THURSDAY, DECEMBER 31, 1896.

ANCIENT ASTRONOMY IN INDIA.

Hindu Astronomy. By W. Brennand. Pp. xiv + 329. (London: Chas. Straker and Sons, Ltd., 1896.)

THE ancient mathematical and astronomical works of the Hindus are worthy of more attention than they have yet received from Europeans. A lengthened residence in India led Mr. Brennand to become interested in the study of some of these, which was frequently interrupted by the pressure of official duties; but after his retirement he took up the subject again, and presented a paper on it to the Royal Society about five years ago. The interest manifested in this has encouraged him in the composition of the work before us, which it is hoped will have the effect of making the Hindu system of astronomy more generally known, and perhaps induce others to make further investigations on the subject. He begins by a discussion of the ancient zodiac, and its general correspondence amongst the Indians, Chinese, Chaldæans, Arabians, and Egyptians; treats also of the other division of the ecliptic into so-called lunar mansions; and shows the bearing of this upon the probability that the Hindus had originally migrated from Central Asia into India. This, however, is a view which probably few at the present time would dispute, as that is understood to be the original home of the Aryan race. It is when we come to the astronomical calculations with regard to the movements of the planets, the precession of the equinoxes, and the prediction of eclipses, that the problem of the source and origin of the astronomy contained in the Hindu books stands before us. Now as to the precession of the equinoxes, H. T. Colebrooke (who afterwards became the second President of the Astronomical Society, succeeding Sir William Herschel) pointed out in 1816 ("Asiatick Researches," vol. xii. p. 221), that the Hindus "had approximated to the true rate of that motion much nearer than Ptolemy, before the Arabian astronomers, and as near the truth as these have ever done since." The Hindus, indeed, appear to have reckoned it at one and a half degrees in a century, which is equivalent to a revolution in 24,000 years; whereas Albatenius, the earliest of the Arabian astronomers who improved upon Ptolemy, made it a degree in 66 years, which amounts to a revolution in 23,760 years. The true value of this is about 25,800 years; but though the Hindu is nearer it than the Arabian, the difference is hardly enough to warrant us in concluding that the two are independent.

Mr. Brennand's second chapter is on "Early Hindu Periods." A day of Brahma was called a Kalpa, and was supposed to comprise a period of no less than 4,320,000,000 years. A thousandth part of this was a Maha-Yuga, and a tenth of a Maha-Yuga was a Kali-Yuga, or 432,000 years. At the beginning of each Kali-Yuga the sun and all the planets were supposed to be in conjunction, and the beginning of the present Kali-Yuga corresponded to B.C. 3102 of our era. But, as Laplace pointed out in the "Exposition du Système du Monde," the conjunction was not near enough to permit us to suppose that the epoch in question was founded on observation; it must have been "invented for the purpose of giving a common origin to all the motions of the heavenly bodies in the zodiac." With regard to the enormous periods of time which the vanity of other nations besides the Hindus led them to claim, it does not seem to us that Mr. Brennand is particularly successful in endeavouring to explain it by taking a year as in fact a month, or season, and so reducing the period. Not to refer again to the Hindu Kalpa, many other periods, even reduced in this way, are absurdly long. Mention is here made of the list of eclipses said to have been sent to Aristotle by Callisthenes from Babylon, extending over a period of nineteen centuries before his time. The sole authority for this is Simplicius, who himself believed that the record sent never reached its destination, as no work then extant of Aristotle referred to it, and, as Delambre remarks, the whole thing is probably a fable.

Our author proceeds to give a very elaborate and interesting account of the Hindu mathematics and methods of astronomical calculation. Colebrooke, in the article which we have already quoted, rightly remarks that these are interesting, not in a scientific (no observations being given that can be verified), but in an historical point of view. History, however, requires chronology as her handmaid; and the date of the beginning of Hindu astronomy seems very difficult to determine with even approximate accuracy. Mr. Brennand's view is that it is really very ancient, but that it suffered an eclipse during the rise of Buddhism, and was afterwards revived. Now this took place about five centuries before the Christian era, but Buddhism first became recognised as a State religion under Asoka about the middle of the third century before Christ. During its rise amongst the people, it is thought that there was a great destruction of manuscripts, and, as Mr. Brennand points out, we are sometimes rather apt to forget how difficult it would be, without the aid of printing, to keep intact scientific knowledge which had been acquired. One of the great revivers of astronomy amongst the Brahmins was a mathematician named Aryabhatta, who is supposed to have lived not long before the time of the Christian era. He taught the diurnal rotation of the earth, and explained the true cause of solar and lunar eclipses; he is said also to have noticed the motion of the equinoctial points, but to have restricted it to a periodical oscillation. It is suggested that the allegory of the death of Durga (which, in the nature of its symbolism, reminds us of the weeping for Tammuz, which the Israelites adopted from the Babylonians, as Ezekiel was horrified to see it practised by them) was invented by the Brahmins to represent and keep in memory the decline of their favourite science, afterwards revived. We must now pass on to the age of Brahmagupta, which was probably about six centuries after Christ, or nearly the time of Mohammed. As compared with Aryabhatta, his teaching appears to have been in some respects retrograde, but his principal work was a revised and corrected edition of the ancient sacred work, "The Brahma Siddhanta," from some earlier copy which had been preserved.

The word "Siddhanta", it may be remarked, signifies "established conclusion," and a number of astronomical treatises exist under this title, though their exact date in their original shape, cannot be determined. Brahmagupta's edition of the above was called "The Brahma Sphuta Siddhanta," "Sphuta" meaning "amended" or "corrected" (perhaps "restored" would be better). Colebrooke translated two chapters of this from the Sanskrit. They are chiefly mathematical, giving methods for performing trigonometrical, geometrical, and algebraical questions. It should be mentioned that the four principal Siddhantas are reputed by the Hindus to have been inspired; the Brahma Siddhanta having been, they say, revealed by Brahma, the Surya Siddhanta by the sun, the Soma Siddhanta by the moon, and the Brihaspati Siddhanta by Jupiter. Mr. Brennand gives a very particular description of the Surya (or sun) Siddhanta, but we can mention only a few points to which he calls attention. The ancient cycle of sixty years, common to the Chaldæans, the Chinese, and the Hindus, consisted, in fact, of five periods of the planet Jupiter round the sun. As regards the planetary motions, they were all supposed to be of uniform velocity in themselves, though some appeared to move more slowly than others, on account of their greater distances. The Brahmins approximated very closely to the true length of a year. Mr. Brennand devotes a very considerable space to a description of their methods of astronomical calculation, which are worthy of careful study; but we must now conclude this short sketch of his interesting work by reiterating his own hope that his book may lead to further investigations on the subject.

W. T. LYNN.

AMBER IN SCIENCE AND THE ARTS.

The Tears of the Heliades; or, Amber as a Gem. By W. Arnold Buffam. Pp. 98, 8vo; with illustrations. (London: Sampson Low, Marston, and Co., Ltd., 1896.)

THAT the classical account of the origin of amber has not been sufficiently practical to satisfy modern inquirers, is proved by the interest that has always been attached to the subject, and more especially in recent years. The wide geographical range over which this fossil resin is now found, and the different conditions of the several deposits, has increased the interest and speculation with regard to the number and character of the trees or plants from which the resin exuded in long past ages; but speculation has of late been largely converted into fact by the systematic study of a mass of material that has been carefully examined by Dr. H. R. Goeppert and A. Menge, and more recently by Dr. H. Conwentz.

Goeppert and Menge's "Die Flora des Bernsteins," published at Dantzig in 1883, is, moreover, illustrated by a number of splendidly executed coloured plates, showing not only lumps of amber of different formation, but also sufficient material of wood structure to prove certain botanical affinities, and of floral and leaf-forms found in masses of the fossil resin.

In Dr. H. Conwentz's contribution to this subject, published at Dantzig in 1886, the plants referred to are arranged in their natural orders, commencing with the monocotyledons. This is also illustrated by a fine series of plates. Dr. Conwentz further published—at Dantzig in 1890—a "Monographie der Baltischen Bernsteinbäume," and at the Ipswich meeting of the British Association in 1895 gave a very valuable address "On English amber and amber generally," and as this paper was printed in *Natural Science* (vol. ix. Nos. 54 and 55, for August and September last), it may certainly be regarded as the best contribution in the English language to this interesting subject.

Though Mr. Buffam in his "Tears of the Heliades" devotes one chapter of twenty-four pages to the consideration of the plants furnishing amber, and their geographical distribution, he does not seem to have been acquainted with Dr. Conwentz's researches. The two authors, however, have been working on different lines. Dr. Conwentz in his paper has paid special attention to English amber and to the sources of amber particularly, while Mr. Buffam has treated his subject, as one of his titles indicates, from an artistic point of view, and in this we must say he has succeeded in making a most charming book. His description of the Sicilian amber shows at once that his admiration of the gem amounts to enthusiasm, and in this the reader is almost carried away with the same enthusiasm with such paragraphs as the following description of a gold and amber necklace which he saw on the neck of an Italian girl.

"Whilst she spoke," he says, "the gems in her necklace flashed in the sunlight, showing colour shades ranging from faint blue to deepest azure, and from pale rose to intense pigeon-blood, ruby red. The varied and lustrous hues here blended in lavish beauty drew from me involuntary expressions of admiration."

The beauty of these gems is further impressed on the reader's admiration by an excellent reproduction in colours and gold, forming the frontispiece to the book; gems such as these, however, it is stated are rare even in Sicily. Sicilian amber, we are told—

"is only found on or near the surface of the ground in an accidental manner, scattered over a wide extent of country, having been transported by down-pouring rains and by brooks and rivers far from its primary bed, which is believed to be in the neighbourhood of the Central Mountains, where Gemmellaro and Maravigna, in fact, affirmed its existence."

It is not necessary to dilate here on the general uses of amber, such as for mouthpieces of pipes, beads, brooches, &c., as this has been exhaustively treated of by nearly all writers on the subject; but the bulk of the amber of commerce is the yellow kind obtained in such large quantities from the Baltic. It may, however, be as well to refer to its early use in medicine, and on this head Mr. Buffam says:—

"The ancients employed amber as a medicine, and it is still prescribed by physicians in France, Germany and Italy, and several chemists in Paris keep it constantly in stock. It has been worn by ladies and children from time immemorial as an amulet, sometimes carved into amphoræ, and has been pronounced of service either taken internally or worn round the neck."

It is remarkable that the resin should still be used on the continent as a medicine, as stated by Mr. Buffam, for though it formerly had a reputation as a stimulant and antispasmodic in England, it has been discarded by us for at least forty or fifty years as possessing no medicinal properties.

As a material for varnish making, amber was a recognised commercial article in the sixteenth century. Whether it was the basis of the varnish used by the old violin makers has long been a disputed question, which can never be satisfactorily settled. It is not improbable that the peculiar electric qualities possessed by amber may have exercised some influence in producing the marvellous tones of the violins of the old masters; and the extremely dangerous and difficult task of melting amber in either fixed or volatile oils, on account of its liability to fire under heat, would preclude any attempt at its manufacture except in the laboratory and under personal superintendence, so that the secret of its preparation died with each master.

On the other hand, the danger and difficulty attending the melting of the substance has been advanced as a reason against the probability of its use. As a modern varnish material, amber is now scarcely in demand. With regard to English amber, though specimens are not unfrequently found on the Norfolk, Suffolk, and Essex coasts, as stated by Dr. Conwentz, there has been some doubt as to the genuine character of some of the pieces, which appear to have been copal or anime rather than true amber. The similarity in the formation of the two resins is borne out by an illustration of concentric structure given by Dr. Conwentz in his valuable paper before alluded to, with a specimen of Demerara copal from the locust tree (*Hymenæa Courbaril*) in the Kew Museum. JOHN R. JACKSON.

THE RED DEER.

Fur and Feather Series: Red Deer. Natural History, by Rev. H. A. Macpherson; *Stalking*, by Cameron of Lochiel; *Hunting*, by Viscount Ebrington; *Cookery*, by A. I. Shand. Pp. viii + 320. 12mo, illustrated. (London: Longmans, Green, and Co., 1896.)

ALL contributions to the natural history of the finest of our British mammals cannot fail to be interesting to all with a zoological turn of mind; while accounts of the stalking and chase of the same noble animal will command attention from a still wider circle of readers. Whether the three chapters which Mr. Macpherson contributes to the little work before us form an adequate account of the natural history of *Cervus elaphus*, may be a moot point, but to our mind they are too "parochial." There is, for instance, nothing said as to the distribution of the red deer, or its relations to other members of the same genus; and the chief attention is directed to its breeding-habits. The author of these chapters appears to derive most of his knowledge of the animal from the Lake District; and the first chapter is nothing more than a description and history of the fells of Westmoreland, with some casual observations on red deer thrown in. It is written in a pleasant and gossipy style, but as its purport has already appeared in the pages of "Lakeland," its reproduction here seems superfluous. The third chapter in the natural history section is entitled "Echoes of the Chase," and would more appropriately have come in Lord Ebrington's section.

Indeed, the work decidedly suffers from insufficient editing. For instance, most of Lord Ebrington's very interesting remarks on antlers, in the chapter entitled "Deer," should clearly find a place in the natural history portion. Again, after Mr. Macpherson had written, on page 34, that "Deer, by the way, are very fond of nibbling the remains of shed antlers," the editor ought not to have permitted the following sentence, by Lord Ebrington, to appear on page 278.

"I have never heard any explanation that accounted for this [the rarity of the discovery of the bodies of dead deer] satisfactorily, for the hinds would not eat carrion, though there seems little doubt that they will eat both bones and shed horns."

Either the matter is, or is not, a certainty, and one allusion would suffice.

So far as we are capable of judging, the chapters on stalking and hunting form admirable and interesting accounts of these sports. In addition to the remarks on antlers already mentioned, Lord Ebrington gives us many interesting observations which might well find a place in works on natural history. In reference to the "gait" and "slot," he writes that—

"A stag's dew-claws point outward, and are large in proportion to his own size, while a hind's are small, turn inward, and point straight down. A stag crosses his legs right and left in walking, while with a hind the prints of the hind foot will be in a direct line with those of the fore foot, unless she is heavy in calf. . . . The extra weight on the legs is no doubt the reason, and at calving time the stags are defenceless too, having shed their horns. The stag moves with more confidence than the hind, so his paces are regular. The hind moves femininely and distrustfully; sometimes she will put her hind feet down in front of the spot from where she has just lifted her fore ones, sometimes on the same spot, sometimes behind it."

Unless we are greatly mistaken, there are few professed naturalists who could have given such details; and yet they are surely of much more interest than the endless multiplication of species.

The illustrations are charming works of art, and the volume must claim a place in the library of every sportsman, if not of the naturalist also. R. L.

OUR BOOK SHELF.

The Tutorial Chemistry. Part I. *Non-Metals*. By G. H. Bailey, D.Sc., Ph.D. Pp. viii + 226. (London: W. B. Clive, 1896.)

Elementary Non-Metallic Chemistry. By S. R. Trotman, M.A. Pp. viii + 183. (London: Rivington, Percival, and Co., 1896.)

IF the publication of text-books is a sign of increased attention to the branches of science with which they deal, chemistry must be making great progress; for no week, and scarcely a day, passes without the receipt of a manual for chemical students. The two volumes now before us are fair representatives of a class of text-books designed to furnish boys with the facts which examiners periodically endeavour to entice from them.

Dr. Bailey's book furnishes a systematic outline of chemistry, so far as it relates to the non-metals. Acting upon the conviction that a knowledge of physical principles and measures should be gained from an elementary text-book of physics, Dr. Bailey has omitted the preliminary chapters usually devoted to these matters, notwithstanding the growth of opinion that experiments and measurements of physical properties of matter form the best basis for a chemical education. He does not, however, ignore the physics of chemistry altogether, for a chapter is devoted to the physical properties of gases.

The fundamental principles of chemistry, and the nature of chemical action, are laid down in the first twenty pages of the book, after which the non-metals and some of their common compounds are described. As a companion in the laboratory, containing details of many instructive experiments, the book should find favour.

On page 8 we read : " Quite recently it has been found that Helium, one of the bodies which had already been observed in the corona of the sun, occurs in the gases extracted from certain minerals by heating them in vacuo." Helium is a constituent of the solar prominences, but not of the corona.

Mr. Trotman's book follows very much the same lines as that of Dr. Bailey ; but it is more suitable for use in connection with elementary classes than for the laboratory. It is an attractive little volume, simply worded, clearly printed, and plainly illustrated. We regret to notice the absence of an index.

Hygiene for Beginners. By Ernest S. Reynolds, M.D. Pp. xiv + 235. (London : Macmillan and Co., Ltd., 1896.)

THERE are a number of good elementary books on hygiene, but this one will find a place among the best of them. The author's "Primer of Hygiene" is very well known, being widely used in Evening Continuation Schools, Technical Institutes, and County Council courses. A knowledge of elementary anatomy and physiology is, however, essential before the main principles of hygiene can be intelligently grasped. Recognising this, the author has introduced chapters on the structures and functions of the various parts of the human body, and has considerably enlarged his "Primer" in other directions. The first hundred pages of the present volume comprise nine chapters on elementary anatomy and physiology ; the remaining nine chapters are devoted to that extensive and varied knowledge concerned in the prevention of disease. The book is thus thoroughly in touch with the syllabus of elementary hygiene of the Department of Science and Art. We are not given to praising books moulded to particular syllabuses, but the present volume does not slavishly follow the lines laid down by the examiner in the subject with which it deals, and the independence is a sign of the author's ability to judge for himself the best arrangement and scope of the matter. It would be to the advantage of the community if every individual had to pass an examination in the subjects dealt with ; and we venture to say that every householder, and every mother having the care of children, should be acquainted with as much of the elementary principles of hygiene as is contained in this volume. As to teachers of South Kensington classes in hygiene, they only need to see the book to appreciate its admirable qualities.

The Parasitic Diseases of Poultry. By Fred. V. Theobald, M.A., F.E.S. Pp. xv + 120. (London : Gurney and Jackson, 1896.)

POULTRY are subject to many parasitic diseases, and the object of this manual is to inform poultry-keepers of the life-histories of these pests, so that means of prevention may be successfully carried out. Mr. Theobald is zoologist to the Agricultural College at Wye, while his knowledge of the characteristics and habits of the parasites he describes has been gained from observation of many diseased birds. Poultry-breeders and fanciers may, therefore, safely trust themselves to be guided by him ; and they will learn from his book how to distinguish and cope with the animal and vegetable parasites which often cause them such serious loss. Entomologists will discover in the work some new points on the life-histories of the parasitic forms dealt with, as well as a list of the parasites found upon fowls.

LETTERS TO THE EDITOR.

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

The Letters of Charles Darwin.

I AM preparing to publish a supplementary series of Charles Darwin's letters. My projected volume will include a full selection from those letters of a purely scientific interest which I was unable to print in the "Life and Letters," as well as from any fresh material that may now be entrusted to me.

I would, therefore, ask those of my father's correspondents who have not already done so to allow me to make copies of any letters of his which they possess. I venture to remind those who may be inclined to help me, that letters of apparently slight or restricted interest are often of value. FRANCIS DARWIN.

Wychfield, Cambridge, December 26.

On the Goldbach-Euler Theorem regarding Prime Numbers.

IN the published correspondence of Euler there is a note from him to Goldbach, or, the other way, from Goldbach to Euler, in which a very wonderful theorem is stated which has never been proved by Euler or any one else, which I hope I may be able to do by an entirely improved method that I have applied with perfect success to the problem of partitions and to the more general problem of demonstration, *i.e.* to determine the number of solutions in positive integers of any number of linear equations with any number of variables. In applying this method I saw that the possibility of its success depended on the theorem named being true in a stricter sense than that used by its authors, of whom Euler verified but without proving the theorem by innumerable examples. As given by him, the theorem is this : *every even number* may be broken up in one or more ways into two primes.

My stricter theorem consists in adding the words "where, if $2n$ is the given number, one of the primes will be greater than $\frac{n}{2}$, and the other less than $\frac{3n}{2}$. This theorem I have verified by innumerable examples. Such primes as these may be called mid-primes, and the other integers between 1 and $2n-1$ extreme primes in regard to the range $1, 2, 3 \ldots, 2n-1$.

I have found that with the exception of the number 10, Euler's theorem is true for the resolution of $2n$ into two *extreme* primes ; but this I do not propose to consider at present, my theorem being that, with exception of $2n = 2$, every even number $2n$, may be resolved into the sum of two mid-primes of the range $(1, 2, 3 \ldots 2n-1)$. As, *ex. gr.*

$$
\begin{array}{llll}
4 = 2 + 2 & 6 = 3 + 3 & 8 = 5 + 5 & 10 = 3 + 7 \\
12 = 5 + 7 & 14 = 7 + 7 & 16 = 5 + 11 & \\
18 = 5 + 13 = & 7 + 11 & 20 = 7 + 13 & \\
40 = 11 + 29 = & 17 + 23 & 50 = 13 + 37 = 19 + 31 & \\
100 = 29 + 71 = & 41 + 59 & & \\
200 = 61 + 149 = & 73 + 127 = \&c. & & \\
500 = 127 + 373 = & 193 + 307 = \&c. & & \\
1000 = 257 + 743 = \&c. & & &
\end{array}
$$

And so on.

My method of investigation is as follows. I prove that the number of ways of solving the equation $x + y = 2n$, where x and y are two mid-primes to the range $2n-1$, *i.e.* twice the number[1] of ways of breaking up $2n$ into two mid-primes + zero or unity, according as n is a composite or a prime number, is exactly equal to the coefficient of x^{2n} in the series

$$\left(\frac{1}{1-x^p} + \frac{1}{1-x^q} + \ldots + \frac{1}{1-x^l}\right)^2$$

where $p, q, \ldots, l$ are the mid-primes in question. This coefficient, we know *à priori*, is always a positive integer, and therefore if we can show that the coefficient in question is not zero, my theorem is proved, and as a consequence the narrower one of Goldbach and Euler. By means of my general method

[1] This number may be shown to be of the order $\frac{n}{(\log n)^2}$ and a very fair approximate value of it is $\frac{\mu^2}{n}$ where μ is the number of mid-primes corresponding to the frangible number $2n$

of expressing my rational algebraical fraction, say ϕx, as a residue, by taking the distinct roots of the denominator, say ρ, and writing the variable equal to $\rho\epsilon^{\theta}$, and taking the residue with changed sign of $\Sigma\rho^{-n}\,\epsilon^{-nt}\phi\rho^{\epsilon t}$, we can find the coefficient of x^n or (if we please to say so) of x^{2n} in the above square, and obtain a superior and inferior limit to the same in terms of $p, q, \ldots, l$; and if, as I *expect* (or rather, I should say, *hope*) may be the case, these two limits do not include zero between them, the theorems (mine, and therefore *ex abundantia* Euler's) will be apodictally established.

The two limits in question will be algebraic functions of $p, q, \ldots, l$, whereas the *absolute* value of the coefficient included within these limits would require a knowledge of the residues of each of these numbers in respect to every other as a modulus, and of $2n$ in respect of each of them. In a word, the limits will be algebraical, but the quantity limited is an algebraical function of the mid-primes $p, q, r, \ldots l$.

J. J. SYLVESTER.

Athenæum Club, December 20.

P.S.—The shortest way of stating my refinement on the Goldbach-Euler theorem is as follows:—"It is always possible to find two primes *differing by less than any given number* whose sum is equal to twice that number."

Another more instructive and slightly more stringent statement of the new theorem is as follows. Any number n being given, it is possible to find two primes whose sum is $2n$, and whose difference is less than $n, n-1, n-2, n-3$, according as n divided by 4 leaves the remainders $1, 0, -1, -2$ respectively.

Major MacMahon, to whom and to the Council of the Mathematical Society of London I owe my renewed interest in this subject, informs me that in a very old paper in the *Philosophical Magazine* I stated that I was in possession of "a subtle method, which I had communicated to Prof. Cayley," of finding the number of solutions in positive integers of any number of linear equations in any number of variables. This method (never printed) must have been in essence identical with that which within the last month I have discovered and shall, I hope, shortly publish.—J. J. SYLVESTER.

Telegraphy without Wires, and the Guarding of Coast Lines by Electric Cable.

IT appears from an article in *Commerce*, December 16, that Mr. W. H. Preece, in a lecture on "Telegraphy without Wires," at Toynbee Hall, said, that from experiments at the Goodwin Lightship it had been found impossible to get a message on board, and "that the intervening sea-water performed much the same function as an iron plate," I would like to call the attention of the readers of NATURE to my paper laid before the Royal Society of Edinburgh in January 1893, when it was shown that neither salt nor fresh water had any appreciable effect on the transmission of these electrical waves. Take this case—an iron steamer afloat above a cable lying on the sea-bottom. If the steamer have on board suitable apparatus, messages sent along the cable from a single Leclanche cell can be and have been read on board ship by ordinary sailors. If it is possible to so convey messages to a vessel not moored by an anchor, it is surely possible to do the same to a moored ship such as a lightship. Mr. Preece's failure at the "Goodwin" is not due to the action of salt water, for, if electric vibrations work through salt water in the Firth of Forth, they will equally do so at the "Goodwin."

One word as to Prof. Boase and Mr. Marconi's systems. Although it may be impossible to say what system may be found best for the detection of the electric vibrations, there is one thing certain that it is needless refinement to try to send the vibrations for lighthouse work ten miles. The vibrations require to be sent only 600 feet, as it is possible to lay a cable guarding a stretch of fifty miles of coast, ten miles off the shore, in at most fifty fathoms of water, and send the vibrations along it, and whenever the ship comes within two hundred yards of the cable the detector on board would give the alarm. Further, the advantage of the cable system is great, as the vessel would know her exact distance off; whereas, by sending the vibrations from a point on shore, this would be impossible.

CHARLES A. STEVENSON.

84 George Street, Edinburgh, December 21.

The Origin of the Stratus-Cloud, and Some Suggested Changes in the International Methods of Cloud-Measurement.

IN his "Instructions for Observing Clouds" (London, 1888, p. 12), Hon. Ralph Abercromby defines *stratus* as "a thin uniform layer of cloud at a very low level," and as an illustration reproduces a photograph of a low sheet of cloud which he says is exceedingly characteristic of east winds in London. In his book "Weather," p. 48, he shows by a diagram that the position of the *stratus* is in the south-west quadrant of the anticyclone. By carefully plotting the observations made at the Blue Hill Meteorological Observatory during the past ten years, I find that this type of cloud has the same position in the anticyclones on the eastern coast of the United States that Abercromby found for England. Moreover the continuous records, made by instruments lifted by kites at the Blue Hill Observatory, furnish a very evident explanation of its origin. In a number of cases the recording instruments were lifted into or through such clouds, and in every case the temperature and humidity rose suddenly as the thermograph entered and passed through the stratus-cloud. This rise of temperature is not shown when the thermograph is lifted into cumulus or nimbus clouds. Hence it is evident that the stratus described by Abercromby is found at the plane of meeting between a cold current and a warmer, damp current overflowing it. The cause of the stratus is undoubtedly the mixture between the two currents and the consequent condensation of moisture in the warmer current.

There is, however, another conception of stratus described by Prof. H. H. Hildebrandsson in his "Classification des Nuages employée à l'Observatoire météorologique d'Upsala," where he says: "One sees that the stratus of Howard is nothing but a fog; at Upsala we designate also, under the name of stratus, fog lifted above the earth, and which exists ordinarily as isolated fragments at a slight distance above the ground." In the Hildebrandsson-Köppen-Neumayer cloud-atlas a picture of one of these isolated fragments is given above the name of *stratus*; and the primary definition of stratus given in large letters is "Lifted Fog."

These two definitions of stratus by Abercromby and Hildebrandsson have apparently been taken as identical by their authors; but I think the facts mentioned indicate that they have no more in common, either in origin or appearance, than have cirrus or cumulus. When the International Committee met at Upsala it recognised the inadequacy of the illustration of stratus given in the Hildebrandsson-Köppen-Neumayer atlas, and, like Abercromby, *pictured* stratus as a thin sheet of low cloud, but *defined* it as "Lifted fog in a horizontal stratum." This compromise between two entirely different conceptions of stratus results in an absurdity. Lifted fog rarely or never forms in a horizontal stratum. Certainly, during ten years of daily observations of clouds, I have not seen such a phenomenon, nor have I seen it described by writers on the subject. Moreover, if lifted fog ever does form in a horizontal stratum, how can an observer know, when he sees a stratus, whether it is lifted fog or is a cloud formed by mixture? I trust at some future meeting of the International Committee this definition may be changed. Probably the authors of the definition will not object to the change, now that the observations with kites have thrown a new light on the origin of stratus.

Another point to which I think the attention of those engaged in the international scheme of measuring the heights and velocities of clouds should be called, is the fact that measurements of cloud-heights by theodolites or photogrammeters give erroneous averages for certain forms of clouds. At Blue Hill Observatory, using every opportunity to measure the altitude of nimbus with theodolites, we find the average height by such measurements to be 2077 metres; yet in our measurements of cloud-heights, made by sending kites into them, we find that on more than half the days when nimbus is present its base is at an altidude of less than 1000 metres, and usually less than 500 metres. The average height determined from the kite-measurements is 497 metres, and by the angle above the horizon of the light reflected at night from the clouds over distant cities it is found to be 845 metres. Similar differences are found in the case of strato-cumulus. The reasons are that low clouds are so indefinite in outline, or they cover the sky with such a uniform veil, that they cannot be measured with theodolites or photogrammeters. It results that the clouds measured by theodolities are principally high clouds. On the other hand very high clouds cannot be measured with kites, and the average height by this method is too low. The average determined from reflected lights (845 metres) is probably most nearly correct.

I think it is apparent that the observations with theodolites and photogrammeters at the international cloud-stations should be supplemented by other methods, if correct averages are to be obtained, and if clouds which cover the sky with a uniform veil are to be measured at all. Small balloons turned loose and followed with theodolites, suggested by Kremser, is a good method in such cases. H. HELM CLAYTON.

Blue Hill Meteorological Observatory, December 7.

Radiography.

YOUR correspondent, Mr. G. M. Lowe, asks for information as to the best methods of working direct on to sensitised paper to save the time and expenses involved in taking glass negatives. "Nikko" paper, as supplied by the Eastman Company, is a good substitute for glass plates, and the results on it are much superior to the smooth bromide papers. Eikonogen is a suitable developer—say five or six ounces of water to the contents of an eikonogen cartridge, and fix in clean hypo solution. Of course, to show the palma-surface of the hand when using a glass plate, the film side is *up* and the palm *down*; but in using paper direct, the film should be down, and the X-rays, therefore, pass through the paper before reaching the sensitive surface.

Radiographs made direct on paper are *negatives*, the bones being shown white. It has been stated that this is the correct way to show the bones, but it is quite a mistake. Bones are white by reflected light; by transmitted light they are black, more or less, and if X-rays are light rays, then the light is transmitted, and radiographs ought to be as usually shown printed from a glass negative; but for surgical purposes, for such as foreign bodies in the hand, negative or positive makes little or no difference. By the direct "Nikko" paper method the exposure must be longer, but to locate a needle in the hand thirty to forty seconds is sufficient. Two, three, or up to a dozen sheets of "Nikko" paper may be exposed at one time. Between the first and second sheet very little difference in exposure will be noticed, but between the first and, say, the twelfth the difference will be considerable. To extend this difference when only a few sheets are used, insert a piece of suitable black paper between each. W. I. CHADWICK.

The Heating of Anodes in X-ray Tubes.

I SHALL be much obliged if any of your readers who work with the X-rays will give me their experience with the 10-inch coils. I have one by Apps, which is excellent in every way; but whether I take from it a 2-inch or a 10-inch spark, the anode of the tube invariably becomes red or white hot within a few seconds.

The tubes are by leading makers, and exhausted for 8 or 10-inch sparks; but, as I have said, even a 2-inch spark makes the anodes red hot.

On the other hand a German coil I have, does not perceptibly heat the anode of the tube even when I use a 5-inch spark.

Is this the experience of others; and why should a 2½-inch spark from one coil make the anode red hot immediately, when a 5-inch spark from another coil does not do so?

This difficulty at present prevents me employing the Apps 10-inch coil at all for X-ray work. WALTER CHAMBERLAIN.

Harborne Hall, near Birmingham, December 19.

Units of Force.

IN your issue of December 10, Prof. O. J. Lodge makes several curious statements.

He speaks of "inertia multiplied by the square of a velocity." He might as well speak of "shapelessness multiplied by the cube of a length." Inertia is a word best left unused, but usually means a property of what is called matter—like whiteness, hardness, inextensibility.

He also speaks of natural formulæ "independent of every system of units that can be devised," and, though he only gives one formula, implies that every mathematical relation can be expressed in a similar manner. Will he be so good as to give a formula connecting the weight, volume, and specific gravity of a body which is "independent of every system of units"?

As to the poundal, the objection to it is that no one uses it in actual work. There may be other objections, but that is a sufficient one.

As to teaching elementary mechanics, I am convinced that we should avoid "mass" as much as possible. When dealing with a particle, express Newton's Second Law by the formula $P/Q = f/\alpha$, where P and Q are the forces producing accelerations f, α, respectively. This will usually take the form $P/W = f/g$. Then you may use any unit of force you choose, and the energy formula becomes $P \times s = W\frac{v^2}{2g}$, which may be in inch tons, foot pounds, or what you please. Is it too much to hope that the poundal may be shortly relegated, even in text-books, to that place, wherever it is, where grades are employed for measuring angles? C. S. JACKSON.

R.M. Academy, Woolwich.

The Distance of the Visible Horizon.

HAS not Prof. Lodge in his enthusiasm, which I fully share, for an absolute system of measurement rather overstepped the mark when in the equation $2\,R\,h = d^2$ for the distance of the visible horizon, he says that "h is not the number of feet, or of metres, or anything else, it is the actual height; d is not the number of miles or of inches to the horizon, but it is the distance itself; and similarly 2 R is the diameter of the earth, and not any numerical specification of that diameter (see NATURE, vol. lv. page 125). Surely the equation as written is an algebraical equation, and, as such, the symbols it contains express numbers and not things. The multiplication as he implies of one length (2 R) by another length (h), is abhorrent to the mind of "the Cambridge mathematician." The superiority of the formula over the mutilated apology for it which Prof. Lodge quotes, lies in the fact that the equation is true in terms of any conceivable unit of length in which the three lengths involved in it are measured. I am of course aware that the particular formula given may be regarded as an abbreviated statement of the approximate geometrical proposition that the rectangle contained by the diameter of the earth and the height of the observer above its surface equals the square on a line equal to the distance of the visible horizon, in which case, of course, Prof. Lodge's description of the symbols would be accurately true; but I do not think that the formula with this interpretation really illustrates his meaning.

I wish to associate myself with Prof. Lodge in his condemnation, for educational purposes, of all formulæ of the engineer's pocket-book type, should it unfortunately happen, that they gain a footing on the scientific side of school instruction it will do much to justify the slur, still too often cast, on science teaching at schools and at the universities, that it is not education. This must be my apology to Prof. Lodge for thus emphasising a mere *lapsus calami*. L. CUMMING.

Rugby, December 12.

Position of Boughs in Summer and Winter.

THE following measurements may perhaps be of interest. They have been made with a view to ascertaining how much the weight of leaves and fruit depressed the branches of a tree. The first measurements were taken on August 3, the second on December 14, 1896:—

Height from Ground in inches.

	August 3.	December 14.
Mulberry tree—		
Lowest twig	0 in. ...	31 in.
Higher branch	59 in. ...	72 in.
Another branch	20 in. ...	39 in.
Walnut tree—		
Lowest twig	15 in. ...	34 in.
Higher branch	60 in. ...	76 in.

In the case of the first branch of the mulberry tree, it was found in December that a weight of 35 pounds was not sufficient to lower it to its summer position. AGNES FRY.

Failand, near Bristol, December 15.

The Cultivation of Woad.

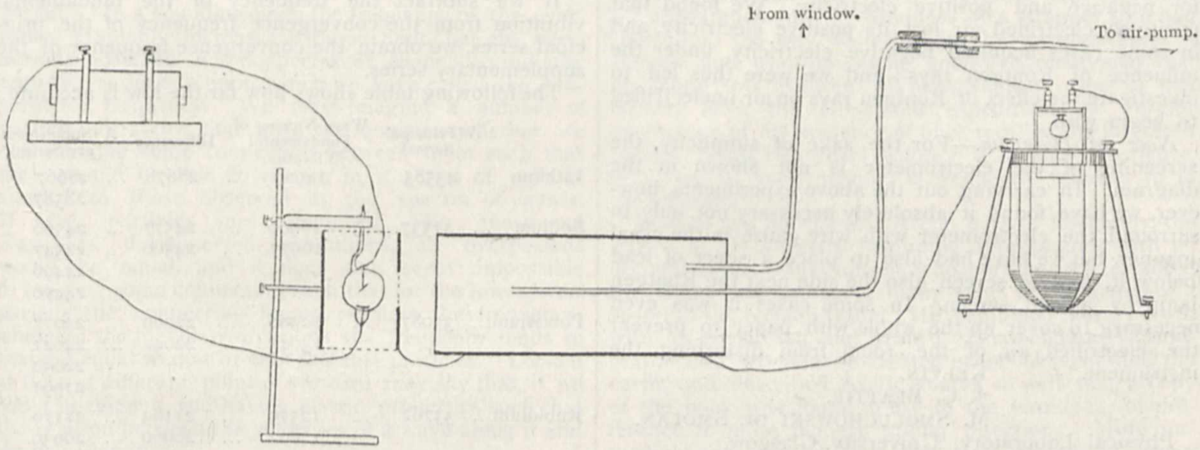
WITH reference to the letter of Rosa M. Barrett, in NATURE of November 26, p. 79, I formerly lived for many years, and my father before me, in the part of Somerset to which your correspondent alludes, viz. the neighbourhood of Bath, and within a few miles of Mells, I never remember to have seen or heard of the cultivation of woad, *Isatis tinctoria*; but "wood-wax" (? woad-wax), *Genista tinctoria*, which grows plentifully in that neighbourhood in pastures on marly soil, used to be collected by the peasant-women for dyeing purposes at the cloth factories in Trowbridge. The plant being very tough to pull up, "wood-waxing" was very laborious work. I am not aware whether it is still carried on there.

Croydon, December 16. H. FRANKLIN PARSONS.

ELECTRIFICATION OF AIR BY RÖNTGEN RAYS.[1]

TO test whether or not the Röntgen rays have any electrifying effect on air, the following arrangement was made.

A lead cylinder 76 cms. long, 23 cms. diameter, was constructed; and both ends were closed with paraffined cardboard, transparent to the Röntgen rays. Outside the end distant from the electrometer (see diagram) a Röntgen lamp[2] was placed. In the other end two holes were made, one in the middle, through which passed a glass tube (referred to below as suction pipe) of sufficient length to allow the end in the lead cylinder to be put into any desired place in the cylinder. By means of this, air was drawn through an electric filter[3] by an air pump. The other hole, at a little distance from the centre, contained a second glass tube by which air was drawn through indiarubber tubing from the open-air quadrangle outside the laboratory.



In one series of experiments the end of the suction pipe was kept in the axial line of the lead cylinder at various points 10 cms. apart, beginning with a point close to the end distant from the Röntgen lamp.

In every case the air drawn through the filter was found to be negatively electrified when no screen or an aluminium screen was interposed between the Röntgen lamp and the near end of the lead cylinder. The air was found not electrified at all, or very slightly negative, when a lead screen was interposed.

When the Röntgen lamp was removed or stopped, and air was still pumped through the filter, no deflection was observed on the electrometer. This proved that the air of the quadrangle was not electrified sufficiently to show any deflection when thus tested by filter and electrometer.

Similar results were obtained with the end of the suction pipe placed so as to touch the floor of the lead cylinder, or the roof, or the sides. Whether the air was pumped away from a place in the cylinder permeated, or from a place not permeated, by the Röntgen rays, it was in all cases found to be negatively electrified.

The following are some of the results obtained on December 16 and 17. The electrometer was so arranged as to give 140 scale divisions per volt.

Conditions.—Large lead cylinder metallically connected with sheath of electrometer. Röntgen lamp surrounded by a lead sheath, which latter was also connected to electrometer-sheath. There was a window in this lamp-sheath 2·5 cms. broad and 5 cms. high. This window could be screened by aluminium or by lead. These screens were always connected metallically to sheaths. During all the experiments a Bunsen lamp (not shown in the diagram) was kept constantly burning, with its flame about 30 c.m. below the Röntgen lamp.

Results.—Röntgen lamp in action; air drawn from lowest point of end of lead cylinder next to the R. lamp.

December 16:—

3·55 p.m.	– 61	scale divisions	in 2	mins.	with aluminium screen.
	– 63	,,	,, 2	,,	no screen.
	– 14	,,	,, 2	,,	lead screen.

4.20 p.m. Air drawn from point on lowest line of lead cylinder 26 cms. distant from R. L. end.

– 14	scale divisions	in 2	mins.	with lead screen.
– 78	,,	,, 2	,,	no screen.
– 24	,,	,, 2	,,	lead screen.
– 83	,,	,, 2	,,	alumin. screen.
– 13	,,	,, 2	,,	lead screen.

December 17. R.L. acting, and air drawn through filter.

10.47 a.m.

					End of suction pipe kept in axial line of cylinder cms.
– 44	in 2 mins.	with	alumin.	screen ...	68 from R. L. end.
0	,,	,,	lead	,, ...	68 ,, ,,
– 28	,,	,,	no	,, ...	58 ,, ,,
– 24	,,	,,	no	,, ...	48 ,, ,,
0	,,	,,	lead	,, ...	48 ,, ,,
– 23	,,	,,	alumin.	,, ...	48 ,, ,,
– 26	,,	,,	alumin.	,, ...	38 ,, ,,
– 9	,,	,,	lead	,, ...	38 ,, ,,
– 7	,,	,,	lead	,, ...	28 ,, ,,
– 26	,,	,,	alumin.	,, ...	28 ,, ,,
– 36	,,	,,	alumin.	,, ...	18 ,, ,,
– 21	,,	,,	alumin.	,, ...	8 ,, ,,

We had previously made experiments with a sheet-iron funnel 1 metre long, 14·5 cms. diameter; and with a glass tube 150 cms. long, 3·5 cms. diameter; and with an aluminium tube 60 cms. long, 4·5 cms. diameter. Air was pumped from different parts while the Röntgen rays were shining along the tube from one end, which was closed by paraffined paper stretched across it. In every case the air was found to be negatively electrified.

1 "Electrification of Air by Röntgen Rays." By Lord Kelvin, Dr. J. C. Beattie, and Dr. M. Smoluchowski de Smolan. (Read before the Royal Society of Edinburgh, Monday, December 21, 1896.)

2 The Röntgen lamp was a vacuum vessel with an oblique platinum plate (Jackson pattern).

3 Kelvin, Maclean, Galt, *Proc. R.S.*, London, March 14, 1895.

In those earlier experiments the air drawn away was replaced by air coming in from the laboratory at the open end of the tube. We found evidence of disturbance due to electrification of air of the laboratory by brush discharges from electrodes between the induction coil and Röntgen lamp, and perhaps from circuit-break spark of induction coil. These sources of disturbance are eliminated by our later arrangement of lead cylinder covered with card-board at both ends, as described above, and air drawn into it from open-air outside the laboratory.

We have also found a very decided electrification of air—sometimes negative, sometimes positive—when the Röntgen rays are directed across a glass tube or an aluminium tube, through which air was drawn from the quadrangle outside the laboratory, to the filter.

A primary object of our experiments was to test whether air electrified positively or negatively lost its charge by the passage of Röntgen rays through it. We soon obtained an affirmative answer to this question, both for negative and positive electricity. We found that positively electrified air lost its positive electricity, and in some cases acquired negative electricity, under the influence of Röntgen rays; and we were thus led to investigate the effect of Röntgen rays on air unelectrified to begin with.

Note on Diagram.—For the sake of simplicity, the screening of the electrometer is not shown in the diagram. In carrying out the above experiments, however, we have found it absolutely necessary not only to surround the electrometer with wire gauze in the usual manner, but we have had also to place a sheet of lead below it, and to screen also the side next the Röntgen lamp by a lead screen. In some cases it was even necessary to cover up the whole with paper to prevent the electrified air of the room from disturbing the instrument.

KELVIN.
J. C. BEATTIE.
M. SMOLUCHOWSKI DE SMOLAN.
Physical Laboratory, University, Glasgow,
December 19.

ON A NEW LAW CONNECTING THE PERIODS OF MOLECULAR VIBRATIONS.

AFTER the attempts to detect harmonic ratios in the wave-lengths of the light emitted by incandescent gases had failed, Balmer led the way in a line of research which promises to furnish a rational explanation of the different periods of a vibrating molecule. If τ_0 is a constant, the frequencies of all the hydrogen lines can be represented by Balmer's formula:

$$\tau = \tau_0\left(1 - \frac{4}{m^2}\right)$$

where for m we must put successively 3, 4, &c., and lines have been observed as far as $m = 15$. We may call τ_0 the convergence frequency, as it is that to which the vibrations approach as m increases. We also call the slowest vibration of the series the "fundamental." For other spectra the relationship is not quite so simple, but confining ourselves to the alkaline metals, the spectra of which have been most carefully examined and discussed by Kayser and Runge, these authors show that the vibration frequencies may be represented by means of three series of the form

$$\tau = A - \frac{B}{m^2} - \frac{C}{m^4} \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot$$

the fundamental in every case being given by $m = 3$. The first, or principal series, has lines which are single in the spectrum of lithium, but double in the other cases, the components separating further and further as the atomic weight increases. The two supplementary series consist of lines which easily widen and show the remarkable property that the convergence frequency A has nearly the same value for both supplementary series.

I must refer to Kayser and Runge's paper for a discussion as to how far the formula is approximate only, as well as to other details; but as the majority of physicists have not hitherto paid much attention to this subject, I may add the remark, that the division of spectra into a number of series of the above nature is by no means arbitrary, but constitutes a most important step in the simplification of a very complex problem. It is known that Runge and Paschen have shown that the constituents of cleveite gas have spectra resembling those of the alkali metals; and I hope in a future communication to show that the spectrum which I have called the compound line spectrum of oxygen also divides into three series of the same type.

As regards my new law, it is so simple that it is astonishing how it could so long have remained unnoticed. It may be enunciated as follows:

If we subtract the frequency of the fundamental vibration from the convergence frequency of the principal series, we obtain the convergence frequency of the supplementary series.

The following table shows how far the law is accurate:

	A (Principal Series)	Wave Number of Fundamental Vibration	Difference	A (Supplementary Series)
Lithium	43585	14907	28678	28667
				28587
Sodium	41537	16960	24577	24566
		16977	24560	24547
				24496
				24476
Potassium	35087	12988	22099	22077
		13045	22042	22022
				22050
				21991
Rubidium	33762	12579	21183	21179
		12802	20960	20939

The numbers given in the table are proportional to the frequencies, being inverse wave-lengths in centimetres. The two numbers given in the second column refer to the two components of the double lines. As the lines of the supplementary series are double in the spectra of sodium and potassium, there are the four convergence frequencies in these cases which are all given. In comparing the two last columns it must be remembered that the quantity denoted by A, which is the convergence frequency, cannot be determined with the highest accuracy because Kayser and Runge's formula is approximate only and fails to give accurate results for the case $m=3$. The only serious differences between the third and fourth columns occur in the two cases in which Runge and Paschen used the case $m=3$ in determining the constant.

In the case of cæsium, the fundamental vibration lies in the infra-red, and has not been observed, but we may use the law to forecast its position, and obtain a wave-length of 8908 for the less refrangible, and of 8518 for the most refrangible component, numbers not differing much from Kayser and Runge's estimate for the same lines. The numbers given by Runge and Paschen for the gases from cleveite are sufficient to show that the law also holds in the case of both sets of the three series into which the spectrum divides itself. In the case of oxygen, I have not obtained sufficient data as yet to determine the position of the principal series, as it lies chiefly in the ultra-violet, and the lines measured so far belong nearly all to the supplementary sets.

The supplementary series have not been observed in the spectrum of hydrogen, but the new law shows that if they exist they must lie in the infra-red, and it is with some confidence that I predict the existence of hydrogen lines in the infra-red, the convergence frequency being 1218·51 ($\lambda = 8206·6$).

Attempts to obtain other similar relations in the spectra of gases have not so far led to any results. If the differences between the frequencies of the fundamental and the higher members of the principal series are taken, we obtain numbers not far distant from the frequencies of the first subordinate series, but this is only a consequence of the facts that the value of B in Kayser's formula does not vary much, and no importance can be attached to these coincidences, which are only very approximate. A few reflections concerning Kayser and Runge's equation may not be out of place here. The different periods of the same series have every appearance of having the same relation to the fundamental vibration as the overtones have to the fundamental of a sounding body. A word is much wanted to express for molecular vibration what corresponds to an overtone in sound. The term "harmonics" is sometimes used, but is not appropriate, as an overtone may or may not be harmonic to the fundamental. I suggest the expression "overperiod." In a sounding body the frequency of the overtones gradually increases without limit, but the overperiods observed in the spectra of elements gradually approach a definite limit, which we have called the convergence frequency. We may imagine a number of particles in a row, and raise the question whether we can imagine some connection between them such that they should be able to vibrate in a series of periods similar to those observed in the spectra of gases. If the particles are detached like those of Reynolds' disconnected pendulums, all overperiods would be equal, and it does not seem impossible to imagine some connection such that for the lower overperiods the connecting forces regulate the frequency, while for the higher overperiods the frequency tends to become equal to that of the separate particles. Looked at from a different point of view we may say that, if we could imagine a rod having elastic properties such that the relation between the velocities of a wave along it and the wave-length is

$$V = a\lambda - b\lambda^3$$

it would, if vibrating freely, give out a number of notes, the relative frequency of which would be the same as that of the luminous vibrations given out by a hydrogen molecule. ARTHUR SCHUSTER.

THE EARLY LIFE OF NANSEN.[1]

THIS volume was compiled at a time when the early confidence in the success of the great Arctic effort had given place in Norway to a feeling of anxiety, if not of alarm. The translation is now published when the preliminary narrative of Nansen's triumphal procession across the polar area has cast his former exploits into the shade, and the expectancy with which the complete account of the expedition is awaited by the public will not be appeased by the book before us. It comes, in fact, a trifle inopportunely. To modify a wearied metaphor, the play of *Hamlet* is cut short before the central scene; the Scandinavian Prince has just begun to absorb attention when the curtain falls. The book is also heterogeneous in a high degree; no less than six authors are concerned in it, and the fact that all the varied contributions are translated by the same hand, robs them of some of their original freshness, although the translation is really done very well. Perhaps the unifying principle of the ill-arranged chapters may be found in "Peer Gynt," copious quotations from which are scattered over the pages. An abstract of that famous work would have proved no bad substitute for the tedious chapter on the Great Ice-Age, which, even when abridged by the translator, has little to do with the other subjects considered. The chapters on the outfit of the *Fram*, her voyage to the Kara Sea, and Baron Toll's adventurous sledging expedition to the New Siberian Islands, should have been left for the forthcoming work on the polar expedition, a fact which will make them none the less interesting to the general reader. The chapter devoted to an interview with Mrs. Nansen is a clever piece of journalism, but of doubtful taste.

So far as the work is biographical it is welcome and fairly satisfactory. It is natural that the world should wish to know something of the personal life of the men who perform great achievements, and it is proper that this wish should be gratified.

No one has ever met Nansen without being struck by his remarkable personal charm. This happened to be the first fact I knew about him. A friend, who had been spending a holiday in Norway nine years ago, told me, on his return, that what most impressed him there was the appearance and the kindness of a stranger who had shown him the way while lost in the tortuous lanes of Bergen, and who, on saying good-bye, mentioned that his name was Nansen. When Nansen came to this country after the Greenland expedition, the curious magnetism of his presence at once recalled the forgotten remark heard two years before. A similar experience, occurring to many people, is frequently referred to in the biography.

Nansen was not the first Arctic hero of his family, the record of his ancestry beginning appropriately with an account of old Hans Nansen, who, born in 1598, explored the White Sea, and spent many years in command of a vessel in the Iceland trade. He combined literary work with his navigation, and wrote a "Compendium Cosmographicum," wherein he treated of the heavens and the earth, and described Arctic routes so well that a copy of the book was found in use in the year 1841, in preference to more modern sailing directions. More immediate ancestors on both sides were persons of strong character, although their interests and activity lay in other departments; and the authors trace to them, with some skill in the application of the laws of heredity, the blending of poetic and æsthetic feelings with the reckless daring and unalterable determination of Fridtiof Nansen's character.

Born in 1861 at Great Fröen in West Aker, near Christiania, Nansen was not long in showing his love for adventure, carelessness of danger, and disregard of pain. His father, a member of the legal profession, was a stern disciplinarian, but the discipline was judicious and directed to the development of character; his mother was remarkable for her determination and practical resourcefulness; she was also an enthusiastic snow-shoe runner, before that pastime became the common sport for ladies it now is.

Nansen's school-life is briefly traced, and his enthusiasm for athletic exercises and sport of every kind treated more fully and sympathetically, with extracts from his own early letters. Entering the Christiania University in 1880, he decided, after some hesitation, to take up zoology as his special study. Two years later he made his first acquaintance with the Arctic regions during a cruise on the *Viking*, which, while unfortunate from the owner's point of view, was full of opportunities for zoological observations and, above all, for polar-bear hunting on the east of Greenland. The story of this voyage is graphically told from the unpublished diary kept by Nansen. Immediately on his return to Norway, the curatorship of the Bergen Museum was offered to him, and eagerly accepted, as it afforded exceptional opportunities for zoological research. Here Nansen was under the direction of Dr. Danielssen, the founder of the museum, a tireless worker, and a true friend. In his farewell letter in 1893, Nansen wrote:—

[1] "Fridtiof Nansen, 1861–1893." By W. C. Brögger and Nordahl Rohlfsen. Translated by William Archer. Pp. x + 402. (London: Longmans, Green, and Co., 1896.)

"If I should grow weary or slack, the thought of your strength of will and your untiring activity will spur me on as it spurs on many and many another. A thousand good-byes until we meet again."

They never met again; the old enthusiast died in 1894.

While engaged on the study of the sex of *Myxine*, and the nervous system of invertebrates at Bergen, Nansen did not neglect his physical training. A feat scarcely surpassed for actual danger and reckless courage by any of his later Arctic adventures was his crossing of Vosseskavlen on snow-shoes in midwinter. In 1886 there came a visit to Dohrn's Biological Station at Naples; but the strictly biological studies were dropped in the following year, when the plan of crossing the Greenland ice-sheet had been definitely formed. It is not necessary to recount this achievement, the success of which raised Nansen at once to one of the highest places amongst Arctic travellers, brought him a shower of distinctions, and prepared the way for the triumph of his Arctic drift.


Hans Nansen.

That Nansen has found his sphere in the work of polar exploration is undoubted. By race, ancestry, and upbringing, adventurous travel was his inevitable destiny, and success was assured by a combination of qualities which are rarely found together. His marvellous physique comes first, making him as nearly impregnable to cold, fatigue and hunger as ever man was; then his equally remarkable determination and daring, qualities of mind which urged his physical powers to the uttermost, and made retreat from any path once entered upon an impossibility. His friends knew that if Fridtiof Nansen did not return successful from any quest, he would never return at all. The principle which most shocked previous Arctic explorers was his rule of providing no means of retreat, in fact of making retreat impossible—the principle of *Fram*. These qualities of body and mind are frequently combined in "record-breakers" of every kind; their combination may result in a champion prize-fighter, a professional football player, a 30-mile-an-hour cyclist, a peak-conquering mountaineer; they ensure, in fact, merely a forced-draught motive power. The third element is that of training, the educational discipline of home and school succeeded by the scientific discipline of the university and the laboratory. This gave direction and controlling power to the fervid energy; but the capital importance of this fact has not been adequately set forth in the biography. It may have been recognised by the authors, but it has been obscured by a quite unnecessary mass of irrelevant matter. A case like this is a splendid proof of the superiority of scientific education over any merely classical teaching in developing the whole power of a man.

We believe that Nansen owed his success in both his great journeys to the fact that he could himself study the conditions he had to meet, and plan his method of meeting them; that having studied and formed his plans himself, he could also carry them out himself with the aid of a few devoted companions. The contrast of the expeditions planned by a large Committee, and executed


Fridtiof Nansen.

by a large crew under orders they cannot deviate from, is quite apparent. The plan suggested for obviating railway accidents, by mounting a director in front of the engine, was that which Nansen adopted. He was ready to stake his life on the accuracy of his methods; and if his biological studies did not settle the problems of the nervous system, or even of the hag-fish, they certainly disciplined his powers of observation and of reasoning, and so enabled him to succeed and to excel in his chosen career.

A few slips in translating or printing may be pointed out. On p. 80 we read, "He pursues the paltriest insect revealed by the microscope, no less impetuously than he pursued the bears over the Arctic wastes." Here *insect* stands apparently for the fine old *animalcule*, although *paltriest* is even in that case a curious word to use with reference to an object of biological research. On p. 119, "the English zoologist G. P. Cunningham" should, of course, be J. T. Cunningham. In comparing the cold of the Greenland Ice-cap with that of Northern Siberia, the very important correction to sea-level is apparently left out of account (p. 205), but the passage is not clear. It was the younger, not the elder, Ross (p. 241) who conducted the successful Antarctic expedition. The historic drift of the *Hansa* in the ice was not "from Smith Sound right down to Davis Strait" (p. 281), but, as correctly given at two other places in the book, along the east coast of Greenland. The "geographical congress which lasted a week" (p. 289) at Newcastle in 1889, was the British Association, Section E of which was addressed by Nansen. On p. 291 the remarkable statement that Dr. Nansen was presented by the Royal Geographical Society with "the patrons of the Victoria medal," is resolved, on reference to the authority cited, into "the Patron's or Victoria Medal."

We miss any statement in the preface as to whether Dr. Nansen gave his approval to the publication of this translation of a work which was compiled in his absence, from data which must have been very incomplete. We are reluctant to suppose that his friends would, without his express sanction, have published so much of a purely personal and, sometimes, of a private nature, and the suspicion that they may have done so should have been made impossible. HUGH ROBERT MILL.

CELEBRATION OF PROF. CANNIZZARO'S JUBILEE.

WITH an impressiveness worthy of the high scientific value of the man who was honoured, the seventieth birthday of Prof. Stanislao Cannizzaro was celebrated on November 21 in Rome. In the wide amphitheatre of the Chemical Institute of the Royal University, in the same place where the illustrious investigator's activity was continually shown, many of the highest and most distinguished persons of the Eternal City met to do honour to him. Colleagues, friends, pupils collected to pay to this renowned chemist their tributes of esteem, veneration and affection, and in these feelings the whole scientific world joined.

No company more distinguished ever sat in those school benches. There were Senators Cremona, Tomasi-Crudeli, Todaro, Blaserna, ex-Minister Bacelli, Profs. Beltrami, Grassi (Darwin Medallist), Strüver, Luciani, Cerruti, Helbig, Bovio, Prof. W. Longuinine of Moscow, and many others.

When, accompanied by the President of the Council, Marquis Di Rudini, by the Under-Secretaries of State, Hon. Galimberti and Arcoleo, by the Prefect Count Bonasi, and the Rector Magnificus, Prof. Semeraro Cannizzaro entered the hall, the audience burst into long and loud applause.

Prof. Senator Paternò opened the proceedings by saying that the Committee spontaneously formed among Prof. Cannizzaro's students has been obliged to confine itself to a few things, not for want of means, as many offers were sent from various parts of the world, but on account of the desire of Cannizzaro himself. Prof. Paternò presented a gold medal of admirable workmanship, which at one side holds in relief Cannizzaro's imagine, and at the other the following inscription: "To Stanislao Cannizzaro scholars and admirers, on the occasion of his seventieth birthday." He presented also an artistic bust of Cannizzaro in bronze from one of his English admirers, and numerous pergamenas, addresses, letters, telegrams, sent by the most important scientific Societies of the world. An address was sent by the Royal Society of London; and the Faculty of Science in the University of Heidelberg sent a pergamena in Latin. Among the bodies which sent letters of congratulation were the "Académie des Sciences de Belgique," the Italian "Accademia dei Quaranta," and the Academies of Turin, Naples, Bologna, Venice, Milan, Catania. The Academy of St. Petersburg sent the following telegram.

"L'académie impériale des sciences rempli de consideration pour les travaux de l'illustre savant participe aux voeux et felicitations unanimes à l'occasion de son jubilée.

"Le sécretaire perpetuel,

"General Lieutenant Doubrovine."

Among the foreign Chemical Societies, those of London, Berlin, St. Petersburg, the American Chemical Society sent addresses:

Congratulatory letters and telegrams were also received from the Société chimique de Paris, Bucarest, Heidelburg, Munich, from the *Verein Deutscher Chemiker*, from the Chemical Society of Finland, the Badische Anilin und Sodafabrik, the Chemical Laboratories of Tübingen, Bucarest, Lisbon, and from all the Italian Universities. The Chemical Society of Aix-le-Chapelle sent a pergamena.

Among the most eminent persons who sent their best wishes to Cannizzaro, we take notice of the following: Lieben, Baeyer, V. Meyer, Mallet, Alnovillicus (Erlenmeyer, sen), Curtius, Wislicenus, Hantzsch, Fittig, &c., General Annibale Ferrero, Italian Ambassador at London, Ministers Guicciardini and Codronchi, Prof. Cosfa (Turin), Prof. Ugo Schiff (Florence), &c. The University of Kasan have made Cannizzaro Honorary Professor on this occasion; and the Grand-Duke of Baden conferred on him the "Komandeurkreuz 1e Klasse des Zähringer-Löwes."

Prof. Semeraro, Rector Magnificus of the Roman University, delivered the following address:—

"This festival does not only belong to the University of Rome, but also to the world's science. Cannizzaro's work is to be considered as having two parts, the one dear to the world, the other dear to us. We have seen the former in the addresses, letters, telegrams sent on this occasion; the latter is to be found in his teaching career, which is nearer to his heart.

"When Cannizzaro received the Copley Medal, the celebrated man said, he has been but a teacher, he had but loved the school and his pupils. My science, said he, has been the aim of my life for their instruction and warfare.

"His greatest glory lies in the fact that most of the professors now teaching in Italian Universities have been his scholars. The pressure of business, as Vice-President of the Senate, and Member of the Superior Council of Public Instruction, and many others, never were pretexts to him for overlooking the modest duty of a teacher. That is what makes him glorious in our University.

"Since Cannizzaro was admitted in Senate for his own scientific merits, he has never rested. He acquired from the Government the means for studying, and creating the first great Italian chemical school, and we all have seen what a happy success he has obtained by these means.

"To-day we can say on Cannizzaro what is said in an ancient inscription of a great Roman church, '*Virtude vixit, fama vivit, gloria vivet.*'"

In presenting to Cannizzaro the Grand Cordon of the Crown of Italy, Hon. Galimberti, Under-Secretary at the Public Institution Office, said:—

"This adds nothing to your fame, but is a proof that your own Government joins with those around you in their congratulations. People could give you this decoration with greater title, but none with greater affection and devotion than I do. Your name, as the Royal Society of London has said, is worthy of being joined with those of Galileo, Torricelli, Volta, and Galvani. To Emanuel Kant, who, in his absolute sentence, considered chemistry as a union of empirical knowledge, you replied half a century ago, pronouncing among the confusion of doctrines immovable ideas and true laws as to render chemistry an exact science, for it lies now on mathematical truth."

Cannizzaro replied to the congratulations in an interesting speech, which we hope to refer to more fully in a future issue. In the course of his remarks he referred to the confusion and uncertainty which dominated chemistry between 1850-51 on the criterions for determining atomic weights and on the value of formulæ, giving an account of the attempts made by others and himself to reduce chemical notation to law and order.

He afterwards referred to the subject of the division of pure science from teaching. He cited De Candolle's book, "Histoire des Sciences et de Savants," to the effect that the two functions of teaching and contributing to the science progress are to be separated. De Candolle hoped for the time when academies would be formed by free investigators. Cannizzaro is of contrary opinion, and dwelt upon the utility of teaching. He said: "Had I not been a teacher, my publications would not have appeared, and I should have continued to disseminate science of new carbonium compounds. I bring here Lord Kelvin's example, who, in his last jubilee, spoke of the utility he had found by the continued conferences with his scholars."

In speaking upon chemical laboratories, Cannizzaro expressed the opinion that the Director ought to dwell in the same building where the laboratories are situated. He thinks the German type of laboratory the best of all; for he said: "The chief must be considered in a scientific institute as the first, and more useful instrument."

Cannizzaro concluded by referring to the large sum obtained by subscription on occasion of his jubilee, and devoting the remainder of it to the extension of the Roman laboratories.

The celebration was concluded amid great enthusiasm and prolonged applause. A. MIOLATI.

THE FINAL ENTOMBMENT OF PASTEUR.

THE remains of Pasteur, which for nearly fifteen months have been lying at Notre Dame, were on Saturday, Dec. 26, borne in solemn procession to their last resting-place at the Pasteur Institute, where a crypt worthy to hold the ashes of that great benefactor of the human race has been constructed. The subjoined report, slightly abridged from the *Times*, shows that the ceremony of the final entombment was an impressive one, and that representatives of British science were among those who, by their presence and their words, testified to the high regard in which the name of Pasteur is held.

As at the original funeral, there were present a number of eminent men of science and thinkers, both French and foreign, and the ceremony derived additional grandeur from the gathering which witnessed the final interment. The man whose ashes lie in this tomb erected by filial piety and universal admiration is depicted in a sentence from his reception speech at the Academy which the architect has inscribed on the stone: "Heureux celui qui porte en soi un dieu, un idéal de beauté, et qui lui obéit—idéal de l'art, idéal de la science, idéal de la patrie, idéal des vertus de l'évangile." The English deputation included Sir Joseph Lister, President of the Royal Society, Sir Dyce Duckworth, Sir John Evans, and Sir William Priestley. Dr. Metchnikoff represented Russia, and Dr. van Hoorn Holland. M. Méline and M. Rambaud represented the Government, and M. Brisson, a number of ex-Ministers, Senators and Deputies, many members of the Institute and the Prefects of Police and of the Seine were among those present.

There was, first of all, a service at Notre Dame, attended by the family and the staff of the Pasteur Institute, who then, with the archpriest, followed the hearse to the Institute. A crowd had collected outside, and amid an impressive silence every head was bared as the coffin was carried up the steps through the grand vestibule and down to the crypt, which was decorated by the wreaths sent by English, Russian, and French societies and institutions. The parish priest pronounced the last prayers as the coffin was deposited in its last resting-place in the presence of the family. They then remounted the steps to the entrance of the crypt, where all the spectators were stationed, and M. J. B. Pasteur, addressing the Council of the Institute, said: "I entrust to you this tomb which we have raised to our father in this Institute which he loved so dearly. We beg you to preserve it carefully." M. Bertrand, President of the Council, thanked the family for their pious idea, thanks to which, he said, the pilgrims who would come from all parts of the world to honour the memory of the great benefactor of mankind would be able to meditate by his tomb. M. Rambaud, Minister of Education, next delivered a warm eulogium. "As those tombs of saints," he said, "on which people saw prodigies accomplished, that of Pasteur will be encircled by a halo of miracles. At every discovery beneficial to mankind, at every ray of scientific glory which will be added to the aureole of France, the gratitude of the country and of the universe will flow to this building, henceforth august in the annals of science, as to the source of all ulterior progress." M. Baudin, President of the Municipality, was the next speaker, and an address by M. Legouvé, the father of the French Academy, was read by M. Gaston Boissier.

Sir Joseph Lister then said: "Je suis chargé de représenter la Société Royale de Londres, le Collége Royal des Chirurgiens d'Angleterre et la Société Médico-Chirurgicale de Londres. Aussi j'ai fait déposer, de la part de l'Institut Britannique de la Médecine Préventive, fille de l'Institut Pasteur, une couronne ici. Il y a quatre ans, à l'occasion du jubilé de Monsieur Pasteur, j'ai eu le suprême honneur de lui présenter, au nom de la Médecine et de la Chirurgie du monde entier, l'hommage de leur reconnaissance. Aujourd'hui j'assiste à ses funérailles! Cette cérémonie est noble et imposante, digne de la mémoire de notre vénéré maître. Mais elle nous remplit d'une profonde tristesse, puisqu'elle nous rappelle que cette grande lumière de la science, si ardente et si claire, est éteinte; que ce caractère, si noble et si aimable, a disparu de notre monde."

Sir Dyce Duckworth spoke as follows: "Au nom du Collége Royal des Médecins de Londres, j'ai l'honneur de témoigner la vive sympathie avec laquelle les médecins d'Angleterre s'associent à cette touchante cérémonie. Je porte l'hommage de nos confrères à ce grand génie créateur, à ce noble caractère de Louis Pasteur. Pour nous il est un des prophètes de la science, il en est un des avant-coureurs les plus éclairés et les plus intrépides. Vénérons ensemble ce bienfaiteur de l'humanité! Vénérons ce Chrétien loyal et convaincu, dont la foi a résisté à toutes les influences matérialisantes de ses études! Que l'œuvre de Pasteur, dont les cendres reposeront désormais dans ce sol consacré par son travail, reste une des gloires impérissables de la France et du monde!"

Sir W. Priestley said the two Scottish Universities of Edinburgh and St. Andrews appointed him as their representative, in token of respect for the memory of one who had done so much to advance the interests of science, and who had conferred such signal benefits on mankind. He remarked that Pasteur not only made great discoveries himself—discoveries which had conferred priceless benefits on man and on the lower animals—but he opened up new and hitherto unexplored paths in the field of science, the horizon of which was almost unlimited.

M. Cornu, for the Academy of Sciences, M. Bergeron, for the Academy of Medicine, M. Perrot, for the Normal School, M. Louis Passy, for the Agricultural Society, M. Tissier, for the medical students, and M. Duclaux, director of the Pasteur Institute, also spoke. A feeling of restrained emotion prevailed during the entire ceremony, at the conclusion of which the spectators passed respectfully before the tomb and greeted the widow and family of the illustrious investigator.

NOTES.

WE deeply regret to have to record the death of Prof. Emil Du Bois-Reymond, the eminent professor of physiology in the University of Berlin. Before his burial in the French cemetery in Berlin, on Tuesday, addresses were delivered in the Physiological Institute, of which Du Bois-Reymond was the founder. The Emperor and the Empress Frederick have telegraphed expressions of condolence to the widow and relatives of the deceased.

A BOTANICAL Institute has recently been opened in connection with the Botanical Garden at Münster, in Westphalia.

IN deference to public demand, the new aquarium of New York City will be open to visitors on Sunday after January 1.

THE report that M. Nobel, the inventor of dynamite, has bequeathed his property, estimated at fifty million francs, to the University of Stockholm, is contradicted.

A PLAN is proposed in the American botanical journals for the establishment of an American tropical botanical laboratory on the plan of that at Buitenzorg, either on the east coast of Mexico, or on one of the islands near the Caribbean Sea.

THE *Botanical Gazette* states that a notable cactus-garden has been established at the University of Arizona. It is the purpose to bring together eventually all the Cactaceæ indigenous to the United States; already more than one hundred species are represented.

PLANS have been prepared for the buildings in the Botanical Garden of New York City. The buildings, with their approaches, will occupy twenty-five acres of ground. They will be dispersed in different parts of the grounds, throughout the garden, and not grouped together.

WE regret to announce the deaths of Mr. G. F. Schacht, treasurer of University College, Bristol, and formerly vice-president of the Pharmaceutical Society of Great Britain; Dr. L. J. Sanford, formerly professor of anatomy and physiology in Yale University; and Dr. Emil Wolff, of Stuttgart, well known for his works in agricultural chemistry.

A VERY remarkable landslip occurred early on Monday morning, at Rathmore, about twenty miles to the east of Killarney, and on the confines of the county Cork. As a result, possibly, of the almost incessant heavy rains of the past few weeks, a considerable portion of bog land slipped from its position, and, taking a southerly direction, swept everything in its course for a mile or two. As a result of the slip a considerable tract of country has been submerged, and the rivers in the neighbourhood are flooded with peaty water.

THE Academy of Natural Sciences of Philadelphia has decided to confer the Hayden Memorial Award for 1896 upon Prof. Giovanni Capellini, of Bologna. Capellini was born in Spezia, August 23, 1833. While yet a student he had made important palæontological discoveries and was in correspondence with illustrious investigators, both Italian and foreign. In September 1859 he was appointed Professor of Natural History in the National College of Genoa, and in the following year he became Professor of Geology and Palæontology in the University of Bologna. The rich collections made by him in Nebraska and elsewhere, while on a visit to North America in 1863, are now in the Geological Institute of Bologna. In 1864 he made interesting scientific discoveries in the petroleum lands of Wallachia. As President of the Second Extraordinary Reunion of the Italian Naturalists in Spezia in 1865, he founded the International Congress of Anthropology and Prehistoric Archæology. He was made Vice-President of the First International Geological Congress in Paris in 1878, and obtained its assent that the second meeting should take place in Bologna in 1881. Elected actual President (in conjunction with Quintino Sella as honorary President) of this Congress, he inaugurated the commission for the unification of geological nomenclature and a commission for the production of a geological map of Europe, outlined at Berlin. Together with Sella he founded, on that occasion, the Italian Geological Society. In 1885 he directed, in great part, the third International Geological Congress in Berlin, and contributed not a little to its success, as also to that of the fourth session in London in 1888. He had now published 140 scientific communications. Having served as Rector of the University of Bologna at intervals from 1874 to 1888, in the latter year he organised and directed a celebration of its eighth century, for which he received letters of congratulation from all the universities of the world. He has been decorated by the Emperor of Germany and other Sovereigns. The University of Edinburgh conferred upon him through its Rector the diploma of Doctor "Honoris Causa." The University of Moscow nominated him honorary Professor. Seventy of the principal academies of Europe and America have registered his name among their members. He was elected a Correspondent of the Academy of Natural Sciences of Philadelphia in 1863.

THE American naturalists continue their researches into the variations and distributions of the multitudinous small mammals of North America with great industry. Besides the labours of Dr. Hart Merriam, to some of whose remarkable memoirs we have lately called attention, we have recently received copies of papers by Mr. G. S. Miller, jun., on the "Beach Mouse of Muskeget Island" (*Microtus breweri*), and by Mr. C. F. Batchelder on the distribution of various rodents in New England and Northern New York.

THE first part of vol. xviii. of Dr. Jentink's "Notes from the Leyden Museum," which we have lately received, contains mostly papers on entomological subjects. There are besides these, three communications from Dr. J. Büttikofer on birds, including the description of a new duck from the island of Sumba (*Anas salvadorii*), which presents rather anomalous characters. It is curious that many of the contributors to this Dutch periodical write in English (which is likewise the language of the title-page), though others use French and German.

SOME American naturalists have taken to give not only the year and month, but even the day and hour of the "distribution" of their "advance sheets." To such an excess is the rush for "priority" now carried! Mr. Outram Bangs has lately made out that the reindeer of Newfoundland (known to be abundant there since the discovery of the island) possesses certain differential characters from the continental forms, and names it *Rangifer terræ-novæ*, dating the notification of this important event on "Wednesday, November 11, 1896, at 5 o'clock p.m." We trust that no British naturalist has yet made the same discovery, or grave international complications may arise.

THE distribution in time of the after-shocks of earthquakes was successfully investigated two or three years ago by Prof. Omori, of Tōkyō (see NATURE, vol. li. p. 423). In an interesting supplement to his valuable memoir, Prof. Omori discusses the after-shocks of the great Japanese earthquake of November 4, 1854, and shows that their decline in frequency obeys the same law as those of the more recent cases examined. The monthly number (y) of after-shocks at Tosa until the end of 1855 is found to be given very nearly by the equation $y = 225{\cdot}2/(x + 1{\cdot}098)$, where x is the time expressed in months, since December 1854. For the whole year 1895, this equation would give between five and six shocks, while, during the years 1885-91, the mean annual number of shocks actually recorded at Tosa was 4·3. The agreement is so close that Prof. Omori seems justified in regarding it as not accidental.

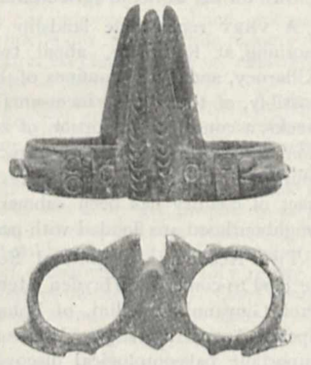
THE *Boletin de la Sociedad Geográfica de Madrid* contains a paper by D. Emilio Bonelli, describing recent explorations in the island of Fernando Po. The coast of this island has long been fully known, but the forest regions surrounding the Peak of S. Joaquin have not hitherto been penetrated. On an excursion round the Bay of Concepcion, Father J. Juanola, a missionary, found himself at the edge of a funnel-shaped chasm some 300 metres in depth. At the bottom of this funnel, probably the crater of an extinct volcano, was a lake about 1200 metres long and 800 metres wide at its broadest part, to which the name Lago Loreto was given. So far as appeared, this lake had no outlet, and was fed by a subterranean stream; the surface was estimated at 1350 metres above sea-level, and the surface temperature of the water was 14° Réaumur.

DIRECTOR KELMERT has presented to the Prussian Academy of Sciences a memoir on the variations of the force of gravity along a line from Kolberg to the Schneekoppe, by Arnswalde, Gröditzberg, Grunau, and Giersdorf, founded on a combination of German and Austrian observations at twenty-two stations. A normal curve of change of intensity is drawn between the two points, and the departures from this curve at particular stations are discussed in detail. The *Mittheilungen* of the Geographical Society of Vienna contains an abstract of the results, which show that an abnormal increase in the force of gravity can be traced to the presence of dense rock masses below the surface, and an abnormal decrease to the partial or total removal of unexposed strata. Under certain assumptions it is possible to estimate the thickness of these subterranean strata; on the Pomeranian lake plateau a layer of about 210 metres in thickness is responsible for increased intensity, while near the Schneekoppe a decrease, which cannot be ascribed to lower density at the surface, is probably due to subterranean layers 200 metres in thickness. These disturbing elements do not in all probability lie deeper than 20 or 30 kilometres. The deflections of the plummet from the vertical are also discussed with interesting results; the maximum deflection, amounting to 18·1", was observed at the Alter Bruch Station on the slopes of the Schneekoppe, at an elevation of 917 metres.

IN the current number of the *Atti dei Lincei*, Dr. G. Folgheraiter concludes his investigations on the state of terrestrial magnetism in the Etruscan epoch as revealed by observations of old vases (see p. 40). The results appear to indicate that in the eighth century B.C. the magnetic dip in Central Italy was small, and in the reverse direction to what it is at the present time, so that a magnetised needle at that epoch would have assumed a position with its south instead of its north pole pointing downwards. Some two centuries later, the declination seems to have approached the value zero, the earth's magnetic field being nearly horizontal, while in many of the vases the direction of polarity is the same as at the present time. There are, of course, many difficulties in the way of drawing precise conclusions from these observations—such, for example, as the uncertainty as to the date of fabrication of the vases; but the results are sufficiently consistent to establish the validity of the method. Dr. Folgheraiter points out that the prevalence of austral magnetism at the bases of Etruscan vases might be interpreted either on the supposition that the magnetic equator has been so displaced as to pass to the north of Etruria, or by admitting that the northern and southern hemispheres had their magnetic polarity opposite to that of the present time, and that the latter state may have been arrived at by successive variations in the magnetic declination. The present state of knowledge does not permit more weight to be given to one hypothesis than to the other; nevertheless, the author already has an idea of trying if it may be possible to throw some light on this question by means of suitable investigations.

THE latest number of the *Centralblatt für Bakteriologie*, Part ii., contains a paper by Messrs. Stutzer, Burri, and Maul, on the capability of growing on foreign culture media exhibited by the *Bacillus radicicola.* These bacilli have attracted a great deal of attention recently in connection with Dr. Nobbe's investigations. It will be remembered that Nobbe has isolated out seventeen or more of these nitrogen assimilating bacteria from the root-nodules of various leguminous plants, and has endowed them with the collective title of Nitragin. The present memoir describes some experiments made with these novel bacterial soil fertilisers derived from lucerne plants. Culture media were prepared from these plants, and also from white mustard plants, by adding to the infusions respectively derived from them, gelatine and grape sugar, the whole being then sterilised as usual. Lucerne-nodule-bacteria were inoculated on to both these different media, and their cultivation was carried on through several generations. Whereas on the lucerne gelatine the bacteria flourished abundantly up to the last, on the mustard gelatine they gradually faded away. It was next tried if these lucerne-nodule-bacteria could be induced to thrive on this mustard medium by gradually training them to become accustomed to this foreign soil. In the course of six months these bacteria had completed their education, and they accepted the mustard gelatine as eagerly as they had before the lucerne gelatine. This highly successful result led the investigators to think that possibly in the interval these bacteria might have lost all taste for their original culture food, but inoculation from these mustard-grown bacilli on to lucerne gelatine showed that this anticipation was not justified, for they grew with their usual luxuriancy. It would be interesting to determine if by suitable training nodule bacteria could be induced to fertilise or "nodulise" leguminous plants other than those from which they were originally derived.

ARCHÆOLOGISTS have long been puzzled over certain bronze objects about 3 inches in length, which consist of two rigid rings united by a short band, from which three spurs project. These may be found in collections of Etruscan, Roman. and Greek antiquities in museums, where they are usually labelled as bow-pullers. Mr. E. S. Morse, the Director of the Peabody Academy of Science, has been interested for seven years in these problematical objects, and now carefully describes and figures a number of them in the *Bulletin of the Essex Institute* (vol. xxvi., 1894, p. 141). Some ten purposes have been suggested for their use; as a practical archer the author dismisses the bow-puller hypothesis, and, indeed, he says he must "reluctantly yield the solving of the enigma to others, having got no nearer an explanation of it than when I first began." Perhaps the most plausible view is that it may have been bound to the band to enable a chariot driver to hold the reins more firmly in driving.



DR. F. TETZNER concludes in *Globus* (Band lxx. Nr. 18) his series of four articles on the ethnography of the Kaschuben of the Leba Sea, in North-west Pomerania. The Kaschuben are the last Slavonic remnants of the Pomeranians in this district. He gives an interesting account of the folk-lore of this remote people, including some folk-songs, rhymes on grave-stones, and a few verses of children's singing-games, among which is the widely-spread "Ringel, ringel Rosenkranz"; but poor though the Kaschuben are in songs, they are rich in traditions and folk-tales. "Stupid Hans," who is so popular in German, Slavonic, and Lithuanian tales, plays the chief part in their märchen and narratives. Another important incident is "The Cheated Devil"; "The Wild Huntsman" comes next in popularity to the Devil as every one has seen him. The articles conclude with sections on manners and customs and festivals.

FROM the concluding summary of the paper on "First Records of British Flowering Plants," in the *Journal of Botany*, it appears that, of the 1440 species now reckoned as British, 510 have been discovered within the last two centuries, since the time of Ray. The earliest records are by Turner, 1538–1568, who seems to have known about 230 species; by Ray's time this had been increased to 725; and he brought the number of British flowering plants up to 930.

AN interesting fact in connection with the Schwendenerian theory that lichens are compound organisms consisting of an algal and a fungal constituent, which carry on a mutually symbiotic existence, is contributed to *Le Botaniste* in a paper by M. P. A. Dangeard. He states that a common and destructive disease of the Lombardy poplar in Italy is caused by a species of *Calicium* which he finds on the living branches, and which he names *C. populneum*. The genus is usually placed among the lichens; but this species is a pure fungus, destitute of any algal constituent, and carries on a parasitic life on the living tissues of the host.

A VERY remarkable degradation of the ovule takes place in some genera of parasitic plants, and is especially described by M. P. van Tieghem in a paper in the *Bulletin* of the Botanical Society of France, in the case of some Balanophoraceæ and Loranthaceæ. In one section of the former order there is in each carpel a single hypodermal endosperm-mother-cell which becomes the embryo-sac. A similar phenomenon occurs in *Arceuthobium* among Loranthaceæ, where two endosperm-mother-cells are produced at opposite sides of the ovarian cavity, and where, therefore, there is a placenta, but no ovules. In *Balanophora* the reduction is carried still further; the monocarpellary ovary produces beneath the epiderm a single endosperm-mother-cell; there is, therefore, here neither ovule nor placenta. This reduction of the ovule is frequently accompanied, especially in certain genera of Loranthaceæ and Balanophoraceæ, by a peculiarity in the mode of impregnation, which van Tieghem terms (in a paper in Morot's *Journal de Botanique*) *basigamous*, in contrast to the usual acrogamous mode. The ordinary position of the "egg-apparatus" or embryonic vesicles and of the antipodals is here reversed, the former being located at the lower, the latter at the upper end of the endosperm-mother-cell. This fact seems to confirm the theory that there is no physiological difference between embryonic vesicles and antipodals, except that the former are usually active, the latter inactive.

THE Austrian Meteorological Society has published an index to the first twenty volumes of its *Zeitschrift*. The work has been prepared by Dr. S. Kostlivy in a very careful manner as a mixed author and subject catalogue, arranged alphabetically. It is well known that the *Zeitschrift*, which was edited by the late Dr. Jelinek and by Dr. J. Hann, is a complete repertorium of climatology and terrestrial magnetism for all parts of the globe. This handy index, referring to the years 1866–85, will therefore be a boon to scientific men who may wish to obtain the results of observations made in any particular locality, without the trouble of referring to, and reducing the original records.

THE Meteorological Observatory of Upsala has published a series of concise tables for photogrammetric measurements of clouds. The usual calculations for determining the height of clouds are always tedious, especially when the photogrammetric method is employed, which, although it renders much greater precision possible than direct methods without photography, introduces fresh corrections and reductions. The tables in question, which have been calculated by Mr. J. Westman, will be found useful for the solution of the trigonometrical values of the cloud measurements which are now being specially made in various countries, in accordance with the wish expressed at the Meteorological Conference at Munich.

MR. W. F. CLAY, of Edinburgh, has sent u a copy of a new and enlarged reference list (No. 80) of books and memoirs on chemistry and the physical sciences generally. Individuals or institutions desiring to add important works to their libraries should procure a copy of Mr. Clay's catalogue.

THE following are the arrangements for lectures at the Royal Victoria Hall, Waterloo-road, S.E., during January, on Tuesday evenings, at 8.30:—January 5, Prof. H. G. Seeley, F.R.S., on the heat of the earth; January 12, Mr. A. Smith Woodward, on Syria and the Syrians; January 19, Mr. A. E. Tutton, on the glaciers and snowfields of the higher Alps.

WE have received the *Papers and Proceedings* of the Royal Society of Tasmania for 1894–95. The volume contains some interesting historical papers: one on the deportation of the Norfolk Islanders to the Derwent in 1808, by J. B. Walker; another on a MS. chart in the British Museum, showing Tasman's tracks in the voyage of 1642–44, by A. Mault; and a third, on early voyages to Papua in 1511 and 1545, by J. R. McClymont. A large number of field observations in botany, geology, zoology and ornithology are put on record, and Mr. H. C. Kingsmill gives an account of meridian observations with the Hobart transit instruments. The Society is largely interested in Antarctic exploration, and in the extension of the Australian meteorological service.

AN important addition to the synthetical methods for the preparation of carboxylic acids has been made by Messrs. Bredt and Kallen, whose work is recorded in the last instalment of the *Annalen*. They have found that unsaturated acids which contain two negative radicles united with one of the unsaturated carbon atoms readily unite with the elements of hydrocyanic acid, when the corresponding ethereal salt is boiled with a solution of potassium cyanide in dilute alcohol. Ethyl benzylidene-malonate, for example, which is formed by the condensation of benzaldehyde with malonic ether, is thus converted, with the loss of a carboxyl-group, into ethyl β-phenyl-cyanopropionate, $C_6H_5.CH(CN).CH_2.COOC_2H_5$, from which phenylsuccinic acid can easily be prepared. Certain unsaturated lactones, such as coumarin, also readily unite with hydrocyanic acid in this way, and there can be little doubt that the new reaction will prove of immense importance in the development of our knowledge of the carboxylic acids.

OWING to the fact that lithium is the only substance with which nitrogen will directly combine at ordinary temperatures, considerable interest attaches to the properties of the compound thus formed, lithium nitride. This compound, as M. Guntz showed some months since, is easily prepared by the combustion of lithium in nitrogen gas, and the specimen thus obtained was regarded as pure, since the nitrogen absorbed corresponded almost exactly with that required by the formula Li_3N. But in spite of this coincidence M. Guntz now shows that the product is not pure, as at the moment of combination the lithium attacks the boat in which it is held, although it is difficult to say exactly in what form the metal goes into solution in the molten nitride. Thus working with iron boats, the nitride contained from 2 to 8 per cent. of iron, and nickel is even more strongly attacked. Silver, platinum, quartz, and graphitic carbon were all tried, and found to be even less suitable than iron. Measurements of the molecular heat of formation of the nitride gave a value of 49·5 calories, or less than the value for the equivalent quantity of lithium hydride (65 calories). From this the conclusion was drawn that hydrogen ought to decompose the nitride of lithium with formation of the hydride, and this was found to be the case. The reaction, however, can be reversed, since at a high temperature a stream of nitrogen converts the hydride into the nitride.

THE additions to the Zoological Society's Gardens during the past week include a Golden Eagle (*Aquila chrysaetus*) from Norway, presented by Lord William Beresford, V.C.; a Raven (*Corvus corax*), British, presented by Mr. J. Collingham; two Tree Frogs (*Hyla arborea*), European, presented by Master Kneeshaw; a Grey Parrot (*Psittacus erithacus*) from West Africa, deposited.

OUR ASTRONOMICAL COLUMN.

COMET PERRINE (DEC. 8).—In *Astronomischen Nachrichten* (No. 3391) are given the elements and an ephemeris of the comet which was discovered by Mr. Perrine on December 8 last. This shows that the comet is decreasing in declination and increasing in right ascension, its position for December 30 being given as R.A. (apparent) 3h. 9·7m., Decl. (apparent) – 0° 9′. Its brightness is now about half what it was on December 10. The elements, communicated also by Dr. F. Ristenpart, are very nearly similar to those referred to above. These are given in a later number of the *Astronomischen Nachrichten* (No. 3393), together with an ephemeris, calculated by Dr. Ristenpart, up to the middle of January, from which we make the following extract:—

12h. Berlin M.T.

1897.	R.A. 1897·0 h. m. s.	Decl. 1897·0 ° ′	log *r*.	log. Δ.	Br.
Jan. 1 ...	3 28 11	... – 0 49·1			
2 ...	33 28	... 0 56·7			
3 ...	38 38	... 1 3·5	... 0·1030	9·6149	0·46
4 ...	43 41	... 1 9·5			
5 ...	48 36	... 1 14·7			
6 ...	53 24	... 1 19·2			
7 ...	58 5	... 1 22·9	... 0·1123	9·6482	0·38
8 ...	4 2 38	... 1 26·0			
9 ...	7 5	... 1 28·4			
10 ...	11 25	... 1 30·2			
11 ...	15 39	... 1 31·4	... 0·1222	9·6823	0·31
12 ...	19 48	... 1 32·0			
13 ...	23 52	... 1 32·1			
14 ...	4 27 46	... – 1 31·6			

A matter of some interest is the similarity of the elements which have been obtained by Messrs. Hussey and Perrine, and those of Biela's comet. The following shows the two systems of elements:—

	Perrine.	*Biela* 1832 *III.*
T =	1896, Nov. 25·67 M.T.G.	1832, Nov. 26·4 M.T.G.
ω =	164° 36′	109° 56′
☊ =	243 49	248 12
i =	16 29	13 12
q =	1·1540	0·8793

"HIMMEL UND ERDE."—The astronomical contributions to the December number of this monthly include, besides a somewhat lengthy obituary notice of M. Tisserand (with a portrait), the last of a series of articles by Dr. G. Witt on the planet Saturn, several illustrations accompanying the text; such recent work as that accomplished by Keeler regarding the constitution of the ring as deduced from the movement of the lines in the spectrum, and Campbell's spectroscopic work are both referred to at some length. Of the shorter articles, an interesting account is given of Prof. Newcomb's important work on the transits of Mercury across the sun's disc. By using newly-constructed sun- and Mercury-tables, Prof. Newcomb still found that differences between calculated and observed values were obtained. How to account for these was his next object of research. Might not such differences be due to a false assumption in assuming that the earth rotates at a constant speed around its axis? Or are they the results of inequalities in the moon's motion? Prof. Newcomb finally concluded that in the mean motion of the moon there must be one, if not more inequalities of long period which our present theory has not yet analytically proved.

ASTRONOMICAL SOCIETY OF FRANCE.—The *Bulletin* of this Society for the month of December contains, among other matters, an interesting address by M. Janssen on the late Director of the Paris Observatory, M. Tisserand. M. Gilbert concludes in this number his article on the mechanical proofs of the rotation of the earth, dealing here chiefly with the experiments made with various kinds of gyroscopes. Among the notes will be found a description, by M. Camille Flammarion, of a pulpit sculptured in wood, having the form of an inclined terrestrial globe with the continental outlines worked on it. The south pole is situated underneath, and bears the inscription, "Regiones australes incognitæ." It was constructed in the year 1600, and is cut out of a single piece of oak. M. Flammarion discovered this pulpit in the Chapel of Saint-Sang at Bruges, a small church built in the year 1150, and restored in the sixteenth century and since.

THE DAVY-FARADAY RESEARCH LABORATORY.

THE Davy-Faraday Research Laboratory, established and equipped by Dr. Ludwig Mond, was opened by the Prince of Wales on Tuesday, December 22. We have already expressed our appreciation of this generous gift to British science, and have described the accommodation and equipment of the new laboratories (vol. liv. p. 200). With a munificence which we hope will find many imitators, and a just regard of the value of scientific research, Dr. Mond has established a place where investigations can be carried on without interruption, and with the best appliances. He has not only furnished the laboratory with the most modern instruments and appliances for researches in pure and physical chemistry, but has also given an ample endowment, so that the laboratory may be maintained in a state of thorough efficiency, his object being to give every assistance and encouragement, within the limits of the endowments, to scientific workers. To accomplish this has cost a hundred thousand pounds, of which sum 38,000*l*. is sunk in the building and its equipment, while the remaining 62,000*l*. constitutes the endowment fund. For the very practical way in which Dr. Mond has shown his interest in the promotion of material knowledge, men of science cannot express too warm a sense of gratitude. We look to the workers in the laboratory to repay the generosity of the founder by their contributions to knowledge, and so induce other benefactors to follow the example set by Dr. Mond.

The following account of the opening of the laboratory, abridged from the report in the *Times*, will be read with interest:—

Dr. Mond, addressing the Prince of Wales, said that under the auspices of his Royal Highness's august father, whose enlightened mind had fully realised that the pursuit of pure science was the most potent factor in the promotion of the intellectual as well as the material progress of this or any other nation or of humanity at large, a movement was set on foot fifty years ago to found an institute for the pursuit of pure chemistry, which was not only to give practical and systematic instruction to students, but was also to provide a place where original research could be conducted by fully-qualified investigators. At first it was proposed to attach this institute to the Royal Institution. The eminent professors of the time, Faraday and Brande, expressed their strong approval of the intended project, and their desire that it might be carried out at the Royal Institution, if it could be done well; but, nevertheless, this idea had to be abandoned, because sufficient accommodation could not be found within the precincts of the institution. The first part of the scheme was carried out a few years later by the foundation of the Royal College of Chemistry, which, under the guidance of the illustrious Hofmann, soon became one of the most successful schools of chemistry in the world; but the second part, that of providing a place where original researches could be carried on by a number of independent investigators, had been waiting all this time for its realisation. Several years before these facts came to his knowledge he had determined of his own accord to found in London a laboratory of research in purely scientific chemistry and in physical chemistry, that borderland between chemistry and physics from which, in his opinion, they might hope to learn more about the real nature of things than from any other branch of natural science. He also had come to the conclusion that such a laboratory would derive the greatest advantage if it could be associated with the Royal Institution of Great Britain, which had during its long existence made the promotion of original research in these sciences one of its main objects, and the laboratories of which had been productive and were still productive of such marvellous results at the hands of the eminent professors elected by the institution. He therefore gladly embraced the opportunity which recently presented itself of acquiring the commodious house immediately adjoining the Royal Institution, and submitted a scheme to its managers, which met with their fullest sympathy and which they readily accepted with unanimity. Work was immediately commenced to alter the building so as to make it suitable for its new purpose, and, thanks to the advice which had been freely extended to him by scientific men all over the world, and the active co-operation of Lord Rayleigh and Prof. Dewar, of the architect, Mr. Flockhart, and of his son, Mr. Robert Mond, to whom he left the selection of the apparatus and the equipment of the place generally, the laboratory which they asked his Royal Highness to inaugurate that day would stand favourable comparison with any other laboratory in or out of England as to the completeness and convenience of its appliances, and was provided with the best instruments made at the present day. It was unique of its kind, being the only public laboratory in the world solely devoted to research in pure science. In order to insure its continued usefulness he had endowed it so as to cover the cost of maintenance of the fabric and all necessary current expenses. He named it the Davy-Faraday Research Laboratory in perpetual memory of those two great pioneers of science who carried out their world-famed and epoch-making researches almost on that spot, and whose example he hoped would stimulate and inspire every one who came to work under that roof. It was a source of very great gratification to him that the eminent successors of those great men, Lord Rayleigh and Prof. Dewar, had consented to undertake the duties of directors of the laboratory, and this gratification had been the greater because those gentlemen made it a condition of their acceptance of the post that it should be without emolument. An experienced superintendent had been appointed in the person of Dr. Scott, and nothing was now wanting for its success but a number of investigators competent and ardent to continue the great work of this century, the unravelling of the secrets of nature. As soon as his Royal Highness had declared the building open, persons of either sex or any nationality would be welcome within its walls who could satisfy the laboratory committee that they were fully qualified to undertake original scientific research in pure and physical chemistry, and preference would naturally be given to those who had already published original work. If this country had distinguished itself in one way more than another in that glorious rivalry with other nations for extending our knowledge of natural phenomena and our power over the forces of nature it had been by the large number of contributors to our knowledge, who on the continent would be called amateurs in science—men who devoted their lives to the study and advancement of science from pure love for the subject. He need only instance the names of Cavendish, Joule, and Darwin to say that they included men of the very highest rank. In giving this laboratory to the English nation he had done so in the firm conviction that this country would continue to bring forth in the future, as it had done in the past, men of the same rank and of the same devotion to science for its own sake, and it was a fond hope of his that such men would find there all the facilities and all the necessary appliances for carrying out their researches. The further we advanced in the study of nature the more accurate and elaborate was the apparatus required, and the more difficult it became to carry on delicate work in a private laboratory. He had placed tnat laboratory in the centre of London because he believed that this great city would continue to be the intellectual centre of the civilised world, where the brightest minds would congregate. He had intrusted it to the Royal Institution so as to insure its being open to men and women of all schools and of all views on scientific questions. It had given him great pleasure that in establishing the Davy-Faraday Laboratory, he had been able at the same time to enlarge the old laboratories of the Royal Institution, and also to make additions to its library and reception rooms, which he hoped would prove a convenience to its members. He looked upon that laboratory as an important step forward in that great movement for the advancement of scientific research in this country, to which his Royal Highness's revered and illustrious father gave so powerful an impulse, and which has been so distinguished a feature of the many-sided and unparalleled progress made by this nation during the glorious reign of his mother, her Majesty the Queen. It was a source of specially great satisfaction to him that his Royal Highness deemed that laboratory worthy to be opened by himself, and he humbly thanked his Royal Highness for having come there that day. His presence on that occasion would certainly add very greatly to the success of the Davy-Faraday Research Laboratory of the Royal Institution.

The Prince of Wales in reply said :—Prof. Mond, it affords me much satisfaction to assist at the opening of the series of beautifully-arranged and well-equipped research laboratories which this country owes to your generosity, and I congratulate the members of the Royal Institution of Great Britain upon this most important accession to the resources which have been placed at the command of the institution for the advancement of chemical and physical science. The Royal Institution has long enjoyed a world-wide reputation, thanks to the marvellous work of the succession of illustrious men whose researches, carried on within these walls, have very largely contributed to secure and maintain for this country a foremost position as a source of great discoveries and important advances in science and its applications. The identification of the laboratories which you have founded with the names of two of the most eminent of former professors of the Royal Institution and of English men of science—Humphry Davy and Michael Faraday—is a graceful act on your part. The fact that the present distinguished professors of physics and chemistry, Lord Rayleigh and Prof. Dewar, have undertaken the important duties of directors of the new research laboratories without any remuneration must afford most gratifying evidence to you of the great faith entertained by them in the benefit to the promotion of science which your wisely-applied munificence is destined to realise.

THE BACTERIA WHICH WE BREATHE, EAT, AND DRINK.[1]

THE surface of the earth is inhabited by bacteria : wherever there is dead organic matter, wherever there are human or animai excreta, wherever decomposition is going on, in stagnating or in flowing water, within our houses and without, bacteria collect. They are so widely distributed that practically everywhere we are surrounded by these minute vegetable cells. From the bacteriological standpoint we live amongst decomposing matter. Without bacteria there is no decomposition or putrefaction ; they reduce the organic matter to "dust," and with the atomised matter they are again carried away by air or water. Dust is laden with bacteria, and since a great part of dust is derived from decomposing matter, it follows that, although we do not realise it, we are living in an atmosphere of decomposition.

The air which we breathe, therefore, contains bacteria. These vary in amount with certain conditions. If the air is calm their number diminishes, but if there is wind or draught, they may be present in enormous numbers. Again, in the open country air there are, other things being equal, considerably less micro-organisms than in the dusty streets of London. Thus there is an extraordinary difference between the air in Oxford Street and on Wandsworth Common.

The air may be roughly tested by coating sterile plates of glass with gelatine, and exposing them for a given time in the locality which we wish to examine. The bacteria will fall on the surface of the gelatine, and on incubation at a suitable temperature they will develop into visible colonies which can be readily counted. The number of colonies is a fair, though not an absolute, index of the bacterial purity or impurity of the air. The more colonies we find on the surface of the gelatine, the more bacteria, of course, the air must have contained. A plate exposed in Oxford Street would be covered with colonies, while a plate exposed on Wandsworth Common would show only a few. This is, of course, only a rough-and-ready method which cannot be used for accurate work, but, nevertheless, it gives us good comparative results.

The lantern slides exhibited on the screen demonstrate to you that the air which we breathe always contains micro-organisms, and that therefore we are always inhaling bacteria. Many organisms are incapable of growing at the temperature of the body ; they require a lower temperature. Such organisms, we may assume, cannot thrive in the body of the warm-blooded animal,

[1] A lecture delivered at the London Institution, by Dr. A. A. Kanthack, Lecturer on Pathology, St. Bartholomew's Hospital.

and are therefore, probably, of little importance so far as we are concerned. Keeping this in view, I have always incubated my plates coated with agar-agar at the body temperature, in order to gain information as to the approximate number of organisms which are likely to find access to our respiratory tract, and which have a chance of thriving there. I have not attempted to separate the aerobic organisms from the anaerobic ones, *i.e.* those which can grow in the presence of oxygen from those which cannot. All I wish to show is that under ordinary conditions of life we must breathe an air which contains bacteria, sometimes many bacteria. These plates do not tell us how many bacteria we inhale in a given time; they simply tell us that the air which we breathe is not sterile.

The bacterial flora of air varies considerably. The lady shopping in Oxford Street will inhale more bacteria than the boy who runs about on Wandsworth Common.

We all expect to find that the air in a railway carriage of the Underground Railway is full of bacteria; but, although very rich in bacteria, it is not so impure as might have been anticipated.

I have prepared a number of plates from the air in my laboratory, exposing them from one to five minutes. Some of them are very full of colonies, others less; and this depends on the number of students that have been at work during that time. The more students the more dust, and therefore the more bacteria.

Wherever many people are congregated the air becomes laden with dust and bacteria. Thus plates exposed in the Surgery of one of our largest general hospitals for three to five minutes are covered with numerous colonies. Compare this with the air in the quadrangle of the same hospital, and you will see the effect of confinement and of crowding together. In the former case we find numerous colonies, while in the latter case—*i.e.* in the quadrangle—the air is much freer from micro-organisms. This is also shown by plates taken from the Apothecary's Shop of the same hospital during a time when the patients collect to obtain their medicines; here the air is laden with bacteria.

If you desire a further example, you will find it in plates prepared in the Smithfield meat-market. After a minute's exposure already they are covered with colonies; and we cannot wonder at this, if we remember how active the life there is, and how much organic matter is carried about.

In foggy or misty weather, when the air is quiet, the number of organisms is greatly reduced. It requires, therefore, but little reflection to recognise that, under ordinary conditions, the air which we breathe contains numerous bacteria: we live in a world which is not sterile, and, therefore, unless there exist special preventive measures, those body cavities which are in direct communication with the outer world must also contain bacteria. The mouth, the alimentary and respiratory tracts, and the pores of our skin are all in direct communication with space outside us, in fact, from the bacteriological point of view they represent simply the outer world.

We may, therefore, expect that the organisms which exist outside, in part at least, also find their way into these body spaces or cavities, even if they were not carried into the mouth with our food, or into the nose by the process of respiration. We cannot possibly prevent the bacteria from entering the mouth, even if we refused any but sterilised food. This is an important point to remember, because it proves the impossibility of excluding bacteria from the digestive tract. Saliva always contains them, often in great numbers; and as saliva is constantly swallowed, they must find their way into the stomach. But to return to the bacteria which we breathe. The air passages, *i.e.* the nose, larynx, trachea, bronchi, and their ramifications, and the alveoli or air spaces of the lungs contain bacteria. In normal respiration the inspired air enters the nose, but the anatomical structure of the latter is such as to act as a bacterial filter, imperfect no doubt, but still capable of retaining from three-fourths to four-fifths of the bacteria of the inspired air. Therefore, although large numbers of bacteria find their way into the nasal cavities, the true mucous membrane of the nose is surprisingly poor in bacteria; and this to some extent is due to filtration, and to the fact that healthy nasal mucus possesses considerable bactericidal or disinfecting power. If, however, the nasal mucous membrane is diseased, and it is frequently diseased in this country, large numbers of organisms may be found. The nose, therefore, is an important bacterial filter, and it follows that breathing through the nose is the best method which the body possesses for the purification of the air which we breathe. When we are forced to breathe through the mouth, bacteria are readily inhaled into the larynx. The latter, as well as the trachea, bronchi, and lungs in man always contain bacteria, because the nose is a very imperfect filter which often gets out of order, and also because the respiratory tract is in direct communication with the outer world. It is stated that the trachea, bronchi, and lungs of animals (rabbits) are almost free from bacteria; it is certainly not so in man.

The air which we breathe contains both organisms which are capable of producing disease, and organisms which are harmless. The latter are far more numerous; still pathogenic organisms, *i.e.* disease-producing organisms, do float about in the air, and may then be inhaled. The organisms which we find in the nose, mouth, larynx, and lung include some undoubtedly pathogenic forms, as for instance the micro-organisms of pneumonia, suppuration, &c. These exist in space around us, and therefore, unless they are destroyed in the respiratory passages, they must find their way into the cavities of the body which are in direct communication with the outer world; and thus we see that virulent organisms may enter the body and remain there without causing any lesions, for although we frequently inhale pathogenic organisms we do not inhale the diseases which they are capable of producing. The bacteria enter the body, but not its tissues; they thrive in the secretions and on the mucous membranes lining the various cavities of the body. But that only means that they are practically still outside the body proper. It is a common error to say that because an organism is found inside some space or cavity of the human body, that therefore it lives in the tissues or in the body. As I said before, all the cavities and spaces in direct communication with the outer world are the outer world, and we may expect in them the same organisms as occur in the outer world. The resistance of our tissues in health, and the absence of predisposing influences prevent the pathogenic organisms present from doing more than leading a harmless or saprophytic existence; but if for some reason or another they actually enter or irritate the tissues, the most serious forms of disease may appear, as for instance pneumonia. Many of us carry the organism of this disease about in our mouths, bronchioles or alveoli, although we remain perfectly healthy; pneumonia, however, frequently appears after a drenching or a chill. The coccus of pneumonia, which lay harmlessly on the mucous membrane, now assumes a virulent character, invades the lung tissues, and in some cases even the circulation. The bacillus of tuberculosis in rare cases has also been found in the nasal mucous membrane of individuals attending on consumptives; it did, however, no harm so long as it was outside the tissues on the mucous membrane, *i.e.* in the outer world whence it had come.

Anyhow, we must recognise that since, under ordinary conditions, we live amongst bacteria and decomposing matter, we must be inhaling large numbers of bacteria into our nose, mouth, larynx, trachea, bronchi and lungs; and that since pathogenic bacteria from time to time occur in dust, these also must find their way into those spaces and tracts. But we need not feel alarmed and insist on a sterile supply of air, because the danger of aerial infection is but slight, and because the survivors amongst the inhaled micro-organisms will remain harmless, unless the system is weakened or rudely disturbed by some interference. It is, however, well to remember that our respiratory passages may, and generally, perhaps, do contain numerous germs capable of producing disease and death, and that these germs may lie dormant there for a long time, ready under provocation to do their worst.

There is one other organism I wish to single out, because I shall have much to say about it subsequently, that is the *Bacterium coli commune*. This organism I have always found in saliva and in sputum, on the tonsils and on the pharynx. It is an ubiquitous organism outside the human body, and therefore occurs in the body spaces and, especially, in the intestines which are continuous with the outer world; and it would be surprising if it did not.

We may here conveniently consider the flora of the mouth and pharynx. Whatever micro-organisms are present in the mouth must have got there from the air or the food. At a particular moment there may be an enormous number of organisms present, but many of them are merely temporary visitors; they either die because they do not find suitable conditions, or they are passed on into the stomach and intestines.

The mouth is not guarded by a filter like the nose, but to a slight extent at least the saliva possesses disinfectant properties. The oral cavity is, however, never free from microbes, and some of them belong to highly pathogenic species, as *e.g.* the coccus of pneumonia, several forms of pus producing micro-organisms; and the diphtheria bacillus has occasionally been detected in the mouth or on the tonsils without there having been any history of direct contact with diphtheria cases.

These organisms may lead a saprophytic existence, and may remain harmless for a long time, till for some reason or another they are awakened to a life of virulent activity. Mr. Stephens and I have frequently found bacilli resembling the diphtheria bacillus in the dust, and though some of them are certainly not true diphtheria bacilli, others must be regarded with suspicion. Personally, after prolonged observations which I have carried on with Mr. Stephens at St. Bartholomew's Hospital, I incline towards the view that the diphtheria bacillus is more widely distributed in space than is generally believed, and that in a harmless or saprophytic condition it may be inhaled and fix itself upon the tonsils.

We have found in the air and on the surface of the body several varieties of bacilli, morphologically identical with the diphtheria bacillus. These are generally called pseudo-diphtheria bacilli. They are widely distributed. Some of them are so different in their biological characters from the diphtheria bacillus that they may be put on one side; others, however, so closely resemble it that they cannot be treated with the same contempt. Some observers feel a peculiar satisfaction in hiding themselves behind the security of the pseudo-diphtheria bacillus, which is an undefined quantity, including many varieties of forms. No one nowadays ventures to define the cholera germ; there are too many varieties of it. We believe that caution is advisable in the diagnosis of the diphtheria bacillus. We have come to the conclusion that when a bacillus is morphologically identical in appearance with the diphtheria bacillus, and in its biological characters closely resembles the conventional type of the diphtheria bacillus, he must be a bold man who ventures to say off-hand that this bacillus is or is not a diphtheria bacillus. We know of no test-tube reaction or animal experiment which will always decide it. We believe that the diphtheria bacillus is found in nature as a saprophyte, and that under special conditions it becomes pathogenic, and then diphtheria results. We see once more, that in the mouth also pathogenic organisms may enjoy a harmless or non-pathogenic existence, until conditions arise which alter their character and render them virulent.

We shall now pass on to a consideration of the bacteria which we eat and drink.

That severe gastric and enteric lesions and derangements, often accompanied by the most severe symptoms, and occasionally followed even by death, are only too frequently the result of consuming unsound food, cannot be questioned; and from the hygienic standpoint we must insist upon the sale of proper and sound food, and upon a careful preparation of food. "Food poisoning" may be due:—

(1) To irritation, the food being good in itself, but indigestible or altogether unsuitable.

(2) To bacterial infection; or

(3) To intoxication with poisons elaborated in the food.

(4) To intoxication by poisons purposely or accidentally added to the food.

Unfortunately, if we except the last cause of "food-poisoning," we have no sure tests which we can readily apply to gain information whether disease lurks in a tempting dish. We generally raise the cry of "death in the pot" after the mischief is done, and as a rule we do not get much further. In a free and easy manner the analyst and the medical officer of health speak of ptomaines and toxines which they generally fail to detect, or it is stated that an appallingly large number of microbes have been found in the fatal dish—and this is often considered sufficient evidence to explain the distressing symptoms which ensued.

The first point which I wish to make clear—or you may say to obscure—is the value of the quantitative bacteriological examination in cases of food-poisoning.

In many reports we read that an unusually large nnmber of bacteria were found, and that amongst these were various forms of the *Bacterium coli commune* or of *Proteus*. Now all such reports are somewhat unsatisfactory, unless we also know more of the circumstances under which the food was prepared or preserved.

First, as to numbers: what do they signify? Because we find 500,000 to a million, or even innumerable micro-organisms in a c.c. of fluid or in a minute particle of solid, can we, therefore, always say, in the absence of other evidence, that as food such articles are unsound, and that such numbers account for the symptoms observed? I think not, because I can quote figures which prove that persons who never suffer or have suffered from food-poisoning habitually ingest enormous quantities of bacteria without any evil accruing therefrom.

(1) *Milk* is constantly consumed by many individuals without harm. Now the best samples of milk that I ever obtained in London contained 250,000 micro-organisms per c.c.; generally we find 1 to 2½ millions per c.c., and if we let it stand at the ordinary temperature of the room these numbers may increase 20 to 1000-fold. Yet such milk is generally harmless, and we are not justified in condemning it on account of the large number of germs present. It is impossible to obtain milk free from bacteria, even if the cows were to be milked in a modern operating theatre, because the ducts in the teats always contain micro-organisms, which are washed into vessels, and there quickly multiply, and during the necessary exposure which must follow, more organisms find access to the milk. Mr. Parfitt has recently made some careful examinations of the bacteria present in London milk in my laboratory, and has found that 1 c.c. of milk contained 1,250,000 microbes, of which 303,000 were capable of growing at the temperature of the human body. I would, therefore, not undertake to condemn milk unconditionally, because 1 c.c. of it contained 500,000 to one million germs, and would hesitate to do so if it contained two, or even twenty, millions. Numbers here are not a true criterion; hundreds and thousands of people consume milk teeming with bacteria. I do not say that there is no danger in milk, for we know that tuberculosis, enteritis, diphtheria, and scarlet or typhoid fever have often been traced to milk; nor do I mean to say that the process of collecting and dealing with the milk could not be improved. These points are beyond our present argument. All I wish to show is that most of us consume habitually a large number of organisms without feeling any the worse for it. I do not recommend a bacterial diet, but I merely state the fact that we consume an enormous quantity of bacteria.

I know very well that milk is a frequent cause of enteritis in children, especially during the hot summer months, and this affection, which destroys the lives of many infants, is undoubtedly frequently due to bacteria present in the milk. Prof. Flügge's experiments have practically settled this point, and we must agree that under certain conditions a considerable accumulation of bacteria in the alimentary tract can hardly be a matter of indifference. It is, however, difficult to say what the limit is, beyond which the ingestion of micro-organisms becomes dangerous; and again it is possible, nay probable, that in many cases, for some reason or another, conditions arise which allow organisms existing in the gut, such as the *B. coli*, to proliferate at a great rate, and thus to produce most serious symptoms and intoxications. It is right and proper to avoid all dangers and risks by collecting and preparing food properly, by cooking it sufficiently, and by consuming no food that has been kept too long; but it is equally right and proper to remember that some articles of food are not only consumed, but also relished, which are known to contain enormous numbers of micro-organisms. We cannot make our lives miserable by refusing all but sterilised food; and I wish to point out to you that some articles of food which we particularly enjoy are teeming with bacteria, and for all that are not to be condemned. The question is, how did the organisms find their way into the food, *i.e.* what are the causes and circumstances of the contamination?

We are everywhere surrounded by danger, so far as bacterial infections are concerned. A slight scratch or a fall on the ground may be the cause of lock-jaw or tetanus. Are we, therefore, to give up all forms of exercise, such as football and bicycling? The friendly services of the bacteria outweigh the injuries which they inflict upon us, and I believe that just as in the world around us they do us many a good turn, so also in the world within us do they assist us. Possibly we could get on without them, but we do not know yet whether we could get on better without them than with them. Let us fight our foes, such as the organisms of typhoid fever and cholera; but this can be done with coolness and common-sense, and insistence on cleanliness and ordinary precautions.

(2) All *cold meat* contains numbers of organisms enough to frighten timid people. These are most numerous on the outside of the meat, but the interior is by no means free from them. I have frequently examined cold meat, and a single platinum loop often carries away innumerable germs from the superficial parts, and yet, as a rule, no ill-results ensue from the consumption of cold meat as ordinarily prepared. It is the custom to declare with disgust the legions of micro-organisms found in potted meat or in cold meat-pies, suspected as the cause of food-poisoning, and yet we find that potted meat and sandwiches bought at the most fashionable restaurants of London possess a flora which almost rivals the most virulent potted meat or veal-pie, and which, if numbers were an absolute test, should prostrate any one partaking of them. An error which is often committed, is that articles of food which bear the stamp of respectability, and which the better class consume, are not examined, so that we remain ignorant as to the numbers of organisms ingested with food acknowledged to be sound. I have recently examined the sandwiches offered for sale at one of the best-known London restaurants, and I find that less than a millionth part of a sandwich examined generally contained innumerable micro-organisms. I have myself eaten four to six sandwiches at that restaurant every day for the last twelve months or so that I spent in town, and live to tell the tale, nor have I ever heard of any one coming to grief from the effects of those sandwiches. Similarly potted meat, bought at the best sources, contains an extremely large number of micro-organisms, both aerobic and anaerobic.

(3) If we fix our attention upon the food of those who care for "good things," we find that *oysters* and cold *game* are also thoroughly impregnated with bacteria, yet, in spite of a few accidents, and in spite of the aspersions cast upon oysters, no one, I think, would venture to declare these articles to be invariably unsound food. Oysters are consumed by thousands of persons without bad effects, and they are often given to debilitated patients. We cannot, therefore, appeal to numbers as an absolute standard of good and bad food. However, we must insist upon this, that the oysters be cultivated and kept under conditions which exclude sewage contamination and filth. The layings should not be subjected to anything approaching risk of infection. Sewage always contains large numbers of *B. coli commune;* therefore, oysters, known to be fattened in sewage-polluted beds, which contain numerous *B. coli commune*, cannot possibly be said to be free from sewage contamination, if they have been properly and ably examined. If we are aware that direct contamination with human excreta has been avoided, we need not be alarmed at the presence of what might appear to be a large number of bacteria in oysters, so long as the latter are fresh. No one, after reading Dr. Thorne Thorne's masterly introduction to the recent Local Government Board Report on oyster culture in relation to disease, can doubt that the oyster may be the cause of disease, and that this danger can be obviated by removing the chances of sewage pollution. The chief danger arises from the possible presence of the typhoid bacillus, or the vibrio of Asiatic cholera. Their presence we must fear, but to restore a little confidence in the abused mollusc, I will quote some of Dr. Thorne Thorne's own words: "Only a few of the layings, fattening beds, or storage ponds round our coast can be regarded as theoretically free from every possible chance of sewage pollution. But, as regards the majority of them, any such polluting matter becomes mixed with so vast a bulk of water that it is difficult to see how the layings can be subjected to anything approaching substantial risk or deleterious influence." Still, as the reports show, there are exceptions to this comforting rule, and this should not be.

(4) What I have said about bacteria in normal food will become still clearer if I quote a few figures obtained by Mr. Stephens, when working in my laboratory on the bacteriology of *ice creams*. It has become the custom of using strong expressions against the ice creams sold by the Italian street vendor. It is indeed disgusting to see the same grimy glass used by a row of dirty boys, it being periodically washed in filthy water and wiped with an equally filthy rag; but in many quarters these ice creams have been condemned on account of their rich flora. Now I may remind ladies fond of ices, that the ices bought at the fashionable confectioners in London, as a rule contain as many bacteria as, if not a larger number than, the Italian's ice creams, on which they would look with disgust, if they regard them at all.

In several samples of street-ices Mr. Stephens found from 2 to 5 millions of bacteria per c.c., while strawberry ice creams bought at well-known West End confectioners at times contained from 10 to 14 million germs per c.c. The average number for the two kinds of ices was 7 millions per c.c.

If street-ices are to be condemned therefore—and there are many reasons why they must be condemned—we are not justified in condemning them on account of the number of bacteria contained in them, for in this respect they are no worse than the best ices sold in the West End of London, which afford great and generally harmless pleasure to many; but we must condemn them on account of the circumstances under which the bacteria have found their way into the Italian ice creams.

I could multiply instances to prove that most of us consume enormous numbers of bacteria. I have examined cakes and many other delicacies, and must come to the conclusion that the better-class people ingest as many, if not more, bacteria than those who from poverty are tempted to procure cheap and stale food. I must, however, content myself with the above statements, which prove the difficulty of deciding from a purely quantitative bacteriological examination of food articles whether, other things being absent, we are justified to express a categorical opinion as to their quality, safety, or nutrient value. In examining drinking water, the number of bacteria present in 1 c.c. is no doubt a measure of the adequacy of the filters, but one may wonder why 100 germs per c.c. should generally be considered the maximum number of bacteria which good potable water is allowed to contain. This or any other number absolutely measures the quality of the filter, but not the safety of the water.

We must now pass on to the second point, *i.e.* the species of micro-organisms present in sound food. Here again we find the results of bacteriological examination often unsatisfactory. We have but few, if any, organisms, so far as our present knowledge reaches, which are absolutely characteristic of unsound food, and which are invariably associated with it. In ordinary food I have always found numerous pathogenic or suspected organisms. The *B. coli commune*, *Proteus* forms, *staphylococci*, *streptococci*, organisms resembling the diphtheria bacillus, they may all be found in food which is considered to be above suspicion as well as in food which, by a process of exclusion, reasonably or unreasonably has laid itself open to doubt as to its integrity. I have frequently examined meat, suspected and unsuspected, and feel convinced of this, that in most cases from a qualitative examination we can only proceed to argue with caution, unless we succeed in separating the bacillus of tuberculosis, of anthrax, or of typhoid fever, the vibrio of Asiatic cholera, or probably one or other bacillus of enteritis. Various forms of *B. coli commune* and various forms of *Proteus* are common in articles of food, and were separated often in numbers in many of the sandwiches and other food articles examined. When they have been found in suspected articles in large numbers, they have frequently been considered as being adequately confirmatory of the suspicions aroused by the circumstances of the case. They are certainly evidence of staleness, and may become condemnatory evidence. For instance, water rich in *B. coli* derived from a river into which sewage flows, or into which excreta are drained, must be condemned, because this is clear evidence of sewage or fæcal contamination and of incomplete filtration. It has been thought that food containing large numbers of the *Proteus vulgaris* cannot be eaten with impunity. That is true in some and it may be in many cases, and is a good *post hoc* argument, but it cannot be made an absolute standard. In many sandwiches examined I have found large numbers of *Proteus*, and yet they proved harmless. This organism *per se* does not justify a verdict against the food. In milk the *Proteus* is extremely common, in most samples, at least, examined and consumed by myself without bad results. Now a writer in the *British Medical Journal* of 1895, having made the same observation, argues thus:—"Forms of *Proteus* are found in putrefying organic matter of all descriptions, and their distribution is wide. Their presence in milk must mean one of two things, either direct contamination with putrefying matter, or needless exposure to an atmosphere containing particles of decomposing matter." Thus this observer writes. *Proteus* is so common in food, because it is found everywhere in dust, and bacteriologically speaking all dust-laden air must contain particles of decomposing matter; hence the presence of *Proteus* may not mean more than ordinary exposure, not even needless exposure. It is difficult to see how one is to avoid the *Proteus*. Matter decomposes because the putrefying germs are present everywhere in dust, and putrefied matter rising as dust increases the stock of such germs in the air; the vicious circle is at once established. It is difficult to say what number of *Proteus* bacilli in an otherwise sound article of food would signify danger, and the value of such statement depends on the experience of the observer and on the circumstances of the case. But as far as mere qualitative evidence goes, viewed from the bacteriological standpoint, we all live amongst particles of decomposing matter. Should we get almost pure cultures of *Proteus* or *B. coli* and these forms present in large numbers, matters are easy enough—such food must be condemned.

Let us now turn to the *Bacterium coli*. Water has been condemned because it contained this organism in small or large numbers, and the writer quoted above asserted that "the colon bacillus, if found in potable water, is usually taken as diagnostic of sewage contamination," and "its frequent presence in milk he derives from the soiled cow and its surroundings, and he regards it as *par excellence* diagnostic of fæcal contamination." Many writers believe that water which contains this microbe at all, in however small numbers, has in all probability been polluted with excremental matter, and they regard this bacillus by itself as typical and specific of fæcal matter.

Now I consider that the *B. coli*, in its various forms, is a rather abused organism. With Mr. Stephens I have worked at this organism since the beginning of 1894, and have separated it wherever I came across it. It is our experience that it occurs in some form or another almost anywhere and everywhere: in the air, in the soil, in the water, in dust, of course in varying and variable quantities. We have found it in the secretions of the body where direct intestinal contamination could be excluded. It occurs in normal saliva, in expectoration, whether of health or disease, not occasionally, but practically always. In diphtheritic membranes, in abscesses, in the skin—everywhere it is. Fluids and solids exposed to air contain it; nothing can avoid it. On the surface of meat, even frozen mutton, on bread, fruit—everywhere it may be found. A few hours after birth it has been found in the intestinal tract of infants. We, therefore, have come to the conclusion that the *B. coli* is present in the intestines, because it is ubiquitous outside the animal body, and not that its presence anywhere and in any number outside the digestive tract necessarily signifies direct filth contamination. No doubt animal excreta assist in keeping up its supply, but it seems to me that simply because this bacterium occurs somewhere or other, to speak of direct filth contamination without other existing evidence is not quite logical. Of course it may be said that since filth forms a great source of this bacterium, and since the alimentary tracts, so to speak, envelops the earth, that therefore the presence of the *B. coli commune* does prove such contamination; but then it comes to this, that from a bacteriological standpoint we live and breathe in decomposing matter. I therefore doubt whether we have any right to condemn apparently sound water or food which contains the *B. coli commune* on the assumption that it has been soiled unless we have real or circumstantial evidence of such soiling. If water contains a large number of *B. coli commune*, we may have to condemn it on the score of being insufficiently filtered if the source of such drinking-water contains the *B. coli commune*; but in the absence of other evidence we cannot always do that. The bacillus fluorescence is almost constantly found wherever the *B. coli* occurs, yet no one would regard its presence in water, even in large numbers, as absolute evidence of direct filth contamination Yet the above observer concluded that the presence of the bacillus fluorescence in milk may be taken as presumptive evidence of added water: this is a *reductio ad absurdum*. Time forbids to say any more about the *B. coli commune*, and I am not here to discuss it.

I know of few organisms which are so indifferent with regard to the medium on or in which they are grown; aerobiosis or anaerobiosis, high or low temperatures, acid or alkaline reactions, light or darkness do not affect the *Bacteria coli* group to any marked degree. At the same time they are chemically extremely active organisms, and therefore their normal presence in the alimentary tract can hardly be of physiological indifference to us and animals. I incline to the belief that their influence in disease is secondary rather than primary, but I shall not discuss this point here.

Now, when we are dealing with organisms which are capable of growing and acting on dead as well as on living tissues, which are furthermore capable of great and varied chemical activity, and also resist external influences extremely well, and readily vary under such influences, it seems to me that an important point may be raised. If there be anything in adaptation, then I think the animal body must have adapted itself as much to the *Bacteria coli* group, as the latter, no doubt, has adapted itself to the animal body. The two organisms, viz. the *Bacterium coli* and the animal body must be well balanced; if the balance is disturbed, one of them must go down. Symbiosis, whether obligatory or facultative, is a problem which has hardly been touched upon as existing between man and low forms of germs. We know that plants make use of micro-organisms, and that vegetation is immensely assisted by nitrifying organisms. Under absolutely sterile conditions of growth a plant thrives badly. Experiments and observations are needed to show how animals would thrive on sterile food and in sterile surroundings. Pasteur, in 1885, expressed the opinion that they would do badly under such conditions. Nuttall and Thierfelder have shown that a guinea-pig brought aseptically into this world may be kept in good condition under sterile surroundings for 8 to 14 days, and from their experiments they argue that the presence of bacteria in the intestinal tract is not necessary to life. The obvious criticism is that a week or two for such experiments is too short a period, and that it would require observations carried on for months before it could be definitely stated whether or no bacteria are necessary not for life but for perfect development.

Fermi seems to incline towards a belief in a form of symbiosis existing between the *B. coli* group and the intestinal mucosa. There are observations which tend to show that lower forms of animal life do not grow or thrive well on sterile food. It seems possible from studying Neumeister's work on physiological chemistry, that some forms of bacteria—not necessarily the *B. coli commune*—are of use in assisting fermentative processes, and in aiding in the resorption and absorption of products of digestion, and it is certain that as putrefactive organisms they do good. Nor is it impossible that they are capable of splitting up certain toxic substances, thus rendering them harmless. The question of adaptation of the body to bacteria is well worthy of extended study; but while it is still a matter of speculation, it is safer to dismiss it with this brief allusion.

Taking a summary review of the points mentioned, we have seen that under ordinary conditions sound food often contains large quantities of bacteria, so that we habitually consume numberless micro-organisms. Further, the qualitative examination shows that we are habitually consuming such forms as the *B. coli commune* and *Proteus*. It is well that we should know the flora of apparently good food, and become familiarised with the idea, alarming to many, no doubt, that many articles of food daily consumed contain bacteria, some of which are described by bacteriologists as the typical organisms of the intestinal contents and of decomposing matter.

I do not wish to be misunderstood. I am not advocating the view that good food should be particularly rich in bacteria. All possible chance of direct fæcal, sewage, or other contamination should be, and must be, carefully avoided. On the other hand, we must not introduce a fictitious standard, and simply put on one side physiological facts and common-sense experience. Similarly we must pause before we give certain bacteria an absolutely specific significance which they possibly do not deserve.

The *B. coli commune* by itself does not prove sewage or fæcal pollution; it may and often does point to it, under certain conditions which Dr. Klein has recently defined, but it cannot unconditionally prove it. Again, its importance as the cause of enteritis must not be exaggerated. Thousands of *coli* bacilli are periodically taken in with the food, and they pass into an alimentary canal already full of these bacilli. This is a point worthy of consideration. True in an enteritis the *B. coli* may be found in pure culture in the dejecta, possessed of virulent properties when tested on the animal. This merely proves one of two things: if the *Bacterium coli* is the cause of the lesion then for some reason or another it must have been transformed from a harmless saprophyte into an irritant pathogenic organism; but it has not yet been shown that this organism is the cause of such a lesion, and therefore its abundant presence in such a lesion may be merely a concomitant phenomenon. The exact position of this extraordinary organism, or rather group of organisms, has not yet been exactly defined. Many observers may not agree with me; my own opinions are, however, based upon my personal acquaintance with the *Bacterium coli* and its varieties. It is of value in water or food examination, not because it is absolutely specific of bad or polluted food, but because it is easily recognised, and therefore its source can often be traced. Food or water exposed to the danger of sewage or fæcal contamination, as for instance Thames water, containing a certain percentage of *B. coli commune*, cannot be said to be freed from all pollution. Further we cannot go.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE Head Masters' Conference at Rugby last week passed the following resolution unanimously:—"That the Conference deplores the time wasted in teaching boys the present system of weights and measures, and would welcome the introduction of a more rational system."

AMONG the French Universities which, in accordance with the new Act, will at the end of next year be permitted to dispose of the money accruing from students' fees, &c., the richest will be that of Lyons, which will thus have an annual income of over 5000*l*. According to the Paris correspondent of the *Chemist and Druggist*, schemes are already on foot for important educational extension in the capital of the centre. The Lyons Faculty of Medicine and Pharmacy, as well as the Faculty of Sciences, will probably be enlarged, and new laboratories built or the present ones extended. But the most important project is the construction of a Chemical Institute, in which will be reunited the various chemistry services of the two above-named faculties; and the already flourishing School of Commercial and Agricultural Chemistry will be also installed in this new building. The erection of this Institute will, it is hoped, be commenced next spring, and the cost is calculated at 60,000*l*., but the municipality will give the site, valued at 16,000*l*., and possibly other aid. The department has voted 2000*l*., and the State, it is hoped, will contribute over 25,000*l*. The head of the University states that, while laboratory research will not be neglected, the University will seek "to incorporate itself more and more with the industrial city" and "develop the technical instruction that may serve the commercial and manufacturing interests of a great city of half a million souls."

SCIENTIFIC SERIALS.

Wiedemann's Annalen der Physik und Chemie, No. 12.—Directed electric surface conductivity, E. Braun. Continuous transition of electric properties in the separating surface of solid and liquid bodies.—Conduction of electrified air. Magnetic currents. By the same author. Crystals have a different electric conductivity in different directions. This was proved by Wiedemann by dusting lycopodium powder on a cleavage surface and sending a spark from a wire into it. An elliptic area was cleared, with axes in a ratio varying from 1 : 2 to 1 : 3. The author obtained different surface conductivities with a steady current, through a layer of condensed moisture. The effect vanished when the thickness of the layer exceeded 50 $\mu\mu$. A steady transition from the polarisation properties of a solid to those of a fluid body may be traced in the surface layer.—Polarisation phenomena in vacuum tubes, by C. A. Mebius. When the current through a gas increases, the rate of fall of potential at the kathode increases more rapidly than at the anode. When secondary terminals are introduced, so as to give a transverse current, the rate of fall of potential at these decreases with an increase of the main current, when a current of a certain strength is sent through the secondary terminals.—On the transition of carbon from the non-conducting to the conducting condition, by G. Brion. The conductivity of carbon depends upon the highest temperature to which it is exposed, the time since elapsed, and the present temperature. It is acquired very rapidly at temperatures between 800° and 1000° C. The conductivity decreases rapidly during the first few hours after heating, and then more slowly.—On electro-capillary light, by O. Schott. On sending the sparks from an induction coil through a capillary tube 0·05 mm. in diameter containing air at ordinary pressure, an intense light is observed in the tube. The latter soon gets roughened and blown out into spherical bulbs. Wider tubes gave a less intense light, and are less altered. Electrodes of various metals may be used with the same effect. In the spectroscope, the light shows a continuous spectrum crossed by bright lines, and dark lines along the spectrum which shift their position at every discharge.—Glow-worm light, by H. Muraoka. Natural glow-worm light behaves like ordinary light. But when it is filtered through cardboard or through copper plates, it shows the properties of X-rays or Becquerel's fluorescence rays. The intensity of the action of the glow-worm rays is intensified by the presence of the cardboard near the sensitive plate. They may be reflected, and probably also refracted and polarised. The author operated with 300 glow-worms at Kyoto, Japan, during the month of June, when thousands of them swarm about the neighbourhood.—An attempt to demonstrate the existence of electrodynamic solar radiation, by J. Wilsing and J. Scheiner. Owing to the absorption of the longer waves by the atmosphere, and the consequent necessity for an instrument of extreme delicacy, the change of contact resistance between two metals was used as a test. But no positive results were obtained.

Bollettino della Società Sismologica Italiana, vol. ii., 1896, N. 4.—Recent observations and results on the form and mode of propagation of seismic waves, by Dr. A. Cancani.—The seismic data of Liguria, with reference to their frequency and periodicity, by Dr. E. Oddone.—On the after-shocks of the great Japanese earthquake of 1854, by Prof. F. Omori.—Notices of earthquakes occurring in Italy during the year 1896, by Prof. L. Palazzo. This catalogue, a continuation of that formerly compiled by Dr. M. Baratta for the Central Meteorological and Geodynamic Office at Rome, contains accounts of all the shocks recorded from January 1 to June 14 The more important are those of Polesina on March 8, near Florence on April 15, in Asia Minor on April 16, and several earthquakes of unknown and distant origin on March 4, April 10, and May 2, 3, and 5.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 10.—"On Prof. Hermann's Theory of the Capillary Electrometer." By George J. Burch, M.A.

In reply to the claim of Hermann (*Archiv für die Ges. Physiologie*, vol. lxiii. p. 440), that his theory of the capillary electrometer received confirmation from the author's experimental results, the author stated that his own theory was completed before he saw Hermann's paper, that it was based upon a totally different hypothesis, and that the identity of Hermann's equation with his own is due simply to the fact that both are the mathematical expression of a movement which is dead beat. Hermann, adopting Lippmann's polarisation theory, had assumed the simplest conceivable relation between the rate of polarisation and the acting P.D., namely, that they are proportional to one another. The author's starting point was the fundamental fact that in the capillary electrometer a mechanical effect is produced by an electrical cause. Writing Q for the quantity of electricity, C for the constant of capillarity, P for polarisation, and W for the work done, the symbolical expression of the problem is—

$$f(Q_t, C_t, P_t) = \phi(W_t).$$

Hermann has passed over C and omitted to take W into account, confining himself to the theoretical relation between Q_t and P_t. But the term polarisation includes two phenomena, viz.:—

(*a*) That condition of the interface between two conductors, of which one at least is an electrolyte, in which the molecules are under a stress not greater than they are capable of supporting without chemical change.

(*b*) A deposit upon the surface of a solid, or in the contiguous liquid, of the products of actual electrolysis.

If one of the conductors is a solid, the inevitable local differences of condition or of composition enable actual electrolysis to take place even with a P.D. smaller than that proper to the chemical change implied. But if both conductors are liquid and perfectly pure, the stress is so far equalised that no electrolysis is possible until the E.M.F. reaches a certain value, more sharply defined in proportion as the materials are pure. The author holds that with differences of potential which do not reach this limit, the electromotive force is transmuted, without electrolysis, into mechanical force, and manifests itself as kinetic energy, until by the motion of the meniscus it becomes transformed into potential energy. The locus of transformation from electrical to mechanical force must clearly be the two interfaces mercury-acid and acid-mercury, and it is upon these that the stress acts. The resistance is distributed along the tube, and is partly electrical, but to a far larger extent mechanical. It does not seem reasonable, therefore, to assume that the sole cause of delay is the "polarisationsgeschwindigkeit" of the meniscus.

The author believes that in the case of an interface between two liquids, the rate of polarisation is to be measured in terms of the vibration period of a molecule, rather than in decimals of a second.

Actual electrolysis does not take place in a properly working electrometer, except with electromotive forces greater than ought to be employed.

According to the author, the capillary electrometer acts by transforming electrical into mechanical energy without any chemical interchange, this being possible because at the interface between two liquids which do not diffuse into each other the stress is so evenly distributed that no one molecule can be strained to a degree sufficient to detach any part of it until the stress is intense enough to break down all similar molecules simultaneously.

In order to investigate the motion of the meniscus under the action of a varying electromotive force, such as a pulsating or alternating current, Hermann puts his equation into the form

$$\frac{dp}{dt} + rp - r\epsilon f(t) = 0,$$

which is identical with the author's formula for the estimation of the E.M.F., viz.—

$$N + k\Delta r = \frac{1}{0\cdot0133} \times f(t) \text{ volt},$$

i.e.—

$$\left\{\begin{matrix}\text{The sub-}\\\text{normal}\\\text{to the curve}\end{matrix}\right\} + \left\{\begin{matrix}\text{A constant}\\\text{multiple of the}\\\text{distance from}\\\text{the zero line}\end{matrix}\right\} = \left\{\begin{matrix}\text{A constant}\\\text{multiple of}\end{matrix}\right\} \cdot \left\{\begin{matrix}\text{The}\\\text{E.M.F.}\\\text{at time } t\\\text{in volts.}\end{matrix}\right\}$$

From this formula it is evident at once that the E.M.F. is zero whenever $N = -k\Delta r$, and that the crossing of the zero line by the mensicus must always lag behind the change of sign of the E.M.F. Hence the curve can never come back to the zero line under the action of a current which pulsates but does not alternate. When N, *i.e.* $\frac{dp}{dt}$ vanishes, as it does at the apex of a spike or the bottom of a notch, the instantaneous value of the impressed E.M.F. is directly proportional to the distance of the meniscus from zero. The author has proposed that this method should be used to determine the characteristic current curves of dynamos.[1]

Royal Meteorological Society, December 16.—Mr. E. Mawley, President, in the chair.—An interesting paper, by Dr. Leigh Canney, on the winter climate of Egypt, was read by the Secretary. The climate of Egypt during the winter is influenced by the Libyan desert, by the Mediterranean Sea, and by the extent of cultivated land. The author gave the results of a series of observations which he had carried on during the past three winters. The observations were started with the object of arriving at a comparative knowledge respecting the climates of the various stations now considered as health resorts in Egypt, and by a strictly comparable method to arrive at the precise differences between the climates of Upper and Lower Egypt, all previous observations having failed in this respect. The stations at which observations were made were Cairo, Helouan, Mena House Hotel, Luxor, Assouan, Valley of the Tombs of the Kings, and the crest of the Libyan Hills. As self-recording thermometers and hair hygrometers were used at each station, valuable data have been obtained on the diurnal variation of temperature and humidity.—Mr. R. H. Curtis also read a paper on an attempt to determine the velocity equivalents of wind forces estimated by Beaufort's scale. The author has compared the anemometric records at Scilly, Fleetwood, Yarmouth, and Holyhead, with the wind forces as estimated by the observers at the same or adjoining stations, and has by this method obtained a satisfactory table of velocity equivalents in miles per hour for the estimated forces by Beaufort's scale.

[1] "The Capillary Electrometer in Theory and Practice." (Reprinted from the *Electrician* of July 17 *et seq.*, 1896.)

Geological Society, December 16.—Dr. Henry Hicks, F.R.S., President, in the chair.—On the subdivisions of the carboniferous series in Great Britain, and the true position of the beds mapped as the Yoredale series, by Dr. Wheelton Hind. In this paper the author gave a summary of the knowledge of the local division of the Carboniferous system, and criticised the present classifications in vogue, laying special stress upon the local variations in the lithological characters of the rocks, and summing up to a large extent the fossil evidence which is available. He maintained that the Yoredale beds were largely the equivalents of the beds which had elsewhere been referred to the Mountain Limestone series, though some local beds which had been included in the Yoredale series might rather be the equivalents of the millstone grit. He would divide the rocks of the Carboniferous system into an Upper Carboniferous or Anthraciferous series, and a Lower Carboniferous or Calcareous series; and indicated the occurrence of three very different faunas in the Carboniferous rocks, viz.: (1) a Coal-Measure fauna rich in fish remains; (2) the Lower Coal-Measure and Grit fauna, largely marine but littoral; and (3) a Limestone fauna, essentially marine, very rich in brachiopods.—Note on volcanic bombs in the Schalsteins of Nassau, by Prof. E. Kayser. The bombs forming the subject of this communication occurred in two localities in the neighbourhood of Oberscheld near Dillenburg. They were generally rounded, though sometimes angular, and varied in size from that of a nut to that of a man's head. Each consisted of a kernel of coarse-grained rock representing a fragment of limestone altered by metamorphism, surrounded by a rind of amygdaloidal rock due to the inclusion of the fragment in molten lava. They demonstrated the pyroclastic origin of the Schalsteins, and also proved the similarity between the old Devonian volcanoes and those which were now active.

MANCHESTER.

Literary and Philosophical Society, December 15.—Prof. H. B. Dixon, F.R.S., in the chair.—Mr. J. C. Melvill read a paper on a collection of marine mollusca, mostly dredged by Mr. F. W. Townsend, who is officially connected with the Indian Oceanic Telegraph Company, whose cable extends from Kurachi to Bushire in the Persian Gulf. Mr. Townsend has made good use of the exceptional facilities he possesses for thus dredging the marine fauna, some forms being obtained attached to the cable itself, and some dredged in shallow, others in deep, water. The collections are of first-rate importance to the malacologist, the more so as this part of the North Indian Ocean has been curiously neglected in the past; the results have not yet been fully examined, but no less than thirty-two forms were described and differentiated as new to science. Of these the principal belonged to the genera *Nassa*, *Sistrum*, *Terebra* (one extraordinary form), *Mitra* (also very unusual in appearance), *Turritella*, *Coralliophila*, many *Trochi*, *Dentalium*, and, amongst Pelecypoda, *Yoldia* (a particularly interesting tropical form of an Arctic genus), *Pectunculus*, *Tellina*, *Donax*, *Chione*, and others.—On the ampullæ on specimens of *Millepora* in the Manchester Museum, by Prof. S. J. Hickson, F.R.S. The author stated that he had discovered ampullæ on several specimens in the museum. At least one of these belongs to the West Indian species *M. alcicornis*, and the observation suggests that all species of *Millepora* at some time produce medusæ similar to those of the Pacific Ocean species *M. murrayi*.

PARIS.

Academy of Sciences, December 21.—Annual Meeting.—M. A. Cornu in the chair.—The President's address was chiefly occupied with an historical retrospect of the X-rays. The losses by death during the year include the names of Fizeau, Tisserand, Reiset, Sappey, Daubrée, Resal, and Trécul.—The Arago medal has been awarded twice during the year, to M. A. Abbadie and to Lord Kelvin. The prizes offered for the present year have been awarded as follows:—In the section of Mathematics, the grand prize to M. E. Maillet for his work on the theory of substitutions, the Bordin prize to M. Jacques Hadamard for his memoir on the theory of geodesic lines, the Francœur prize to M. A. Valson, and the Poncelet prize to M. Painlevé for general contributions to mathematics. In the section of Mechanics, the extraordinary prize of 6000 fr. is divided between MM. Darrieus (for a work on the improvement of the naval forces), Baule (for the application of the gyroscope to the determination of the altitudes of stars at sea), Schwerer, Blot, Monaque, Morache, Paqué, Terrier, and de Vanssay (for their magnetic observations). M. Henry Parenty receives a Montyon prize for his experimental researches on the theory of fluids, and M. Marbec the Plumey prize for his memoir on some applications of graphical mechanics. In Astronomy, the Lalande prize is given to M. P. Puiseux (for his selenographical work), the Valz prize to M. Bossert (for his reduction of older observations previously inaccessible), and the Janssen prize to M. Deslandres (for his studies in spectroscopy). No memoirs were received for the Damoiseau prize. In Statistics, a Montyon prize is taken by M. Huguet (for his study of the statistics of voluntary mutilation and assumed diseases in the army), and another by the *Comité des Compagnies d'Assurances a prime fixes sur la vie* (for a report on the tables of mortality used by them). In Chemistry, the Jecker prize is divided between MM. Matignon (for his thermochemical studies), V. Auger (for his researches on the chlorides of dibasic acids), M. Bouveault (for researches on the nitriles), and M. Genvresse (for work in organic chemistry). In the section of Mineralogy and Geology a Vaillant prize, which was offered for an experimental study of the physical and chemical causes which determine the existence of rotatory power in transparent substances, is awarded to M. Ph. A. Guye, and another, for which the subject of the theoretical and practical improvement in the methods of geodesy or topography was suggested, to M. C. Lallemand, for his contributions to the methods of levelling; whilst the Fontannes prize is given to M. Douvillé for his stratigraphical and palæontological researches. In the Botanical section M. E. Bescherelle receives the Desmazières prize, and M. Flagey a Montagne prize. In the section of Anatomy and Zoology, the Thore prize is taken by M. C. Janet, the Savigny prize not being awarded. In Medicine and Surgery, Montyon prizes are taken by M. Laskowski, for an anatomical atlas, by M. Legrain, for his studies on alcoholism, by MM. Imbert and Bertin-Sans, and MM. Oudin and Barthélemy, for their applications of the Röntgen rays in surgery and medicine. Honourable mentions are accorded to MM. Comby, Brocq and Jacquet, Broca and Maubrac. The Barbier prize is divided between MM. Bertrand and Fontan, and M. Raynaud; the Breant prize between M. Rénon and MM. Netter and Thoinot. The Godard prize is awarded to M. Max Melchior for his contribution on urinary infection, M. P. Delbet receiving an honourable mention. The Serres prize is divided equally between MM. Mathias Duval and Alfred Giard for their embryological studies, whilst M. Brun receives the Bellion prize, an honourable mention being accorded to M. Bodin. The Mège prize is awarded to M. Mauclaire, the Lallemand prize to M. R. Dubois, and the Baron Larrey prize to M. Edm. Delorme. In Physiology, M. Contejean receives a Montyon prize for his work on gastric digestion, Dr. Joachimsthal the Pourat prize for his experimental work on the locomotive apparatus, and M. Tissot, the Philipeaux prize for his study of the gaseous exchanges in isolated muscle. In the section of Geographical Physics, M. André Delebecque receives the Gay prize for his study of the French lakes. Of the general prizes, a Montyon prize is awarded to M. E. Cacheux for his construction of healthy workmen's dwellings, and other labours in connection with the improvements in the health and conditions of life of the working classes; the Trémont prize to M. Charles Frémont for his experiments on the effects of punching on metals; the Gegner prize to M. Paul Serret; the Delalande-Guérineau prize to M. Toutée for the scientific results of his expedition on the Niger; the Jean Reynaud prize to M. Henri Poincaré; the Jérome Ponti prize to MM. Benoit, Chapuis, and Guillaume, for the metrological work done in the International Metric Bureau at Breteuil; the Leconte prize (arrears) to M. Roussel, for his geological work in the Pyrenees, and to M. Henneguy for his researches on the development of the bony fishes; the Tchihatchef prize to Prince Henri d'Orleans, for his geographical work in Central Asia; the Houllevigue prize to M. Joannis, for his chemical researches; the Cahours prize to MM. Freundler, Lebeau, Hébert, and Varet; the Saintour prize to M. Renault, for his memoirs on vegetable palæontology and fossil bacteria, and to M. Guntz for his researches on fluorides, the alkali metals, and absorption of hydrogen and nitrogen by lithium; the prize founded by Mme. la Marquise de Laplace, to M. de Nanteuil de la Morville; and the prize founded by M. Félix Rivot, to MM. de Nanteuil de la Morville, Dutilleul, Balling, and Leroux.

DIARY OF SOCIETIES.

THURSDAY, DECEMBER 31.

ROYAL INSTITUTION, at 3.—Visible and Invisible Light: Prof. S. P. Thompson, F.R.S.

FRIDAY, JANUARY 1.

GEOLOGISTS' ASSOCIATION, at 8.—An Outline of the Petrology and Physical History of the Alps: Prof. T. G. Bonney, F.R.S.

SATURDAY, JANUARY 2.

ROYAL INSTITUTION, at 3.—Visible and Invisible Light: Prof. S. P. Thompson, F.R.S.

SUNDAY, JANUARY 3.

SUNDAY LECTURE SOCIETY, at 4.—Röntgen or X-Rays: Richard Kerr.

MONDAY, JANUARY 4.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—An Expedition to the Barotse Country: Captain A. S. Gibbons, Percy C. Reid, and Captain Alfred Bertrand.

SOCIETY OF CHEMICAL INDUSTRY, at 8.—The Smelting and Refining of Cyanide Bullion: Arthur Caldecott.—The Industrial Use of a Recording Pyrometer: Prof. Roberts-Austen, C.B., F.R.S.

VICTORIA INSTITUTE, at 4.30.—Paper by Dr. F. A. Walker.

TUESDAY, JANUARY 5.

ROYAL INSTITUTION, at 3.—Visible and Invisible Light: Prof. S. P. Thompson, F.R.S.

WEDNESDAY, JANUARY 6.

GEOLOGICAL SOCIETY, at 8.—On the Structure of the Skull in a Pliosaur: C. W. Andrews.—On the Pembroke Earthquakes of August 1892 and November 1893: Dr. C. Davison.—Changes of Level in the Bermuda Islands: Prof. R. S. Tarr.

THURSDAY, JANUARY 7.

ROYAL INSTITUTION, at 3.—Visible and Invisible Light: Prof. S. P. Thompson, F.R.S.

FRIDAY, JANUARY 8.

ROYAL ASTRONOMICAL SOCIETY, at 8.

SATURDAY, JANUARY 9.

ROYAL INSTITUTION, at 3.—Visible and Invisible Light: Prof. S. P. Thompson, F.R.S.

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