieties of microscopic structure as the associated gabbros and dolerites, these structures being described under the names of "granitic," "ophitic," and "porphyro-granulitic." The felspars, which are rare in the peridotites, are intermediate in composition between labradorite and anorthite; they rarely, however, exhibit evidence under the microscope of being built up of laminæ belonging to different species. The study of the lamellar twinning, which is a common, but by no means universal, character in these felspar crystals, points to the conclusion that it has been induced by pressure or strain, like the similar structure in rock-forming calcite. The pyroxenes are represented by many varieties, both of the monoclinic forms (augites) and the rhombic forms (enstatites), the former being by far the most abundant. The olivines below are, for the most part, highly ferruginous varieties. The spinellids, magnetite, chromite, and picotite occur in these rocks, as do also titano-ferrite and its alteration-products. Among the accessory constituents biotite is the most abundant. It was shown that each of the minerals of these rocks is found to undergo remarkable changes as we pass from the superficial to the central portions of these intrusive rock-masses. The most important of these changes is that for which the author proposed the name "schillerization." It consists in the development of microscopic inclosures, in the form of plates and rods, along certain planes within the crystal, giving rise to metallic reflections or a play of colour. The felspars, pyroxenes, and olivines are all found to be affected in this way when they have formed the deepest parts of these volcanic cones. In this way common augite is seen at gradually increasing depths, passing into the deep-brown variety known as pseudo-hypersthene. The last-mentioned substance presents a curious mimicry of true hypersthene and paulite, which is the schillerized form of a ferriferous enstatite. The Tertiary peridotites present many variations, not only in their structure, but also in their mineralogical constitution. Among them occur examples of the rocks which have received the names of dunite, picrite, and lherzolite, with some curious types composed of felspar and olivine. Among the older peridotites of Scotland a new and very interesting type is described from near Loch Scye in Caithness. It appears to have been originally a mica-picrite, but the whole of the original minerals have been converted into paramorphs, firstly by schillerization, and subsequently by amphibolization and serpentinization. In conclusion, it was pointed out that the discrimination between the effects of the changes described as schillerization, and those known as uralitization, amphibolisation, serpentinization, and kaolinization is of the utmost importance, not only to the petrographer, but to the mineralogist.—Boulders wedged in the Falls of the Cynfael, Ffestiniog, by T. Mellard Reade, F.G.S.

Royal Microscopical Society, February 11.—Anniversary Meeting.—The President (Rev. Dr. Dallinger, F.R.S.) in the chair.—The Report of the Council showed a remarkable development of the Society during the last six years, 301 new Fellows being elected as against 97 in the preceding six years. The income showed also an important increase.—Dr. Carpenter, in moving the adoption of the Report, referred also to the success which had attended the Society's *Journal*.—Dr. Dallinger then gave his annual address to what was probably the largest gathering of Fellows ever assembled on a similar occasion. After briefly referring to the increased interest lately manifested in the study of minute organisms and recalling the characteristics of the doctrines of abiogenesis and biogenesis, he passed rapidly in review the results of the observations of Tyndall, Huxley, and Pasteur as bearing upon these questions, and called attention to the observations of Buchner as to the transformation of *Bacillus anthracis* and *Bacillus subtilis*, and *vice versâ*, and referred with approval to Dr. Klein's criticisms thereon. Having spoken of the desirability of careful and continuous study of this class of organisms, and the importance of endeavouring to establish the relation of the pathogenic form to the whole group, he said he should be better able to deal with the subject by recording a few ascertained facts rather than by making a more extended review, and he therefore devoted the main part of his address to a description of "the life-history of a septic organism hitherto unknown to science." In his observations of this form—extending over four years—he had the advantage of the highest quality of homogeneous lenses obtainable, ranging from one-tenth to one-fiftieth of an inch, his chief reliance being placed upon a very perfect one-thirty-fifth of an inch; and from the continuous nature of the observations, as well as the circumstances under which they were carried on, dry lenses had for the most part to be employed. Having in his possession a maceration of cod-fish in a fluid obtained from boiled rabbits, he found at the bottom of it, when in an almost exhausted condition, a precipitate forming a slightly viscid mass, to which his attention was particularly directed. It was seen to contain a vast number of *Bacterium termo*, but on examination with a one-tenth inch objective showed that it also contained a comparatively small number of intensely active organisms—one being discovered in about eight or ten drops of the sediment. These measured 1-10,000th of an inch in length by 1-19,500th of an inch in breadth. The fluid had originally been kept at a temperature of 90° to 95° F., and it was noticed that, when placed upon a cold stage under the microscope, the movements of the organisms became gradually slower, until at last they entirely ceased; the necessity, therefore, arose for the use of a warm stage, and the very ingenious contrivance, by which a continuous and even temperature was maintained within the one-tenth of a degree, was exhibited. The greatest difficulty in the matter was, however, experienced in obtaining specimens for observation, in order to be able to trace them from their earliest to their latest stage.—The President then explained, by means of an admirable series of illustrations projected upon a screen by the oxyhydrogen lantern, the life-history of the organism to which he had referred, exhibiting it first as a translucent, elliptic, spindle-shaped body, with six long and delicate flagella, the various positions in which the five specimens were drawn giving a very good idea of its peculiar porpoise-like movements. The various positions which it assumed in making an attack upon a portion of decomposed matter were also shown, the movements quite fascinating the observer by their rhythmical character. The supposed action of the flagella in the production of the movements observed was explained, distinct evidence being afforded of a remarkable spiral motion, at least of those behind. The process of fission was illustrated in all its observed stages from the first appearance of a constriction to that of final and complete separation, the whole being performed within the space of eight or nine minutes. A description of the process of fusion from the simple contact of two organisms to their entire absorption into each other followed, as well as their transformation into a granular mass which gradually decreased in size in consequence of the dropping of a train of granules in its wake as it moved across the field. The development of these granules was traced from their minute semi-opaque and spherical form to that of the perfect flagellate organism first shown, the entire process being completed in about an hour. Experiments as to their thermal death-point showed that, whilst the adults could not be killed by a temperature less than 146° F., the highest point endured by the germs was 190° F. Illustrations of a variety of other modes of fission discovered in previous researches on similar forms were given, showing the mode of multiple division and a similar process in the case of an organism contained in an investing envelope. The President concluded his address, which was listened to throughout with the greatest attention, by remarking that, though the processes could be seen and their progress traced, the *modus operandi* was not traceable. Yet the observer could not fail to be impressed with the perfect concurrent adaptation of these organisms to the circumstances of their being; they were subject to no caprices, their life-cycles were as perfect as those of a crustacean or a bird, and, whilst the action of the various processes was certain, their rapidity of increase and the shortness of their life-history were such that they afforded a splendid opportunity of testing the correctness of the Darwinian law.—Dr. Carpenter complimented the President on the value and interest of his address, and moved a vote of thanks, which was seconded by Mr. Crisp, who referred to the sacrifices the President had had to make in the performance of his duties during the past year. The new Council were elected.

Anthropological Institute, February 24.—Francis Galton, F.R.S., President, in the chair.—A paper on the race-types of the Jews, by Dr. A. Neubauer, was read. The opinion that the Jewish race have kept their blood unmixed is based chiefly on the fact that a Jew is almost at once recognised amongst thousands of others. From the earliest times, however, we find evidence of intermixture. Abraham's son, Ishmael, was the offspring of an Arabian woman; Joseph married an Egyptian, and Moses a Midianite. David descends from Ruth, the Moabitess, Solomon is the son of a Hittite woman, and he himself had foreign wives. We are often reminded in the Bible of the non-Jewish women who came in contact with the Israelites, and undoubtedly the "proselytes" increased the mixture of races by marrying Jewish women. At Rome the conversions were numerous, and, of course, the converts frequently married Jews. Evidence was also adduced of intermarriages in later times between Jews and Christians of various races. The differences between the Spanish-Portuguese Jews and the German-Polish Jews were so marked that in the middle ages they were believed by the Jews themselves to have descended from different tribes—Judah and Benjamin respectively. But the Italian Jews, both in features and habits, stand between the rough German and the polished Spanish Jews, and there is no evidence of any systematic emigration of the various tribes. The pronunciation of Hebrew words also varies, and this variation is believed by Dr. Neubauer to be due to the influence of the language spoken by the surrounding peoples. The difficulties of obtaining accurate measurements of Jews are very great, and but few skulls have been examined; all evidence, however, goes to disprove the existence of any pure Jewish type, uninfluenced by contact with the nations amongst which they dwell.—Mr. Joseph Jacobs read a paper on the racial characteristics of modern Jews. After enumerating the various classes of Jews now existing, the inquiry was limited to the biostatics and anthropometry of the Ashkenasim Jews, who form more than nine-tenths of the whole number. Their superior fecundity and vitality were found to be due to social causes, and were therefore only secondarily racial; an indication of racial influences was found, however, in the fact that mixed marriages between Jews and Christians are infertile. Jews enjoy no immunity from any special diseases, but they are more often colour-blind, blind, deaf, and insane than others, owing, perhaps, to their life in cities and to their frequent intermarriages. Jews were then shown to be the shortest of all Europeans except the Magyars, and to have the narrowest chest. Their skulls are mostly brachycephalic. An examination of over 100,000 Jews showed that they have darker hair and eyes than those of any nation in Northern Europe, though nearly one-fifth of the Jews have blue eyes, and they have nearly twice as many red-haired individuals as the inhabitants of the Continent. A number of composite photographs of Jewish boys, prepared by Mr. Galton, were exhibited to show the Jewish type, and were compared with early representations of Jews in Assyrian art. The Jewish face was said to be a combination of Semitic features and Ghetto expression. Turning to the question of the purity of the race, it was pointed out that this depended on the number of proselytes made by Jews in ancient and mediæval times. The earlier proselytes, before the foundation of Christianity, were mostly fellow-Semites, and would not affect the type, while the numbers made afterwards were too small to modify the race, owing to their infertility and the tendency of the offspring to revert to the Jewish parent. A considerable number of Jews, the Cohens, or descendants of Aaron, were not allowed to marry proselytes, and must consequently be tolerably pure. The general conclusion reached was therefore in favour of the purity of the Jewish race.

Royal Meteorological Society, February 18.—Mr. R. H. Scott, F.R.S., President, in the chair.—Messrs. H. B. Baker, M.D., S. Dixon, R. Foster, and B. O. Meek, F.L.S., were elected Fellows of the Society.—The following papers were read:—How to detect the anomalies in the annual range of temperature, by Dr. Buys Ballot. The author shows that it is most likely that only a long-continued series of observations can give some evidence of an interruption of rise and fall, especially in latitudes where the temperature of the same day in different years may differ by 20° C., as in St. Petersburg.—Cloud observing, by D. W. Barker. As there is a great deal of confusion amongst cloud-observers, not only as to the particular names of clouds, but more especially with regard to their movements, the author recommends that there should be two simple divisions, viz. "stratiform" and "cumuliform." To the stratiform belong all the higher forms of cloud and a few of the lower; to the latter belong the typical cumulus cloud always seen in the lower atmosphere. From the result of numerous observations the author's conclusion is that the actual normal action of the cirro-filum cloud is along the line of filature, and that, knowing the bearing of the V or radiating point, the direction of its motion can be at once inferred. In all cases the V point first formed in the point from which the cloud is coming, but it will frequently be noticed that threads first appear parallel to a certain point on the horizon, and in all sorts of positions between this and the central V point.—A suggestion for the improvement of radiation-thermometers, by W. F. Stanley. The author suggests that the radiation-thermometer should indicate the amount of heat radiated by the sun upon a metal ball of a certain size, this being an object easy of uniform reproduction by mechanical means. For experiment he made three hollow copper balls, which were cast with ordinary filed cores, and were of different weights. These balls were turned to exact external diameter of 1·4 inch, with similar necks of the insertion of thermometers. The surfaces were oxidised by heating to resemble the oxidation produced by the atmosphere. In each of these balls a similar thermometer was inserted, closing around the neck just sufficient to keep it steady by cotton thread soaked in paraffin. The three thermometers thus inclosed in the metal balls, when exposed to sunshine and placed at two inches above a piece of black board, appeared to register, under similar conditions, exactly alike. The experiments for three summer months gave from 6° to 11° difference between the sun and shade.

Entomological Society, February 4.—R. McLachlan, F.R.S., President, in the chair.—The President returned thanks for his election, and nominated Messrs. Dunning, Stevens, and Weir as Vice-Presidents for the coming year.—Two new members were elected.—Mr. J. W. Slater exhibited a specimen of *Polyommotus chryseis* from Aberdeenshire.—Rev. A. Fuller exhibited a collection of insects captured along the line of the Canadian Pacific Railway.—Mr. W. Cole exhibited a wasp's nest which appeared to have been inhabited by *Vespa norvegica* and *sylvestris* in common.—Mr. W. L. Distant exhibited a series of wings of Indian butterflies, received from Mr. de Nicéville, showing the differences between broods of the same insect in the wet and dry seasons respectively, which had hitherto been generally regarded as distinct species.—Mr. E. A. Butler exhibited egg-cases of three species of *Mantidæ* from Bechuanaland.—Mr. W. F. Kirby (on behalf of Herr Buchecker, of Munich, who was present as a visitor) exhibited three volumes of drawings of *Hymenoptera*.—Mr. H. T. Stainton exhibited specimens of *Chauliodus insecurellus* from Gascony.—Mr. T. R. Billups exhibited various English *Ichneumonidæ* and *Hemiptera*.—Papers read: — Mr. G. F. Mathew, on the life-history of *Papilia Schmeltzi*, *P. Godeffroyi*, and *Xois Sesara*; and Mr. G. Lewis, on a new genus of *Histeridæ* from Japan.

EDINBURGH

Royal Society, February 16.—Mr. John Murray, Vice-President, in the chair.—Mr. W. E. Hoyle read the first part of a paper on the Cephalopoda of the *Challenger* Expedition. Mr. Hoyle confined attention in this paper to the octopods. Nineteen of these are new to science.—Sir W. Thomson gave a communication on energy in vortex motion. This subject he treated under five heads:—(*a*) energy in vibrations; (*b*) unlimited augmentation of energy of a simply continuous fluid mass in a space of given shape, by changes from and back to this shape; (*c*) annulment of energy under same conditions; (*d*) reduction of energy to absolute minimum in a multiply continuous space of given shape; (*e*) unlimited augmentation of energy in a multiply continuous space.—Dr. Thomas Muir submitted the first part of an exhaustive investigation into the theory of determinants in the historical order of development.—Dr. Muir also, in a paper on bipartite functions, developed a new notation for the expression of quantics, and proceeded to exemplify its use by applying it to the simplification of the proof of Galois's theorem regarding the continued fraction representation of the roots of an equation.—A letter from Prof. Michie Smith was read. He observed the zodiacal light from the top of Dodabettah. No bright lines were seen, and the light did not appear to share in the diurnal motion of the heavens, for it was seen unaltered in position for four hours after sunset. Prof. Smith also showed that in a condensing mist the air-potential is uniformly and markedly higher than the average for the time of day, while the reverse occurs in an evaporating mist.—Prof. Tait gave experimental proof of Sir W. Thomson's theory of the equilibrium of vapour with a liquid under surface-tension by means of two atmometers, in one of which artificial condensation was produced, while under the atmospheric conditions at the time evaporation would be going on in both.—Mr. John Aitken exhibited a new apparatus for the combination of colours.

PARIS

Academy of Sciences, February 21.—M. Rolland in the chair.—Annual elocution, by M. Rolland, President of the Academy for 1884. The chief topics touched upon were the life and work of the late distinguished members of the Academy —M. J. B. Dumas, MM. du Moncel, Wurtz, and Thenard; the aërostatic essays of MM. Renard and Krebs, which were regarded as marking a new era in aërial navigation; M. Janssen's action in reference to the universal meridian adopted at the recent Congress of Washington; the outbreak of cholera in the south of France and in Paris; M. Pasteur's experiments with the charbon virus and rabies; the progress of electric discoveries.—Announcement of the prizes awarded for the year 1884. Amongst the successful competitors were MM. Manen and Hanusse (mechanics); M. Baills (traité de balistique rationelle); M. Riggenbach (mountain railway); M. Jules Houël (contributions to pure mathematics); M. du Rocher du Quengo (improvement in screw steam navigation); M. Radau (astronomy); M. Ginzel (lunar physics); M. G. Cabanellas (theory of applied electricity); M. Alfred Durand-Claye (statistics); M. Chancel (organic chemistry); M. Emile Rivière (geology); M. Otto Lindberg (botany); M. P. Fischer (zoology); M. Testut (medicine and surgery); Dr. Cadet de Gassicourt (diseases of infants); M. Tourneux (embryology); MM. Cadiat and Kowalevsky (anatomy); MM. Jolyet and Laffont (experimental physiology); Capt. H. Berthaut (physical geography); M. Marsant (improved safety lamp for miners); M. de Tastes (meteorology); Dr. Neis (geographical exploration); M. J. Boussingault (applied chemistry).—Programme of the prizes prepared for the years 1885, 1886, 1887, and 1893. Amongst these is the sum of 100,000 francs left by M. Bréant in 1849, and still unawarded, "to whoever shall find an efficacious remedy for Asiatic cholera, or shall discover the causes of this terrible scourge." To secure this valuable prize it will be necessary (1) to find a means of curing Asiatic cholera in the immense majority of cases; (2) or to indicate with absolute certainty the causes of Asiatic cholera, so that by their suppression the epidemic shall cease; (3) or to discover a certain prophylactic as infallible, for instance, as is vaccination for small-pox.

STOCKHOLM

Royal Academy of Sciences, February 11.—The following memoirs were presented for insertion in the *Transactions* of the Academy.—Contributions to the knowledge of the Spongiæ of Bohuslan, by Herr C. Fristedt.—On a Silurian scorpion from Gotland, by Profs. Tamerlan Thorell and G. Lindström.—Review of the Salmonoids of the Stockholm Museum, by Prof. F. A. Smitt.—On the structure of the organs of circulation and digestion in the Annilides of the families of the Amphoretidæ, Terebellidæ, and Amphictenidæ, by Herr A. Wirén.—Prof. Edlund communicated the results of his latest researches on the nature of the electric discharges in air of unequal density.—Prof. Rubenson spoke in his paper on the passage of the light through isotropic substances.—Prof. Nordenskjöld presented for the *Proceedings*—(1) Catalogue of the meteorites in the Mineralogical Museum of the University of Upsala, by Dr. G. Holm; (2) researches on varieties of Diopside from Nordmarken, by Herr G. Flink; (3) hydrographic and chemical observations during the Swedish expedition to Greenland in 1883, by Herr Axel Hamberg.—Prof. Warming reported on comparative researches on the anatomy of the stems and the subterranean stolons, by Herr Fritz Haupt.—The Secretary of the Academy, Prof. Lindhagen, presented: on the minerals of the didymium group, by Dr. M. Weibull. On mononitro-a-naftalacid, by Dr. A. G. Ekstrand. On the chlorophyllophyceæ of Siberia, by Dr. R. Boldt.

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